WAYS OF KNOWING ABOUT WEAPONS: THE COLD WAR'S END AT THE LOS ALAMOS NATIONAL LABORATORY

BY

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DISSERTATION

Submitted in Partial Fulfillment of the
Requirements for the Degree of

Doctor of Philosophy
Anthropology

The University of New Mexico
Albuquerque, New Mexico

May, 2001

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ABSTRACT

This dissertation addresses questions of knowledge, identity, scientific activity and social reproduction among nuclear weapons experts at the Los Alamos National Laboratory. Throughout the Cold War, the laboratory's weapons community produced an enormous body of knowledge about nuclear weapons through engaging iteratively in an experimental cycle that consisted of designing, building and testing experimental nuclear explosives. This design and test cycle also fulfilled critical social functions, providing a site for the reproduction of skills and understandings in novice weaponeers as well as an engine for the ongoing integration of the many "ways of knowing" that existed in the laboratory. However, Congressional legislation halted the design and test cycle in 1992, and since that time, the laboratory has neither designed new nuclear devices nor tested any existing ones. Without the design and test cycle, senior weapons experts

frequently express concern that the laboratory is on the verge of losing critical skills, understandings and abilities necessary to make judgments about the state of the stockpile.

This dissertation explores the “knowledge loss” problem at Los Alamos, drawing on two and a half years of ethnographic fieldwork, including interviews, participant observation, and archival research conducted among weapons experts between August of 1997 and April of 2000. I explore learning as social process that takes place through engagement with other people, and with various aspects of the physical world, within locally meaningful settings – in this case, a nuclear weapons laboratory. In doing so, I argue that learning must be understood as a process of identity formation through which unknowing, unschooled novices gradually come to understand themselves as contributing, knowing members of a particular community of practice – in this case, nuclear weapons experts. With this comes far more than just a set of weapons-related skills. As people engage in the laboratory’s activities, they come to understand their work as meaningful in relation to larger moral, social, and political bodies of knowledge – about the moral rightness of nuclear deterrence to prevent war, for example, or the social and political position of the United States vis-à-vis other nations. In that sense, the activity settings through which people learn how to move fluently through the world of weaponeering – in security training sessions, in working with high explosive assemblies, as novice weapon designers – are properly understood as the mechanisms through which the larger weapons community and its many interrelated ways of knowing about weapons – technical, moral, social, political – are reproduced.

Anthropologists, you miss a chance
By examining not what makes
A warhead scientist salivate.
Homo Los Alamos! How deservedly unique!
- Don Eduardo de Los Alamos (Edward B. Grothus)
Local resident and antinuclear activist,
Los Alamos, NM

CHAPTER ONE: INTRODUCTION

I did not begin my doctoral fieldwork intending to write an ethnography of nuclear weapons scientists and engineers. When I arrived at the Los Alamos National Laboratory (LANL) in the summer of 1997, I knew a great deal about the town of Los Alamos, a little about the organization, and nearly nothing about nuclear weapons. Like most anthropologists, I came to my field site prepared to study the people in its margins, not at its center: I went to the laboratory to explore the formation of social networks among women and minority scientists, to see how they built supportive, career enhancing mentoring relationships in the traditionally white, masculine domains of physics and engineering, in a weapons laboratory whose management structure was replete with men, not women. However, after several months at my field site I found a more compelling topic: the fear, widely shared among many staff members, that crucial skills and understandings, local "ways of knowing" about nuclear weapons, might be disappearing.

By the time I began my fieldwork, knowledge loss had been a major concern at Los Alamos since the late 1980s, when political trends at the end of the Cold War began to impact the laboratory's research environment. Throughout the Cold War, Los Alamos was the nation's flagship nuclear weapons research and development laboratory, one of three such facilities owned by the United States Department of Energy (DOE). Los

Alamos and its sister design laboratory, Lawrence Livermore National Laboratory in California, have historically been responsible for designing and certifying prototype nuclear explosives for the United States' nuclear stockpile. Staff at Sandia National Laboratory in Albuquerque have acted as an engineering bridge between Livermore and Los Alamos and the Department of Defense to ensure that the DOE's nuclear explosives are properly fitted to the DOD's missiles and bombs. Throughout the Cold War, the three laboratories designed, built and tested nuclear explosives in support of the nation's nuclear deterrent, certifying that weapons in the stockpile were safe to handle, secure from terrorist detonation, and would work reliably when required to do so.

For nearly fifty years, weapons experts at Los Alamos fulfilled this weapons research and development mission through an iterative experimental process that consisted of conceptualizing prototype nuclear explosives, refining these concepts through computer simulations and high-explosive experiments, and testing the prototype at the United States nuclear proving ground, the Nevada Test Site (NTS). Between 1945 and 1992, the weapons community at Los Alamos engaged in hundreds of iterations of this "design and test cycle" in the course of developing and certifying nuclear explosives for insertion in bombs and missiles in the American nuclear stockpile. Testing drove the ongoing expansion of the laboratory's scientific and engineering knowledge base, providing a foundation of expertise for policy decisions in fields from strategic defense to arms control. In addition, the experimental process provided a site for novice weaponeers to engage in the specific test-related problems and activities, so they could gain expertise in the arcana of nuclear weapons research.

Mission Crisis

However, between 1988 and 1993, a rapid series of political put the laboratory's weapons mission in jeopardy. The crisis began in the late 1980s, when revelations of environmental mismanagement at DOE materials production and processing facilities eroded public support for continued investment in nuclear weapons (Fehner and Holl 1994, Bartimus and McCartney 1991: 192-195). At the same time, the Soviet Union was rapidly dissolving, leaving the United States unchallenged as a military superpower. In combination with a politically powerful antinuclear movement in the United States and Europe, these trends put both the Bush administration and Congress under intense domestic pressure to cut military spending and end the arms race. After declaring an end to the Cold War in November of 1990, President Bush pursued a series of unilateral arms withdrawals as well as the Strategic Arms Reduction (START I and II) negotiations, which called for bilateral reductions in strategic weapons. In addition, he cancelled several new orders for new nuclear weapons systems, effectively placing a moratorium on design activities at Los Alamos.

Spurred by the rapid deflation of the arms race, Congress soon followed with the bipartisan Hatfield-Exon-Mitchell amendment (or "Hatfield amendment") to the fiscal 1993 Energy and Water Appropriations Act, the legislation that provides funding to the Department of Energy. Named for its sponsoring senators, the Hatfield amendment required a temporary nine-month moratorium on all nuclear tests and called for the President to pursue negotiations towards a CTBT by 1996. Within a few short months, then, scientists and engineers at Los Alamos found their expensive experiments quite literally suspended, left hanging above the dry desert floor of the NTS as funding for the testing program evaporated.

If the United States had permanently renounced nuclear weapons in the early 1990s, the test moratorium might well have marked the end of the laboratory's weapons mission. Indeed, by the time the Clinton administration came into office, laboratory officials were quite concerned about the institution's future, given the Clinton administration's strong opposition to nuclear testing and its definition of national security as a matter of economic competitiveness rather than military might. Anticipating more changes in defense policy, laboratory managers developed an aggressive post-Cold War mission marketing strategy, identifying a swath of politically popular social issues – including environmental degradation, energy, economic competitiveness, drugs and terrorism, and health care – and targeting laboratory capabilities towards them.

With the laboratory facing an uncertain future, morale at Los Alamos fell quickly. Members of the weapons community questioned the United States' commitment to maintaining a nuclear deterrent, while employees throughout the laboratory expressed concern about job security. Their concerns were confirmed when laboratory managers reacted to potential cuts in Los Alamos' budget and mission by restructuring the workforce. Between 1993 and 1995, Los Alamos went through a series of workforce reduction initiatives, beginning in October of 1993 with a voluntary employee retirement incentive program, or VERIP, which cut the laboratory's workforce by roughly fifteen hundred employees. In September of 1995, a second workforce reduction program, which consisted of a voluntary separation program followed by layoffs, cut another 1500 or so positions from the laboratory's workforce. In less than two years, Los Alamos had reduced the total size of its workforce from a high of roughly 15,600 employees to less than 12,500. Morale, not surprisingly, reached a nadir. One prominent geophysicist, describing life at the laboratory in the early 1990s, said, “The whole place was in free-fall. You know, people said, ‘You’ll never be able to sell your house, just leave it and walk. This place is collapsing, because nobody wants it, it doesn’t have a role anymore, it doesn’t have a mission’” (Chick Keller, quoted in Vasquez et al 1997: 69).

A Reversal of Fortune

Ironically, however, the Clinton administration’s strong opposition to nuclear testing would bring a reversal of fortune to Los Alamos. In 1994, he ordered the Pentagon to conduct the Nuclear Posture Review (NPR), a sweeping assessment of the role that nuclear weapons would play in maintaining national security in the wake of the Cold War’s end. In the end, 1994 NPR actually set the stage for a *reaffirmation* of nuclear deterrence and, by extension, Los Alamos’ weapons mission by asserting that the end of the Cold War had created a “world in which the proliferation of nuclear weapons and other weapons of mass destruction, rather than the nuclear arsenal of a hostile superpower, poses the greatest security risk.” In an uncertain post Cold War environment, maintaining a reduced nuclear deterrent capability and offering its protection to America’s non-nuclear allies would strike a “prudent balance between leading the way to a safer world and hedging against the unexpected” (Department of Defense 1995). As a result, by 1995, it was apparent that the “United States [would continue] with the policy of nuclear deterrence of the Soviet Union/Russia, accompanied by negotiated reductions” in strategic forces (Schell 2000: 30).

The Nuclear Posture Review put the Clinton administration in the odd position of reaffirming the importance of nuclear weapons for American security while simultaneously seeking an international ban on nuclear testing. This meant that the Department of Energy would have to develop an alternative means of maintaining confidence in the nuclear stockpile without placing the test ban in jeopardy. In 1994, both President Clinton and Congress each issued separate official directives requiring the DOE to "establish a stewardship program to ensure the preservation of core intellectual and technical competencies of the United States in nuclear weapons" (United States Department of Energy, Office of Defense Programs, 1995).

To address these concerns, the Department of Energy (DOE) in 1995 formally adopted a new, non-test-based, multidisciplinary approach to certifying weapons in the nuclear stockpile under the conditions of the CTBT. The Stockpile Stewardship and Management Program (SSMP) is a $4 billion-per-year, multiprogrammatic approach to maintaining confidence in the stockpile without redesigning or testing nuclear explosives. The SSMP is designed to maintain the "core intellectual and technical competencies of the United States in nuclear weapons" while maintaining confidence in the safety, security and reliability of the nuclear stockpile, without conducting nuclear tests (United States Department of Energy, Office of Defense Programs 1995). The program includes continuous stockpile inspection activities, including routine inspections to detect abnormalities or potential problems in weapons; analysis to determine the impact of abnormalities on weapon safety and/or performance, and limited repair and remanufacturing of defective components to maintain the integrity of the weapon. The program's success depends a great deal on the "core intellectual competencies" of the weapons laboratories, because the expertise of weapon designers and engineers is required to make judgments about inspection findings, to perform appropriate analysis, and to decide when and how to remanufacture aging parts.

To this end, one of the key components of SSMP is Science Based Stockpile Stewardship, or SBSS, a research paradigm for the weapons laboratories that is designed to replace the empirical validation provided by nuclear tests with a better scientific understanding of the underlying physics principles and basic phenomena that govern nuclear explosive behavior – hence the modifier "Science Based." Rather than perform nuclear tests, weapon designers at Livermore and Los Alamos will certify the stockpile using data from multiple sources, including archived nuclear test data, results from present-day subcritical and hydrodynamic experimental programs, inspection data, and experimental data that describes how materials in the weapons age. Supercomputing is the keystone of SBSS, since the weapons laboratories will be integrating multiple sources of data in elaborate nested models that simulate nuclear explosions. To this end, Department of Energy and the weapons laboratories have formed the Accelerated Strategic Computing Initiative, or ASCI, a research consortium composed of the DOE laboratories and several industrial and academic partners. ASCI's goal is to develop unprecedentedly powerful supercomputers capable of executing one hundred trillion floating point operations (one hundred teraOps) per second. If ASCI and SBSS are successful, one can imagine future generations of weapons experts using computers to integrate vast amounts of data and run complex predictive simulations of weapon behavior.

The DOE's Stockpile Stewardship and Management Program won generous fiscal and political support from the Clinton administration, which was acutely aware that, as long as nuclear weapons remained a pillar of national security, ratification of a CTBT would require backing from the experts who would be responsible for ensuring the health of the nuclear stockpile. Even as he announced in 1995 that the United States would seek to negotiate and ratify Comprehensive Test Ban Treaty, President Clinton reaffirmed the strategic importance of the American nuclear stockpile, saying,

> As part of our national security strategy, the United States must and will retain strategic nuclear forces sufficient to deter any future hostile foreign leadership with access to strategic nuclear forces from acting against our vital interest and to convince it that seeking a nuclear advantage would be futile… In this regard, I consider the maintenance of a safe and reliable nuclear stockpile to be a *supreme national interest* of the United States (United States Commission on Maintaining United States Nuclear Weapons Expertise 1999: 1; emphasis added).

Upon submitting the CTBT to the Senate for ratification, President Clinton also reaffirmed his commitment to maintaining the laboratories by including a series of six safeguards, the first of which was the conduct of a Stockpile Stewardship Program to maintain confidence in the nuclear stockpile. In addition, the President called for the laboratories to "maintain readiness" to resume basic nuclear test activities should the United States cease to be bound to the CTBT. As I discuss below, these mandates are a source of considerable concern for the weapons laboratories, Science Based Stockpile Stewardship notwithstanding. However, strong political and fiscal support for SBSS meant that the nation's nuclear weapons laboratories had emerged with a new mission, a generous budget, and central role in maintaining one of the nation's "Supreme National Interests," the Cold War's end and a test ban notwithstanding.

Stockpile Stewardship and Knowledge Loss

To anyone exploring the laboratory's website, reading public relations material, or reviewing LANL's annual mission statements, it is immediately apparent that, on an institutional level at least, the laboratory has fully embraced Science Based Stockpile Stewardship as the new and reigning paradigm for producing knowledge about weapons. With funding being poured into programs like ASCI and the construction of new experimental facilities at Los Alamos and Livermore, the practical emphasis of the nation's weapons program has, in a very real sense, turned away from full-scale nuclear testing.

At the same time, Science Based Stockpile Stewardship has not been entirely unproblematic. The nation's nuclear weapons laboratories are required to maintain readiness to return to designing and testing nuclear explosives if political circumstances should force the United States to withdraw suddenly from the CTBT. Secondly, the Department of Energy's weapons scientists and engineers are still responsible for certifying, year in and year out, that stockpiled nuclear devices will reliably perform to military specifications. They must do so despite the fact that they are no longer allowed to perform full-scale nuclear tests or design new devices to replace aging versions. To complicate matters, the hiring freezes and budgetary cutbacks in the late 1980s slowed recruitment activities and limited training opportunities, so that currently there is a dearth of younger staff trained in the design, testing and production of nuclear weapons. In addition, the laboratory lost many of its experienced staff members during the workforce reduction initiatives of the mid 1990s. Current staffing forecasts predict that the most knowledgeable persons in weapons programs at the Los Alamos National Laboratory will have reached retirement age or may even have retired from the Department of Energy by 2005. Without a full-scale program of nuclear testing, both junior and senior experts throughout the weapons community are concerned that future generations of weapons experts will not have the knowledge required to return to testing, or to make informed judgments about the stockpile.

For these reasons, knowledge loss is a major issue in the nation's three Department of Energy nuclear weapons laboratories. Worry is not confined to the weapons laboratories. It is also a Departmental issue, a Congressional concern, and is highlighted in a presidential mandate that the nation's nuclear weapons facilities "maintain readiness" to resume weapons-related activities within a given time frame, should the caprices of international politics make such activities necessary. This latter mandate presents a difficult mission to fulfill, given that nuclear weapons have been out of production since the early 1990s, that the testing program is effectively over in light of the proposed Comprehensive Test Ban Treaty, and that design work was halted by President Bush in early 1992 and has not resumed.

The laboratory – indeed, the entire DOE nuclear weapons complex – recognizes and is developing strategies to address the knowledge loss question. Science Based Stockpile Stewardship emphasizes a "knowledge transfer" initiative made up of two components, one of which seeks to capture and archive as much information as possible from the Cold War weapons programs, creating comprehensive, electronically archived design-and-manufacture histories for each of the systems in the enduring stockpile. The second component focuses on ensuring that newcomers are trained in key Cold War-era skills as identified by senior experts in various areas of the weapons community. Yet without an active program of nuclear weapons work, there are serious concerns that Los Alamos is growing rusty, that weapons knowledge is quite literally facing an imminent (and, from the perspective of most nuclear weapons experts, untimely) demise.

MY ENCOUNTER WITH KNOWLEDGE LOSS

My "ground level" encounter with knowledge loss began early in my fieldwork at the laboratory, during a conversation with an Hispanic engineer who had expressed a strong interest my anthropological background. At that point, I was still interested in studying women and minority issues in science, and based on our e-mail exchange, I had expected a discussion of diversity issues at Los Alamos, perhaps a commentary on the paucity of Hispanics in management positions. However, to my surprise, he wanted to talk about the end of the Cold War, how it had affected his ability to recruit and train new engineers. His experienced weapons engineers were talking about retirement, and he did not have many younger staff in line to replace them. Most of all, he missed working at the Nevada Test Site (NTS), worried that younger staff members who lacked NTS experience might not be able to execute a weapon test if international politics mandated it. He then paused, and said he had heard that I was studying mentoring at Los Alamos. Did I have any suggestions for using mentoring relationships to mitigate his "knowledge loss" problem, to encourage "knowledge transfer" across generations of weaponeers? I was a bit startled by this question, and stammered something to the effect that I was unfamiliar with the problems he was describing, that my focus was workforce diversity, not "knowledge transfer" or "knowledge loss." He looked a little disappointed, and asked, "But don't you anthropologists work with Native Americans to preserve stories, art, legends?" He paused and looked out the window, then looked back at me. "I mean, how do you save a dying culture?"

This was the first of many conversations I had with weapons experts about knowledge loss under Science Based Stockpile Stewardship. "It's not as though you can walk into university and sign up for a course in nuclear weapons," one physicist pointed out to me over a cafeteria salad one sunny November afternoon in 1997, during my early explorations of the knowledge loss problem. This is not simply a matter of security regulations. Rather, nuclear weapons science is a unique, specialized, multidisciplinary amalgamation of many research areas: physics, engineering, chemistry, metallurgy, to name a few. Throughout my fieldwork, experienced weaponeers described how they had developed their skills by working in some area of the weapons programs, and adamantly insisted that it is impossible to learn everything there is to know about weapons work without hands-on experience in the designing and testing of nuclear explosives.

Despite the fact that the laboratory seemed to be making a effort to capture and preserve weapons related knowledge, many of the physicists and engineers I met expressed concern that their "knowledge" was not as "valued" as it once had been, despite the fact that the laboratory's senior managers were emphasizing the importance of "preserving" and "transferring" certain skills and abilities. This was apparent in my first interview with the engineer who had described his work as part of a "dying culture." Another physicist I interviewed made a similarly telling comment: "I guess I can see why you'd want to study us. We're becoming a bunch of relics." Later, in the foyer of the laboratory cafeteria, he introduced me to one of his friends, a middle-aged engineer who had worked extensively at the Nevada Test Site. The engineer stared at me when my physicist friend described me as an anthropologist studying weapons culture: "Don't you folks usually study dinosaurs?" he asked, apparently mistaking anthropology for paleontology. Then he looked at the physicist and said, with a slightly sarcastic laugh, "Wait a minute. I keep forgetting that we *are* dinosaurs."

Despite the fact that I was doing fieldwork in a weapons laboratory, I still identified ethnography with narrowly focused descriptions of small-scale, pre-industrial societies on the fringes of the state system (Weldes et al 1999: 7). I thought of Los Alamos, in contrast, as a federally funded scientific laboratory that, like its academic and industrial counterparts, acts as an "intellectual engine of modern rationality with embedded institutional functions related to governance and social order" (Marcus 1999: viii). My original mentoring project "worked" in this setting because it dealt with questions of marginalization, gender and ethnicity, all themes that resonated clearly with Laura Nader's description of anthropology as the quintessential academic champion of the underdog. I found it difficult to imagine the laboratory's weapons community, one of the most politically powerful groups of scientists in the world, becoming the subject of an article in *Cultural Survival Quarterly.*

However, based on my early discussions with mid-career weaponeers, I started to think of knowledge loss as question of social reproduction, insofar as the people I had spoken with consistently expressed concern with "transferring" their knowledge to newcomers. In emphasizing concern about the rapid loss of technical and scientific skills, the weapons experts I met at Los Alamos seemed to be expressing a deeper anxiety about whether or not they would see their ways of knowing extended to another generation. Concerns about the survival of the laboratory's Cold War culture were framed in technical-political discussions about the production, maintenance and acquisition of weapons-related knowledge; about making tacit knowledge into explicit knowledge, so it can be transferred to young people; about the role of experience in teaching novices, about judging whether or not another individual is fluent in the practices and understandings of weapons work.

Secondly, and more subtly, I perceived a conflict between two visions of the "best" way to do weapons science, one rooted in the defunct Cold War design and test paradigm and the other emerging with the adoption of SBSS. The experts I met had acquired their skills during the Cold War, and although they expressed support for the Department of Energy's efforts to develop a new methodology for certifying the nuclear stockpile, they wondered how their Cold War skills would be worked into the new paradigm. To me, their concerns over knowledge loss represented an effort to assert the continued value of their knowing selves to the laboratory's weapons mission, even as the institution was making a firm commitment to SBSS as the reigning mode of knowledge production.

Gradually, I realized that I had arrived at Los Alamos at a time of crisis and recovery, at a point when a graying workforce was making a difficult and highly politicized transition from an established research paradigm to a new one. In subsequent lunchtime conversations with two experimental physicists, a retired engineer, and a theoretical physicist, I learned that knowledge loss was a widespread concern among Cold War weapons experts, many of whom worried that the next generation of weaponeers would not acquire the skills and understandings required to make sound judgments about stockpiled weapons. As I listened to these weaponeers describing how their career paths had changed in the wake of the Cold War's end, I realized that the "knowledge loss" issue might offer me the chance to document how various groups of scientists at Los Alamos were experiencing, and reacting to, the laboratory's transition from the familiar territory of bilateral Cold War arms race to the ambiguous, multilateral environment of the post Cold War era.

KNOWLEDGE LOSS AS AN ETHNOGRAPHIC PROBLEM

These issues I encountered among weapons experts seemed eminently anthropological to me, insofar as anthropology is centrally concerned with knowledge, in documenting how people "… acquire and display knowledge – rules values and beliefs" (Crick 1982), or "…what people employ to interpret and act on the world: feelings as well as thoughts, embodied skills as well as taxonomies and other verbal models" (Barth 1995). The issue of knowledge loss raised interesting anthropological questions about the relationship between knowledge and identity, about what it means to learn, to engage with the world in a particular fashion, to *know*.

At the same time, the fact that I was studying these issues in a weapons laboratory put me in a bit of a quandary: science represents skills, practices and knowledge that are supposed to transcend the local boundaries of culture, yet I was trying to understand the laboratory's knowledge loss problem, which stemmed from recent and sudden changes in the laboratory's paradigm for weapons science, as a cultural issue. Although a few social scientists before me had attempted to identify and critique cultural elements of nuclear weapons work, their descriptions were not particularly helpful (Mojtabai 1986, Rosenthal 1990) because they tended to ignore or take for granted the scientific and technical aspects of weapons work, focusing instead on the sociopolitical constructs that "enable" people to create weapons of mass destruction. I realized that any discussion of the knowledge loss problem would have to take into account the changing scientific practices of weaponeering, yet I lacked the technical background necessary to weigh the scientific merits of SBSS against those of the Cold War design and test paradigm. And as fascinating as it was to hear engineers likening themselves to indigenous peoples – a comparison that hinted at the enticing possibility of parallels between high-tech, Western military nuclear weapons science and traditional knowledge systems – I did not want to enter into philosophical arguments about the relative epistemological status of weapons science.

Sociological Approaches to Knowledge Loss

I had very few direct ethnographic models for addressing these problems. While sociologists use ethnographic methods to study Western science, and anthropologists use ethnographic methods to study non-Western knowledge systems, there are not many truly *anthropological* studies of Western *scientists* (with a few notable exceptions; e.g., Dubinskas 1988, Gusterson 1996, Kreiger 1992, Nader 1996, Traweek 1988a, 1988b, 1992, 1996). For the most part, anthropologists have left the study of Western scientific institutions to historians, philosophers and sociologists, while choosing instead to make a scientific problem out of non-Western cultures. In producing knowledge about the ethnographic Other, scientific methods have enabled ethnographers to transcend culture, to establish reliable understandings about the human condition. In a complimentary fashion, specializing in the study of the Other has given anthropologists a claim to a unique discipline with its own explanatory frames, methods and subjects.

Outside anthropology, however, there are many ethnographic studies of Western science, most of which emerged from the "sociology of scientific knowledge" movement that developed in France and Great Britain during the 1970s and 1980s. "Without much anthropological involvement," writes Bryan Pfaffenberger (1992: 491), European sociologists "discovered" participant observation as research tool, producing a steady stream of laboratory-based ethnographies (Latour and Woolgar 1979, Knorr-Cetina 1981, Pickering 1984, 1992; Latour 1987, Collins 1992). This movement called for scholars to critically re-evaluate the epistemological hegemony of the natural sciences over the social sciences and the humanities by documenting the social processes generative of scientific fact. Sociologists in this movement sought to "strip science of its extravagant claim to authority" by demonstrating the significant role that social negotiation plays in the production of scientific knowledge (Callon and Latour 1992: 346).

Initially, I was quite intrigued by this branch of scholarship because it seemed so anthropological, with its proponents focusing on small, clearly defined communities of actors and conducting extended periods of participant observation. In addition, certain threads of inquiry seemed particularly helpful in understanding why the laboratory was having difficulty capturing and encoding its Cold War knowledge for future generations. More specifically, several of these sociological ethnographies asked how scientific knowledge exists locally, emerging in relation to specific contexts and activities. This theme seemed important for understanding how knowledge can be "lost" as the context of activity changes: as Harry Collins has written, the myth of science as "universal" implies that all scientific knowledge and skills are or can easily be made explicit. Being universal, the processes, methods and skills of science should be exportable/importable from person to person, time and distance notwithstanding. Collins refers to this as an "algorithmical" model of science, which "…rests upon a notion of knowledge as a set of formal instructions, or pieces of information, about what to do in a variety of circumstances" (1992: 56).

However, following Michael Polanyi (1992: 54), Collins argues that algorithmic models of science are highly unrealistic because they fail to take *tacit* knowledge into account. Tacit knowledge is embodied, inchoate knowledge that can only be acquired through experience. According to Collins, being knowledgeable is not a matter of possessing a specific set of discrete skills, but involves a process of becoming skillful, of being able to perform fluently in a research setting without necessarily being able to articulate the source of one's knowledge. In describing how tacit knowledge is transferred among laser physicists, Collins develops an ethnographically-based "enculturational model" of science, stressing that "…if a crucial component of laser building ability is tacit knowledge, then it should come as no surprise that written information [would be] inadequate" (1992: 57) for transmitting laser-related knowledge from person to person, even among experienced physicists.

Donald MacKenzie and Graham Spinardi (1995) use Collins' observations on tacit knowledge in their provocative article on the "uninvention" of nuclear weapons, in which they suggest that permanent ban on nuclear testing could lead to the decline and eventual disappearance of tacit weapons-related knowledge. Using work-narratives gathered among weapons experts at Los Alamos, Sandia and Livermore, they emphasize that nuclear weapons were the product of teamwork among members of a "complex and differentiated organization" in which the flow of tacit, unwritten understandings about different aspects of weapons design, engineering and testing were crucial in tying together many different disciplinary communities. Becoming a useful member of this community required years of training as novices mastered an enormous body of unwritten knowledge about weapons, gradually developing that critical sense of judgment without which "the functional capabilities of nuclear explosives cannot be fully established… [this judgment] tests on knowledge that has not been, and perhaps could not be, codified" (1995: 62). MacKenzie and Spinardi argue that without practice in designing and testing nuclear explosives, it will be difficult, if not impossible, to reproduce Cold War knowledge in future generations of experts.

Observations like these on the role of tacit knowledge in science were extremely useful in understanding why Los Alamos was so concerned about the impact of the test moratorium on its experts. Nevertheless, I found that most sociological ethnographies were largely silent when it came to the significant emotive issues that I sensed behind jokes and comments about "dinosaurs," "relics," and "dying cultures." Indeed, as I compared the sociology of science to what I pictured as a critical anthropology of science, it struck me that these writers conceptualized culture quite narrowly, their ethnographic approach notwithstanding. As sociologist Andrew Pickering has written,

> *culture*… denotes the field of resources that scientists draw upon in their work, and *practice* refers to the act of making (and unmaking) that they perform in this field. [These terms are] not a way of gesturing at grand, all encompassing worldviews, for example, or at big cultural currents that flow between science and the outside world… (1992: 4).

In this project, paradigmatic shifts in certain areas of science – such as nuclear weapons research and development – present valuable case studies for epistemological critique, successfully demonstrating the significant role of non-explicit knowledge in Western science. At the same time, I did not find sociological perspectives particularly helpful in understanding how engagement in scientific practice creates people who meaningfully identify their lives and work in relation to larger social, political and moral structures, or in documenting what happens to these people when their landscape of meaning and practice shifts.

Anthropology, Indigenous Knowledge, and Western Science

The sociology of scientific knowledge is an explicit and direct critique of Western science, one that works largely from within the Western canon of knowledge to place the humanities, the social sciences and the natural sciences on the same epistemological plane. Sociologists of scientific knowledge are indebted to anthropology's twin hallmarks, culture and participant-observation, because they have provided epistemological and methodological lynchpins for claims about the social, and therefore relative, ontological status of Western scientific knowledge (see also Cole 1996, Haack 1996). As Laura Nader points out, "…it is important to recognize that crucial ideas coming out of early anthropological work, such as relativism, comparison, and ethnographic fieldwork, greatly benefited… the work of science and technology studies in history, philosophy and sociology" (1996: 225).

In contrast, anthropology tends to critique Western science from outside the Western tradition by identifying and asserting the subjugated knowledges of non-Western peoples. In addition, the critical focus is shifted to rest on knowers as well as knowledge. Anthropological accounts of knowledge production describe how knowing people are embedded within in, and reproduce, the moral, social and political bodies of understanding that mark their engagement with the world. This holistic perspective resonated with my sense that weapons experts were lamenting more than the simple erosion of tacit skills and abilities, encouraging me to consider how critical issues of identity and meaning might be encoded in local concerns about knowledge loss.

Most anthropologists who study science focus on the production of non-Western empirical knowledge. This field of study, also referred to as ethnoscience, has long been an important thread in anthropological inquiry (Nader 1996, Goodenough 1996: 41). Rooted in cognitive anthropology – which seeks to discover what individuals need to know in order to function fluently in a particular social environment – anthropological studies of indigenous knowledge systems frequently challenge the idea that non-Western knowledge is epistemologically inferior because it is "knowledge that may be considered true only within a specific cultural narrative world; for example, rules of specific cultural practices, origin stories, folk aphorisms" (Purcell 1998: 259). In this sense, studies of indigenous knowledge represent far more than an epistemological pursuit: they connote a political stance, often challenging the legitimacy of Western science as a sole basis for policy decisions that may impact native peoples without taking their traditional ecological and social ways of knowing into account. As Antweiler has written,

> …the meaning of the term indigenous... has come today to be used in a context in which "non-Western" or "anti-Western" knowledge, or the knowledge of minorities, is compared and contrasted with knowledge at the level of the nation state… Given that there is an intention to promote small or marginal groups, "indigenous knowledge" is far from being just a purely descriptive term…(1998: 460).

Indeed, debates over the epistemological "legitimacy" of indigenous knowledge are simultaneously political debates, expressed in epistemological and ontological issues: judging what kinds of knowledge qualify as indigenous, identifying internal standards for truth and falsity, or evaluating the cognitive content of indigenous knowledge vis-à-vis Western science.

I did not want to use the knowledge loss problem Los Alamos as an entrée into comparing the cognitive content of weapons science with that of indigenous knowledge systems. However, because studies of non-Western knowledge systems are fuller than most sociological critiques of Western science, I found ethnographies of indigenous science quite useful in shaping my thinking about weapons science at Los Alamos as knowledge-in-context. As Colin Scott (1996) points out, it makes little sense to extract local knowledge from its context, because the production of empirical knowledge is linked to the formation and maintenance of worldviews that, in turn, make the pursuit of knowledge meaningful. For example, in describing the ecological knowledge of the James Bay Cree, Scott points to a root "paradigm of a sentient, communicative world that transcends but includes humanity" that orients the Cree to their environment in such a way that they are astoundingly adept at predicting the movement and behavior of animals around them. Anthropology, he argues, is the only Western discipline positioned to assert the epistemological sophistication of knowledge systems like that of the Cree: "Our understanding of practical indigenous knowledge cannot be adequately formulated without reference to the root metaphors most vividly condensed in myth and ritual," he writes. "Anthropology is unique in the degree to which it…values ways of translating indigenous knowledge that reflect the symbolic and institutional contexts in which the knowledge is generated" (Scott 1998: 71-72; see also Rushforth 1994).

Although most anthropologists study knowledge production in non-Western settings, several have successfully pursued this kind of holistic ethnographic critique among Western scientists (Gusterson 1996, Kreiger 1992, Traweek 1988a, 1988b, 1992, 1996). Unlike their sociological counterparts, however, they rarely challenge "science *qua* science," (Collins 1997: 9). Instead, they explore how multiple, complex, locally significant meanings can be embedded within, and exist around, scientific "facts" whose relevance often does transcend cultural boundaries. In doing so, they emphasize that science is not pursued in a vacuum, but exists in a dynamic relationship with seemingly more ephemeral bodies of understanding like morality and politics.

Among these ethnographies, I was most interested in the work of Hugh Gusterson and Sharon Traweek. In his ethnography of weapons designers at Lawrence Livermore, Gusterson explores weapon designers as members of a moral-scientific community organized around a central axiom: "…the laboratory designs nuclear weapons to ensure, in a world stabilized by nuclear deterrence, that nuclear weapons will never be used…. [They] exist to save lives and prevent war" (1996: 56-57). In linking the technical activities of weaponeering to the reproduction of the moral universe of his subjects, he describes how the development of weapons technologies at Lawrence Livermore had the concomitant effect of producing larger "common sense" discourses about weapons, safety, threat and security. These, in turn, create a meaningful context for Livermore's pursuit of knowledge about weapons. Gusterson's approach to weapon science is most classically anthropological when he applies ritual analysis to nuclear weapons testing: for members of Livermore's weapons community, he argues, the process of conducting a nuclear test represented an act of hope that symbolized not just faith in the power of deterrence, but also "the fertility of the scientific imagination… a weapon is destroyed, and a community is born" (1996: 164).

Similarly, Sharon Traweek explores how the worldview of the international high-energy physics community – which she describes as a "culture of no culture" – is reproduced as novice physicists in both the United States and Japan gradually learn to manipulate the assumptions, practices, beliefs, understandings, rules and actions that gird local communities of physicists. As they are transformed into competent practitioners of physics, unschooled neophytes learn to "represent their world as free of their own agency" (1988a: 162); and in doing so, reproduce both their local culture of research practices while perpetuating and extending the larger field of activity that is international physics.

Like their counterpart sociologists working in Western scientific communities, anthropologists explore how the production of empirical knowledge is embedded within a set of spatial and historical boundaries. However, unlike most sociology of scientific knowledge, the anthropological goal is to understand how the activity of knowing is intimately linked with other significant cultural dimensions. This holistic approach was quite different than what I had initially encountered in the ethical critiques of weapons science (e.g.. Rosenthal 1990) and in the narrow sociological focus on the knowledge dynamics of Western science. At the same time, I was intrigued by the way that sociologists of science focused so intently on the generation and transmission of tacit knowledge among scientists. In their own ways, both sociologists and anthropologists emphasized that knowledge exists locally, that the production of knowledge occurs within historically and spatially defined boundaries. Studying knowledge loss at Los Alamos, I thought, would provide me the chance to engage in a conversation with both areas of literature.

Los Alamos as a Community of Practice

Although I had identified parallels and points at which the anthropology and the sociology of science could engage with each other, I was having a difficult time bringing this conversation to life in my fieldwork. I was not entirely sure how to observe tacit knowledge in action, or to document how the transmission of tacit knowledge had changed at Los Alamos. Similarly, I was not sure how to locate connections between scientific practice and the laboratory's institutional worldview, which remained centered around nuclear deterrence despite the fact that the Cold War was over. More difficult still was figuring out how to connect changes in the shifts in the production and transmission of tacit knowledge to the maintenance of this worldview, which I considered rather anachronistic. I needed a perspective that was capable asking practical questions about the generation, emergence, and transmission of tacit knowledge, one that would link the dynamics of learning and knowing to larger questions about the laboratory's worldview and institutional mission. I found just such a set of tools in the writings of Jean Lave, Etienne Wegner, and Seth Chaiklin, whose ethnographic, cross-cultural studies on learning caused me to recast knowledge and knowing in terms of community, membership, practice and identity.

In her studies of apprenticeship among West African tailors, Jean Lave (Lave and Wenger 1991: 29-43; 69-72) asks a simple question: what does it mean to learn? Lave and her colleagues explore this question by first emphasizing that knowing always occurs within a particular social context. They locate all human knowledge within "communities of practice" (Lave and Wenger 1991, Chaiklin and Lave 1996, Wegner 1998): variably organized social entities that emerge over time as individuals engage with each other, and with various aspects of the physical world, in the sustained pursuit of a particular enterprise (Wenger 1998: 45). Because they exist as "…purposive sets of relations… among persons, activity, and the world," communities of practice "…are an intrinsic condition for the existence of knowledge," because they provide interpretive frames of reference that make human action meaningful (Lave and Wenger 1991: 98).

Communities of practice are critical in understanding the social organization of knowledge because they provide a social location for the maintenance and extension of collectively held ways of knowing. This theory became a lynchpin in my research because of its clear emphasis on a) local communities as lively institutions through which individuals engage with, and reproduce, the world around them, and b) the centrality of meaning and identity in understanding *how* individuals *know*. In framing knowledge and knowing as social phenomena, this paradigm emphasizes that learning involves more than the acquisition of skills and information. Rather, as novices learn, they are embarking on a trajectory of membership and participation in a particular community of practice: as Etienne Wegner writes,

> A community of practice is a field of possible trajectories and thus the proposal of an identity. It is a history and the promise of that history. It is a field of possible pasts and of possible futures, which are there for all participants to

> engage with… [Hence] understanding something new is not just a [discrete] act of learning. Rather, [it] is an event on a trajectory through which [learners] give meaning to their engagement in practice in terms of the identity they are developing (1998: 155-156).

In this sense, learning involves the concomitant, ineffable transformation of the self, as novices begin to understand possibilities for defining themselves in relation to the past and the future of community. Moreover, learning cannot occur unless novices engage deeply with the goals, values and practices of the community they are joining. In other words, meaning becomes a core requirement for the perpetuation and extension of communally held knowledge.

Using this perspective on learning as a socially transformative process of engagement with the world, I began to locate my conversation between anthropology and sociology in the work-narratives I collected from weapons experts. Along the way, I discovered that I was far less interested in knowledge *per se* than in the very human activity of knowing. My explorations of the weapons community convinced me that knowing is intricately connected to the formation of identity, to the way that we locate ourselves in relation to other people and to the physical spaces we inhabit. Gradually, as I listened to people talk passionately about their work, and watched designers and engineers explaining and solving technical problems, I came to realize that identity is not merely a matter of aligning oneself with a particular social category, nor is it revealed in the strategic display and manipulation of symbols. Rather, as I began to think of learning and knowing as intrinsically connected to the formation of identity, I realized that we define ourselves according to the way we engage with the world around us. Indeed, the formation of identity is the process through which we build living, knowing linkages with other people; and it is through our communities of belonging that we actively relate to the world. In this sense, a weapons laboratory really is not much different than a community of Cree hunters in the Canadian wilderness, since both provide their members with a means of engaging deeply with the social, natural, physical, and moral worlds in which their communities are embedded.

In documenting how their pursuit of empirical weapons knowledge is rooted in, and reproduces, wider structures of meaning, I have attempted to write a critical ethnography of the weapons community, exploring the ties that bind weapons experts, their knowledge, and the laboratory to the shifting historical context of the Cold War. In doing so, I have incorporated sociological perspectives about scientific practice as a tacit, context-dependent form of knowledge that is generated and transmitted experientially. At the same time, understanding weapons-science-as-culture calls for more than narrow epistemological critique. After all, the shifting political environment of the Cold War ultimately brought significant paradigmatic changes to the weapons community, yet the maintenance of nuclear deterrence was dependent on the laboratory's ongoing pursuit of weapons knowledge. This suggested a dynamic connection between the wider sociopolitical context of weaponeering and the local setting of its pursuit at Los Alamos, and points to the fact that individual elements of the Cold War weapons community – knowledge, weapons, experts – were (and remain) mutually constituted, each implicated in the existence of the other.

THE PLACE

Los Alamos, New Mexico, is located somewhat precipitously along the mesas and canyons of the Pajarito Plateau, an apron of volcanic tuff that stretches to the Rio Grande Valley from the eastern rim of the Jemez Mountains. This area of New Mexico shows evidence of volcanic and tectonic activity dating to approximately 13 million years ago. The present-day Jemez range and its accompanying plateau are remnants of a much larger volcano that would have rivaled modern Himalayan peaks with an estimated altitude of 26,000 feet. The striking expanses of the Jemez Caldera, a valley formed by the volcano's eruption, are evidence of the mountain's once-prodigious size, just as the miles of volcanic cliffs and mesas that skirt the Jemez Mountains point to an enormously violent eruption some twenty-five centuries ago. The explosion not only formed a range of smaller mountains; it sent basalt, andesite, dacite, quartz latite, and rhyolite spilling over earlier rock formations, creating a flat cap of multilayered volcanic rock that juts south and westward below the peaks of the Jemez range (Dransfield and Gardner 1985).

Covered with Pleistocene ash flows, and skirting the southwestern edges of the Jemez Mountains, this flat cap is known to archaeologists, geologists, and local residents as the Pajarito Plateau (Steen 1977). Since the late 1800s, this area of New Mexico has been famous for its riche cache of archaeological ruins, many of which are nearly a thousand years old. Adolf Bandelier "discovered" these ruins in 1880 at just about the same time that American anthropologists were attempting to establish a unique scientific discipline dedicated to the study of human history and culture. Throughout the next century, the plateau and its surrounding Native American and Hispanic peoples became valuable resources for white ethnologists and archaeologists like Edgar Lee Hewett, who used the ethnographic riches of the region as a springboard to found a distinctively Southwestern school of American archaeology. By the early 1900s, many indigenous settlements in the southwestern United States became sites in "Harvard's Backyard,'" so that New Mexico was transformed into a place where fledgling anthropologists could learn their craft. Even today, thousands of tourists visit nearby Bandelier National Monument to explore cliff dwellings, ruins, cave and rock paintings, ceremonial kivas; and to look at the baskets, pots, projectile points, and other artifacts left by the Anasazi peoples who once inhabited these cliffs and canyons.

The Pajarito Plateau is a strikingly beautiful place, its reaches cut into a maze of twisting, finger-like mesas and canyons by centuries of slow erosion, as water has drained seasonally from the heights of the Jemez peaks to the Rio Grande Valley below. Driving west on Highway 4 from San Ildefonso Pueblo, one is overshadowed by ancient mesas of crumbling white tuff, orange sandstone and dark basalt, their flat stony surfaces feathered with dark green stands of juniper and piñon; while between the mesas lie deep, precipitous canyons whose narrow headways gradually widen into gentler alluvial plains as they drop into the Rio Grande basin below. Arid and inaccessible, the Pajarito Plateau is home to some of the loveliest country in New Mexico.

Although I have often heard residents of Los Alamos describe their town as an isolated backwater lacking civic amenities, the area's physical beauty and remoteness are precisely why the laboratory is located here. Los Alamos is famed as the place where the world's first atomic bomb was developed during World War II's Manhattan Project. J. Robert Oppenheimer, the scientific director for the Manhattan Project, founded the laboratory in 1943. Oppenheimer had spent part of his boyhood in New Mexico and spoke often of his desire to combine his two great loves – physics and the desert landscape of the American southwest. In late 1942, he persuaded General Leslie Groves of the Manhattan Engineering District to condemn approximately forty square miles of land belonging to the Los Alamos Ranch School and adjoining Hispanic and Native American communities for the purpose of establishing a temporary wartime research and development laboratory, a top-secret "intellectual center," for the nation's atomic bomb project. Initially, Oppenheimer and Groves intended that the facility exist only for the duration of the war. However, the success of the wartime project – as demonstrated in the Trinity test of July 1945, and the subsequent bombings of Hiroshima and Nagasaki in August of the same year – caused the federal government to rethink the laboratory's temporary status. In 1946, Congress passed legislation establishing the Atomic Energy Commission (the predecessor to today's Department of Energy) and in doing so, made Los Alamos into a permanent research and development center for the nation's burgeoning nuclear weapons program.

Throughout the Cold War, from 1945 until 1992, the scientists and engineers at Los Alamos maintained one of the world's most sophisticated nuclear weapons research and development programs. Until the 1970s, the laboratory was narrowly focused on weapons work, with smaller spin-off programs in nuclear reactor research, nuclear-fueled rockets, and basic physics. During the energy crises of the 1970s, however, the laboratory's charter broadened dramatically as the Atomic Energy Commission underwent political reconfiguration to become the Department of Energy in 1977. The newly formed DOE was charged with four basic mission areas: national security, energy resources, environmental quality, and basic scientific research. During this time period, Los Alamos' leaders encouraged researchers to pursue non-military initiatives in alternative energy, laser fusion, geothermal energy, solar power, environmental sciences, human biology, chemistry and materials development. With the diversification of the DOE's mission and the laboratory's research portfolio, non-weapons funding became increasingly common as the laboratory's base of research interests and fiscal support expanded. By the early 1980s, Los Alamos had been transformed into one of the DOE's flagship "multi-program sites" (United States Department of Energy 1999); and today is one of the largest such institutions in the world, with a broad range of research programs and collaborations with universities and industries throughout the world.

Despite the military character of its responsibilities towards the nuclear stockpile, the laboratory, along with the twenty-seven other research facilities owned by the Department of Energy, has historically existed as a civilian organization. Members of the laboratory's workforce, even those who have spent long careers designing and developing nuclear weapons, are quite adamant about their civilian status. The direct presence of the military at Los Alamos is limited, despite the central role that Los Alamos has played in maintaining America's nuclear deterrent capabilities. And although the lion's share of the laboratory's fiscal support comes directly from the federal government, rarely do workers at Los Alamos describe themselves as "government employees." Instead, they seem to identify more strongly with academic institutions than they do with the military, with the federal government or private industry. To a great extent, this can be attributed to Los Alamos' status as a "GoCo" facility (Government Owned, Contractor Operated) administered by the University of California on behalf of the federal government. Roughly 6,800 of its 12,000 or so employees are directly employed by the University of California, while another 2,800 work for local and out-of-state contractors that bid services to the laboratory.

The majority of Los Alamos' researchers are physicists, engineers, or chemists; while others work in mathematics, computational sciences, human genome research, geophysics and climatology, and other disciplines. In addition, the laboratory employs an army of administrative and research support staff: secretaries, librarians, machinists, technicians, janitors, computer systems administrators, accountants. This diverse workforce is headed by a director, who is chosen by a search committee headed by the Regents of the University of California. Since Oppenheimer's tenure during World War II, Los Alamos has had five directors, each with a similar pedigree: Ph.D.-holding, middle-aged white males, all chosen from the ranks of the weapons programs, and – with the exception of former director Sigfried Hecker, a metallurgist – all physicists. The current director, John Browne, is an experimental physicist who once managed the Los Alamos Neutron Science Center (LANSCE), the laboratory's sprawling high-energy physics research facility.

The laboratory is administratively subdivided into program *directorates*, each of which is managed by either a deputy laboratory director (for business and administrative functions) or an associate laboratory director (for research functions). The program directorates, in turn, are composed of research *divisions*, which are a conglomeration of three or more research *groups* (see organizational chart). As tidily hierarchical as the laboratory's administrative structure appears on paper, it is constantly changing, much to the exasperation of laboratory staff. Los Alamos tradition dictates that an incoming director and his managers make their historical mark on the laboratory by reorganizing it:

breaking one directorate into two, merging two directorates into one; moving divisions among directorates, re-naming divisions, shuffling groups from one division to another, all depending on perceived administrative linkages among research areas. Because reorganization happens so frequently (and seemingly capriciously), it is a favorite theme for poking fun at upper management. One of the laboratory's weapon testing groups – which has done the same type of engineering work and been staffed with the same people for years – has decorated its office at the Nevada Test Site with a set of small mock gravestones, each marking the birth and demise of a different name and organizational location in its forty-plus years of existence.

The longer I worked at Los Alamos, the more I realized that place is by far the most stable feature of laboratory organization. Spatially speaking, Los Alamos is divided into numbered Technical Area, or TAs, where specific functions are located: TA-55 houses the plutonium facility, for instance, while TA-16 is weapons engineering and high explosives research. As a rule, people who perform a particular job – for instance, high explosives machining, genome mapping, or particle physics research – will generally do that work in the same place, year in and year out, administrative shuffling notwithstanding. There are roughly thirty-five Technical Areas at the laboratory, some of

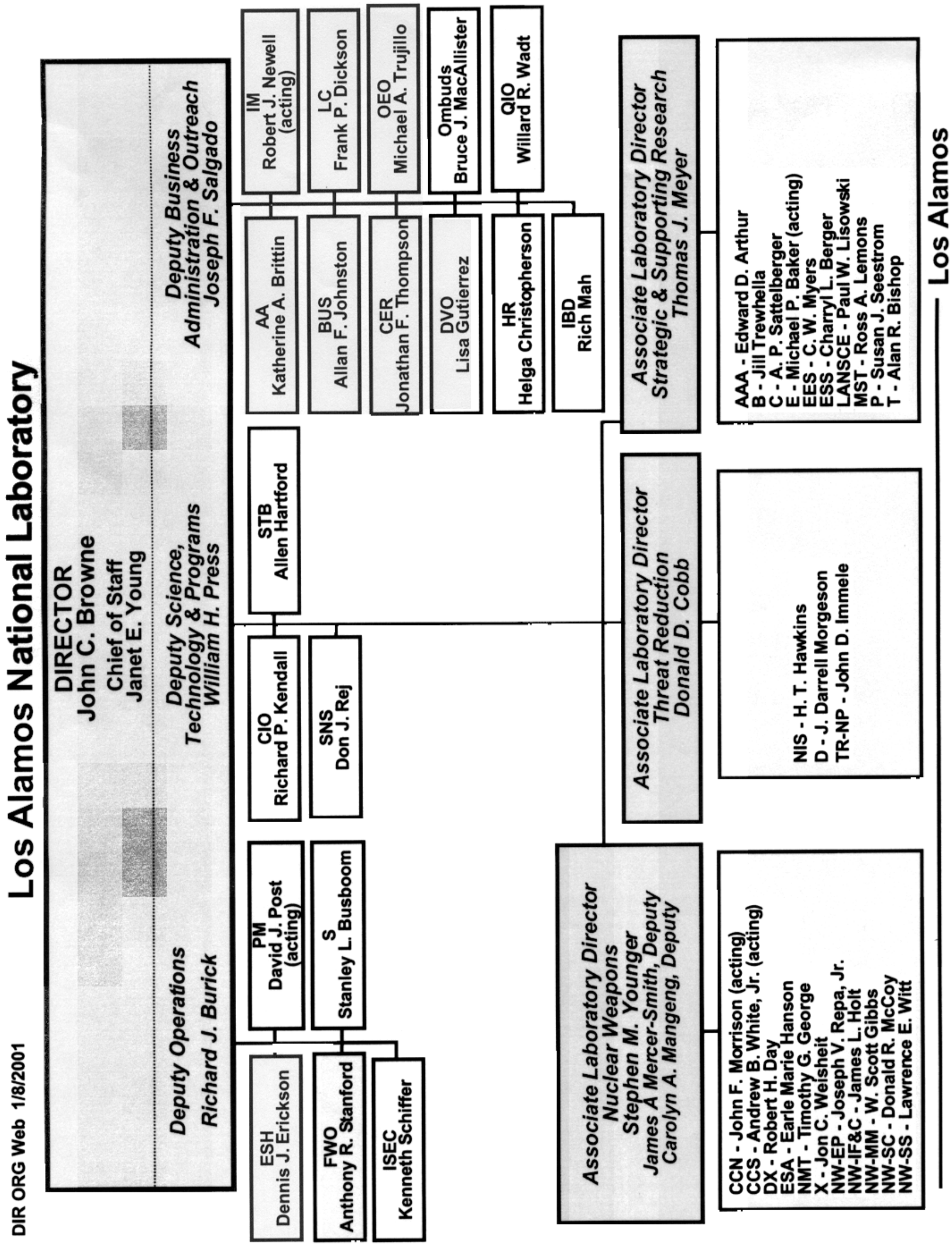

Figure 1-1. Laboratory Organizational Chart

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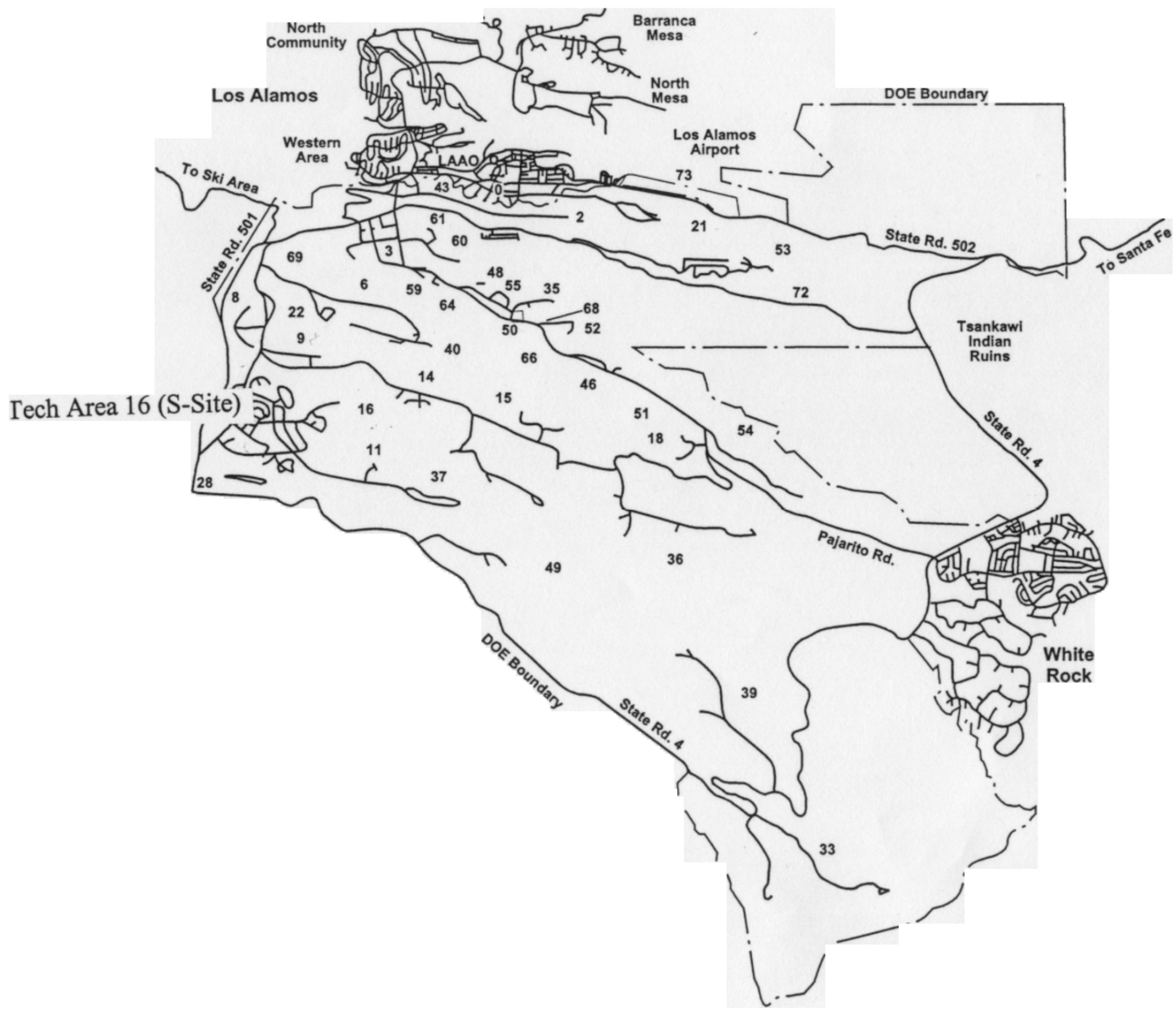

Technical Area Locations

TA-0	Unassigned Land Reserve
TA-2	Omega Site
TA-3	South Mesa Site
TA-5	Beta Site
TA-6	Two Mile Mesa Site
TA-8	Anchor Site West
TA-9	Anchor Site East
TA-11	K-Site
TA-14	Q-Site
TA-15	R-Site
TA-16	S-Site
TA-18	Pajarito Laboratory
TA-21	DP-Site
TA-22	TD-Site
TA-28	Magazine Area A
TA-33	HP-Site
TA-35	Ten Site
TA-36	Kappa Site
TA-37	Magazine Area C
TA-39	Ancho Canyon Site
TA-40	DF-Site
TA-41	W-Site
TA-43	Health Research Lab & DOE Headquarters
TA-46	WA-Site
TA-48	Radiochemistry Site
TA-49	Frijoles Mesa Site
TA-50	Waste Management Site
TA-51	Radiation Exposure Facility
TA-52	Reactor Development Site
TA-53	Meson Physics Facility
TA-54	Waste Disposal Site
TA-55	Plutonium Facility Site
TA-57	Fenton Hill Site
TA-58	Two Mile North Site
TA-59	OH-Site

Figure 1-2. Map of Los Alamos National Laboratory.

them housing several dozen research facilities, parking lots, office buildings, and "portables" – trailers where summer students and postdoctoral students have their offices (see map).

Despite its illustrious scientific reputation and high-tech mission, Los Alamos looks on the outside like a rather low-tech place. Sharon Traweek (1988a) has commented that most non-scientists expect scientists to wear white coats and work in immaculate laboratories with shiny, clean, high-tech equipment. Similarly, I found that outsiders visiting Los Alamos tended to be rather surprised, if not disappointed, by the laboratory's decidedly frumpy architecture. Nearly a third of the laboratory's office buildings and facilities were built before 1970. The main Administration Building was built in the 1950s, and despite the fact that it houses such important functions as the laboratory's weapon designers and the director's offices, its hallways very much resemble those of an aging high school building. Many of its offices lack air conditioning, the flooring is either brown carpet or vinyl tile, and exposed heating, cooling and wiring pipes run along the hallway ceilings. Although LANL and DOE managers are currently planning several new facilities to replace aging ones like the Administration Building, the majority of the laboratory's buildings and warehouses appear remarkably worn out, and in most places landscaping is nonexistent.

The laboratory's dress code is quite casual. When I arrived at Los Alamos, I assumed I would have to shed my graduate school jeans-and-sweater uniform for a more tailored wardrobe. However, Los Alamos prides itself on its campus-like atmosphere, and scientists and engineers – people who come from graduate school environments – tend to eschew fashionable clothing for jeans and tee shirts. A few areas of the laboratory, particularly the Business and Human Resources divisions, tend to align themselves more with industrial culture than with academia, and their staff members observe a de facto "business casual" dress code. When I started working at Human Resources, I was careful to style my hair and wear nice skirts, jewelry and lipstick, but I quickly realized (to my relief) that the scientist and engineers I met, both men and women, seemed to take me more seriously as a researcher and as an "insider" when I wore jeans, a tee-shirt, a ponytail and no makeup. Later, I was told that "only secretaries, job interviewees and politicians" dress up at Los Alamos; indeed, it is a laboratory joke that visitors and newcomers are easily distinguishable by their nice clothing.

METHODOLOGY

I became interested in Los Alamos as a field site nearly a decade ago, when I was just beginning my graduate work at the University of New Mexico. Growing up in the 1970s and 1980s among an array of decidedly antinuclear friends and family in Albuquerque, New Mexico, I had come to think of Los Alamos and its sister laboratory, Sandia in Albuquerque, as ethically reprehensible bomb factories. Yet they were also two of the most politically, socially and economically powerful institutions in the state, seemingly permanent and unassailable despite the most concerted efforts of the massive antinuclear movement that burgeoned in New Mexico throughout the late 1980s.

However, the end of the Cold War brought rapid and unprecedented changes to the nation's weapons laboratories. As I completed my Masters' degree in cultural anthropology at the University of New Mexico and made preparations to pursue doctoral fieldwork, I spent a great deal of time watching Los Alamos grapple with these changes: collecting newspaper clippings, volunteering once a week in the archives of the Los

Alamos County Historical Museum, attending DOE-sponsored public meetings about the future of the laboratory, conducting occasional interviews with long-term residents to gather memories of their lives in Los Alamos. For a long time, my interest in the laboratory was actually quite peripheral: it was the town of Los Alamos, one of America's Cold War "atomic cities," that fascinated me. I planned to study the formation and development of the community, the domestic sphere surrounding the masculine, scientific-military world of the laboratory, exploring how American Cold War values were inscribed in the town's social and physical landscape, and documenting how the community was changing with the Cold War's end.

My focus shifted from the town to the laboratory in early 1997, however, when a friend told me that the laboratory administered a program to sponsor students pursuing dissertation research. At about the same time, I discovered that the laboratory's Human Resources Division was working to develop a mentoring program aimed at providing support and training for women and minorities in the workforce, so they could widen their range of career opportunities. One of my committee members, Ruth Salvaggio, had studied the career obstacles that women scientists and engineers encounter at Los Alamos and suggested that I contact Gloria Cordova, an administrator in the laboratory's Human Resources Division. In March of 1997, I wrote Cordova a letter proposing a study of mentoring relationships and career development. I explained that I was interested in documenting the kind of career obstacles faced by non-white and/or women scientists and engineers, and that I thought my research might enhance the efficacy of the laboratory's proposed mentoring program. Although she no longer worked in Human Resources, Cordova forwarded my letter to the team in charge of the mentoring program; and in

August of 1997, I was hired as a Graduate Research Assistant in the laboratory's Training and Development Group. Our agreement was simple: I would help the group understand the dynamics of its pilot mentoring program; they, in turn, would facilitate my dissertation fieldwork with an office, a computer, financial support, and – perhaps most importantly, given the laboratory's reputation for insularity – an official "in" as a laboratory staff member working on a LANL-sanctioned project.

My official fieldwork began on August 4th, 1997, when I joined Human Resources as a Graduate Research Assistant. It ended in April of 2000, when I stopped collecting data to focus on analysis and writing. I divide this fieldwork into three phases: an exploratory phase between August of 1997 and February of 1998, when I worked for Human Resources; a middle stage between February 1998 and February of 1999, when I moved from Human Resources to pursue my dissertation research in the laboratory's Archives and History Programs; and a final, intensive phase between February 1999 and April of 2000, after I received a security clearance from the Department of Energy and gained full access to people, places and information in the nuclear weapons programs at Los Alamos.

Initially, the Human Resources mentoring program provided me with an interesting and manageable dissertation project. The laboratory is a large place; and although nuclear weapons research and development has historically provided Los Alamos with its core research mission and funding, the laboratory houses many other non-weapons-related research programs: in geology, biology, botany, environmental science, supercomputing, theoretical physics, chemistry, to name a few. Although I was curious about the impact of the Cold War's end on the weapons community, I seriously doubted I would have the opportunity to gain access to any of the weapons divisions. The areas of the laboratory involved in weapons research are spectacularly protective of their secrets; and when I expressed an interest in observing mentoring relationships among weapon designers, one of my HR colleagues told me I had little chance of getting a security clearance. Within a few weeks of starting my fieldwork, then, I decided to focus on mentoring and career development issues with the people involved in the pilot mentoring program – none of whom were directly involved in nuclear weapons research.

Most anthropologists have experienced the confusion of starting fieldwork with a set of ostensibly good research questions, only to have their preparations blown to smithereens by some extraordinary encounter that shakes all presumptions about what is "important" at their field site. This is precisely what happened to me in September 1997, just after I had a meeting with Mary Meyer, an anthropologist employed at Los Alamos who would become one of my most important mentors during my fieldwork. Meyer had received her doctorate in ethnology from UNM in 1986, with a dissertation about workforce issues at Los Alamos. Subsequently, she had spent most of her career working in various areas of the laboratory, including the weapons programs. After listening to my ideas about studying mentoring and minorities at Los Alamos, she asked if I would like to discuss my research topic with people who worked in the weapons programs. Later that afternoon, she sent me an e-mail saying she had forwarded my name to several acquaintances in the weapons programs.

It was through Meyer's contact list that I met the Hispanic engineer who introduced me to the knowledge loss issue, an encounter that permanently changed the direction of my research program. At this point, I re-evaluated my initial dissertation topic, and by November of 1997 or so I had decided to drop the mentoring program in favor of focusing on the knowledge loss problem in the weapons community.

Access to the Weapons Programs

Once I had decided to pursue the knowledge loss issue, I started looking for a position at the laboratory that would provide my new research project with the same legitimacy that Human Resources had given the mentoring project, but with greater access to the weapons programs – perhaps even a security clearance. In November of 1997, one of the physicists that I had met through Mary Meyer suggested that I contact the laboratory's Archives and History Programs office. Roger Meade, the laboratory's archivist, had recently received a small amount of funding from the Department of Energy to pursue an historical archiving project covering the laboratory's last series of nuclear tests, which occurred between 1991 and 1992 under the test operation code-named Julin.

Although I did not yet have a security clearance, Meade hired me as a research assistant help with "Archiving Operation Julin," as the project was called, saying that my proposed involvement in a classified project might justify his request for my security clearance. Although I was still uncleared, and therefore unable to access most of the records for the Julin project, the situation seemed ideal for studying the knowledge loss problem. One of the major goals of the Julin project was to archive testing-related documents knowledge for the future, so that later generations of weaponeers and historians would understand how the Cold War testing program worked. Being involved with this kind of project, I thought, would make it easy for me to ask my own dissertation-related questions about the larger issue of knowledge loss. In addition, the project would require a great deal of interaction with senior weapons experts, so that my participation would allow me to observe them discussing, debating, and grappling with the knowledge loss problem.

The second phase of my fieldwork began when I left Human Resources in February of 1998 and joined the laboratory's Archives and History Programs. Lacking a security clearance, my interactions with the weapons experts I wanted to study were quite limited, and I could not contribute much to the Julin project. Since I had no access to the classified materials I would need for the Julin project, I conducted some unclassified interviews with retirees from the weapons programs. However, the History Programs requested and received approval for my security clearance, and I began the clearance process in the spring of 1998. At this point, I decided to use my own transition from uncleared to cleared status as a springboard for discussing the practices and dynamics of security at the laboratory.

The most intensive phase of my fieldwork began on February 26, 1999, when I finally received a "Q" level security clearance from the Department of Energy. Even before I received the security clearance, working in the Archives and being affiliated with the Julin project provided justification for my questions about the history of weaponeering at Los Alamos; and when I explained to weapons scientists and engineers that I was piggybacking a dissertation about knowledge loss on top of the Julin project, they generally offered to help me gather the data I needed. However, since I did not have a security clearance, they could only describe the Julin tests and their own work to me in a very general fashion, while most weapons-related documents were off limits to me.

The security clearance, then, was critical for both the Julin project and for my research, since it granted me access to areas of the weapons programs that had previously been strictly off-limits. With a Q clearance, I was able to move with relative ease through the office buildings where engineers and scientists work and to conduct interviews in which weapons experts used classified technical examples to teach me about their work.

In the end, my involvement with the Julin project proved extraordinarily valuable for my dissertation research, and not just because it got me a security clearance. Rather, the project put the knowledge loss issue into a far deeper historical context, one I would have lacked without being involved on the project. Working on Julin, I read classified nuclear test proposals, planning documents, and reports from Cold War weapons programs. I visited the Nevada Test Site four times, even spending one night on site in the dormitories that, during the Cold War, had bustled with dozens of Los Alamos staff members involved in fielding nuclear tests. Weapon designers and diagnostic physicists pulled out classified technical documents and used them to give me physics lessons, explaining the purpose and problems of particular experimental designs and data-collection methods. Gradually, I gained a sense for the rapid pace and rhythms of the testing program, for the complexity and of the laboratory's experimental process, for the camaraderie engendered while working round-the-clock to meet tight deadlines. I gradually came to appreciate how involvement in the Cold War design and test cycle reproduced the weapons community, reinscribing the affective ties that bound nuclear weaponeers to each other and to their work.

My experience with the Julin project not only gave me valuable insight into the social organization and integration of the Cold War weapons community; it allowed me to learn what experienced weaponeers thought about knowledge loss. I spent many hours listening to weapons experts discussing the knowledge loss problem and debating proposed remedies and solutions, which included formalized classroom programs, archiving projects and "knowledge preservation" initiatives like the Julin project, mentoring and training programs. Not infrequently, they would ask me if my anthropological training gave me any insight into the issue. Could I suggest creative ways to capture and impart their knowledge to newcomers? "Best practices" for identifying and preserving key skills? What was the best way to structure a mentoring program?

I dreaded these questions because I did not know how to answer them. At first, I would offer a vague explanation about anthropologists being more interested in studying culture change, as opposed to directly effecting it. Later, as I became more secure in my research, I learned to answer, simply, "I don't know," and then elicit their solutions to the problem. This tactic proved quite valuable in shaping my own thinking about the relationship between culture, change and knowledge at the laboratory. When I asked physicists and engineers to describe their ideas or approaches to knowledge preservation, the conversation invariably turned into a sort of career history interview, as they contrasted the present with their own recollections of learning: mentors, formative experiences, surprises, even failures.

Participant Observation

Gradually, I realized that, in focusing on the knowledge loss issue, I would have to answer several questions about learning and activity. After all, most of the senior weaponeers I met were insisting that it is difficult, if not impossible, to become a competent judge of weapon behavior without actually engaging in weapons research. Given that a great deal of learning takes place in classrooms, I often wondered why they were so adamant about the importance of experience in learning. Does this say something about weaponeering as a unique enterprise? Or perhaps their characterizations of weaponeering pointed to a larger issue about the way that humans learn by engaging in activity?

I decided to explore this question by arranging a period of participant observation in which I could watch novice weaponeers in some area of the weapons community learning a particular facet of weapons work. Of course, by the time I had arrived at Los Alamos, the Cold War nuclear design and test cycle had been defunct for five years; and short of time travel (unlikely even at Los Alamos), it would be impossible for me to arrange a situation in which I could watch novices learning the complexities of full-scale nuclear explosives. Nevertheless, under Science Based Stockpile Stewardship, Los Alamos continues to pursue experiments related to nuclear explosive behavior, albeit on a vastly different scale of activity than that which characterized the Cold War. In the context of these experiments, expert subcommunities still train their novices by having them participate in some aspect of experimental work, under the supervision of an expert mentor.

Initially, I thought I might arrange my participant observation among weapon designers in X Division. As the scientists who developed, refined, and finally certified experimental weapon concepts, weapon designers have traditionally occupied a central location in the weapons community. If were to understand weaponeering, I thought, then participant-observation in X Division would be required fieldwork. Moreover, expert judgment is extremely important in the design community, since designers are responsible for assessing and certifying weapons in the nuclear stockpile to the Department of Energy, the Department of Defense, and the military. Hence, worries about knowledge loss are particularly acute in X Division.

Perhaps serendipitously for my research, the Wen Ho Lee espionage case broke in March of 1999, and several of my colleagues warned me that the political fracas would make it difficult, if not impossible, to carry detailed fieldnotes about weapons designers outside the confines of X Division. Knowing that I was interested in watching neophytes in training situations, one of my interviewees, an engineer that I met at the Nevada Test Site, suggested that I contact the group leader in ESA's Weapons Engineering (WE) group, saying that this individual was worried about knowledge transfer and would be amenable to the presence of a researcher.

As a result, I spent most of my participant-observation time in the spring and summer of 1999 among assembly engineers and technicians in ESA Division. The assembly engineering community is addressing the "knowledge loss" issue with a structured mentoring program in which novices are paired with experienced engineers to work on small-scale high-explosive experiments. I watched senior and novice engineers in a variety of activity settings, including planning meetings for one of the laboratory's experimental programs, assembly review meetings, a safety exercise at the Nevada Test Site, and high-explosive assembly operations at Los Alamos. To round out my participant observation material, I conducted formal interviews with eight assembly engineers who had varying levels of experience in weapons engineering: the two most senior people had been trained at the Nevada Test Site during the Cold War and held leadership positions in the group. Another two of my interviewees had joined ESA after 1992, but their supervisors described both of them as "experienced," and had them actively involved in mentoring newcomers. Four of my interviewees were relative novices to the ESA-WE. In addition, I struck up informal conversations with other engineers and technicians as often as I could.

Participant-observation among assembly engineering community was the most exciting part of my fieldwork. For one thing, as I watched the assembly engineers solving problems with their counterpart engineers and assembly technicians, I realized that knowledge is emergent: absent a context that calls knowing selves into action, it is difficult – if not impossible – to fully appreciate the skills, understandings and tacit sensibilities embodied in any expert. Secondly, I came to think of expertise as synonymous with identity, in the sense that individual experts understand themselves at several levels: as having certain responsibilities and ties to other experts in the weapons community, as being responsible for fulfilling a particular stage of an experiment, and in relation to the material artifacts that they create. As a result of this experience, I came away with a new appreciation for the complexities of weapons engineering, as well as a deeper understanding of learning and knowing as ongoing processes embedded in context and activity (Chaiklin and Lave 1996).

I found that I could learn a great deal about the Cold War design and test cycle while watching present-day assembly engineering activities. The experimental projects that currently engage weapons engineers are similar to the high-explosive experiments conducted during the Cold War, insofar as they exercise and extend some of the same linkages, capabilities, and responsibilities. In this regard, the location of weapons engineers in relation to other experts in the weapons community, and the practices and activities of weapons engineering, have not changed so much that the discipline today is totally dissimilar to that practiced during the Cold War. Related to this issue is the Department of Energy's mandate to "maintain readiness" in case the United States should suddenly return to testing. This concern hovered in every interview I conducted and in every high-explosive assembly that I observed. Senior engineers described how they learned the craft of weapons engineering during the Cold War, and wondered if their novices would have the necessary skills to work with full-scale nuclear explosive devices. Novice engineers expressed curiosity about what it might be like to build a full-scale nuclear device for detonation in Nevada. As I observed assembly procedures, the assembly technicians would tell stories to each other, to me, and to novice engineers about "what things were like when we were still testing."

Throughout my fieldwork, as I went through the process of getting a security clearance, arranged interviews, visited the Nevada Test Site, watched engineers at work, I found that my own position and experience also provided a useful tool for making sense of my research problem. In many ways, researching the knowledge loss issue meant trying to understand how people learn to move fluently in, and contribute productively to, the laboratory's environment. As a neophyte ethnographer trying to feel my way around Los Alamos, my own experience of learning resonated strongly with my research questions. Reading Lave, Chaiklin and Wegner's writings about learning as a transformative, identity shaping process was particularly helpful in this regard: their discussions heightened my own awareness of the process through which I was becoming a member of the laboratory community; so that as I did my research, I found that I was experiencing firsthand many of the processes they described. I charted my experience in fieldnotes and diaries, describing what it felt like to be “new” at Los Alamos and examining the events and experiences that marked my increasing familiarity and ease with aspects of laboratory culture that initially seemed strange and perplexing. The exercise of documenting my trajectory of learning and belonging at Los Alamos helped me understand how newcomers to the laboratory gain fluency in the practices of a particular area of the weapons community.

Secrecy, Security and Fieldwork

In many ways, I think, my fieldwork was fairly standard ethnography: forty or so interviews, both tape recorded and not; participant observation, some of it occurring informally during my day-to-day life at Los Alamos and other periods formally arranged among weapon designers and weapon engineers; “paper” research in the laboratory’s Archives and the library, digging through records from the weapons programs, institutional documents, newspapers and magazines; and of course, volumes of fieldnotes throughout the process. At the same time, doing ethnographic research in a top-secret weapons laboratory made my data collection efforts rather cumbersome at times. Although I spend most of Chapter Two describing security and secrecy at Los Alamos, it is worth discussing how secrecy impacted my data collection methods, because security regulations presented an often intimidating set of fieldwork challenges in terms of recording, storing, and analyzing the data I collected during interviews and in participant observation sessions among the weapons experts I was studying.

Interviews were particularly difficult. During my fieldwork I conducted a total of forty or so interviews, with about half of those involving unclassified discussions. The unclassified interviews – most of which I conducted while waiting for a security clearance – were relatively simple: I asked people to meet me in convenient, quiet places, such as the back of LANL's cafeteria or library, in their homes, even meeting rooms in the Los Alamos County public library. Most of these interviews lasted two to two-and-a-half hours and consisted of general career history questions with very little technical information about the weapons programs. I taped nearly all of these interviews (with the interviewee's consent), created verbatim transcripts, and after cleaning out identifying characteristics, I imported a great deal of this data directly into my dissertation – as most ethnographers do.

However, roughly half of the interview data I used in my dissertation came from interviews I conducted after receiving a security clearance, under the Julin project. These interviews, which involved questions about classified aspects of the Julin test series, provided an invaluable source of data about the weapons programs. However, they were far trickier to arrange and conduct: for one thing, the laboratory strictly delimits places where staff can hold classified discussions. These places are usually offices and conference rooms in so-called "secure" areas where weapons personnel do classified work. Moreover, recording and transmission equipment of any kind is usually prohibited in these areas. The only place I could tape classified interviews was in my own office at the Archives, which are one of the few areas where classified interviews are allowed. Unfortunately, the Archives are also located across town from the main areas where most weapons personnel work. More often than not, the classified interviews I managed to tape in the Archives were conducted with retirees or part-time workers, since full-time staff members were often reluctant to take an afternoon from their research projects to drive across town and sit for a classified interview.

For the most part, then, I had to record my interviews by hand. But I could not take notes on just any pad of paper: instead, the laboratory requires that staff keep their classified meeting notes in heavy, hard-backed, leather-bound laboratory notebooks, the kind that most "bench" or laboratory scientists use for recording data. My notebook was covered with bright red stickers that read "PROTECT AS CLASSIFIED." When I took this notebook out of my office to an interview site, I had to wrap it in two manila envelopes, seal it with heavy packing tape, write my office address on the outer wrapping in case I lost the notebook en route, and drive a government-owned van to the interview site, since no classified information is supposed to be carried in private vehicles. When the interview was over, I re-wrapped and sealed the notebook, carried it back to my office, and locked it in a large, heavy file safe with a combination lock on the top drawer. I followed the same similar procedure when taking fieldnotes during participant observation in classified settings – among assembly engineers, for instance, or in meetings about upcoming experiments. Occasionally, when I attended classified meetings without my classified notebook, I took notes on a yellow legal pad; and when the meeting was over, I returned immediately to my office, stamped the notes "SECRET," and taped them in my classified notebook so I could not lose them.

Collecting and storing classified data was burdensome, but preparing it for use in an unclassified document was even more so. I had to request access to a special classified computer before I could type up any fieldnotes or interview notes taken in classified settings, and (of course) I could not take any of the resulting files of fieldnotes or interview data home to work with them. If I decided to use material from classified interviews or participant observation in a chapter draft, I had to write my draft on a classified computer system, even when I was fairly certain that the specific pieces of information I was using were not classified, or I was wording my draft in extremely general terms. This is because the laboratory requires staff to take formal training before they can officially make a distinction between classified and unclassified training, and I had not attended any of this training. Moreover, laboratory staff are warned to be particularly careful of something called the "mosaic effect," when several pieces of unclassified information come together to create a classified document. Given that I was working with a great deal of classified information in my interviews, this was a very real possibility, so anytime I was using classified material, I took pains to draft my ideas on classified machines.

After I finished drafting a particular section of a chapter, I carried it to the document reviewers in the security office, who read through my draft and stamped it unclassified. At that point, I was free to put the material on my unclassified computer system; but because security prohibits the direct electronic transfer of information from a classified computer to an unclassified computer, I had to re-type the hardcopy onto my unclassified machine, where the rest of my chapter sat. Later, the same security reviewer checked the entire chapter again and marked it unclassified, at which point I could release it to my committee for review. As inconvenient and time consuming as this convoluted process was, it protected me from inadvertently misplacing, losing, disclosing, or releasing classified information, so I was quite willing to follow it.

OVERVIEW AND SUMMARY

In the following chapters, I describe what I have learned about nuclear weapons scientists and engineers during my two-and-half year encounter with weapons scientists and engineers at the laboratory. Although each chapter covers a unique set of subjects, all explore, in varying degrees, the relationship between knowledge, power and identity. Throughout my fieldwork, I was interested in exploring learning as social process that takes place through engagement with other people, and with various aspects of the physical world, within locally meaningful settings – in this case, a nuclear weapons laboratory. In a larger sense, I wanted to understand how the process of becoming a competent practitioner of some area of nuclear weaponeering, be it engineering, design physics, etc., is simultaneously the process through which the practices, understandings, beliefs, and activities of the weapons community as a whole are reproduced.

Chapter Two, "Isolated in a World of Threats," challenges the narrow perspective of European sociology of science by pointing out that the pursuit of nuclear-weapons related knowledge only makes sense when one understands the "sweeping worldview" of weapons experts at Los Alamos. In other words, if individuals are to engage fully and meaningfully with the most mundane technical aspects of laboratory's nuclear weapons mission, they must come to understand nuclear weapons are morally, politically and socially sensible because they deter various threats – from terrorists, from rogue nations, from ideological adversaries like China. Using my own experience in getting a security clearance as a starting point for my analysis, I explore how immersion in the practices and principles of security at Los Alamos is a crucible for the reproduction of this worldview in the larger laboratory community, and I explore some of the surprising tensions and contradictions that emerge when scientists are asked to work under the demanding conditions of secrecy as practiced at Los Alamos.

Chapter Three, "Cycles of Cold War Knowledge Production," discusses the laboratory's historic role in producing confidence in the nation's nuclear deterrent, focusing in particular on the experimental cycles that characterized the Cold War environment of the weapons community. Throughout my fieldwork, I interviewed many Cold War weapons experts, asking them to describe their particular field of weaponeering, its relationship to other weapons-related disciplines, and how they became experts in their particular area. I also did a great deal of archival research on weapons testing, so I could learn as much as possible about the social organization and scientific activities of the Cold War weapons community. I focus particularly on the social functions of the design and test cycle, exploring how experimental activity was an engine for the ongoing integration of the laboratory's many "ways of knowing," including various branches of engineering, physics, radiochemistry, craft work, etcetera. In this sense, the Cold War cycles of scientific activity at Los Alamos not only produced a massive body of knowledge about nuclear weapons; they reproduced a diverse but integrated community of knowing selves dedicated to maintaining the nation's nuclear deterrent. In this chapter, I pay particular attention to Cold War constructions of time and place in reproducing the weapons community and its knowledge. This theme re-emerges in Chapter Five, in which I argue that the knowledge loss problem can be strongly attributed to shifts in the temporal rhythms of Cold War weaponeering.

Chapter Four, "Activity and the Social Reproduction of Expertise," is largely based on my spring and summer of participant-observation among weapons assembly engineers. I open the chapter with a brief history of assembly engineering at Los Alamos, pointing out that today's process is not entirely dissimilar to that practiced during the Cold War. Assembly engineers work quite closely with weapon designers and diagnostic physicists, but when they are actually putting together a high-explosive experiment, they spend most of their time working with a group of highly skilled, blue-collar assembly technicians in a sister ESA group. Using interview material and participant observation data, I describe the interactions that assembly engineers have with different parties in the weapons community, and provide a detailed discussion of the process of putting together a high-explosive experimental device. The second half of the chapter discusses the social reproduction of the assembly engineering community, mapping the stages through which novices must pass before they are considered competent members of the community, and discussing the role of activity in shaping the "knowing selves" of novice engineers.

Chapter Five, "Weaponeering under the New Paradigm," describes how the shift from testing to SBSS has impacted the laboratory's ability to reproduce Cold War "ways of knowing" in the latest generation of weapons experts. I doing so, I describe the "official" factors that the laboratory has identified as contributing to knowledge loss, including the closure of the DOE's production facilities, an aging workforce, and an "enduring" stockpile of nuclear weapons. However, I explore these factors through an anthropological lens, arguing that worries about knowledge loss must be understood as a temporal matter, insofar as the end of the Cold War halted the cycles of testing that consistently renewed the laboratory. I then explore the impact of SBSS on the weapons community: while some groups are actively working to develop "new ways of knowing" the stockpile, others are engaging in a symbolic battle to reassert the value of their knowledge to the laboratory's mission. Although the weapons community's landscape of practice has changed immensely since the end of the Cold War, I argue that many elements of weapons culture have survived the transition from testing to SBSS and are thriving in the new generation of weapons experts.

I close this dissertation with a concluding discussion entitled "Knowledge, Identity and Practice." In this coda, I review the connections between knowledge and identity in various situations: learning the practices of security, working on a nuclear test, becoming an assembly engineer, worrying about the loss and displacement of one's knowledge. In doing so, I emphasize the laboratory as an institution whose members reproduce an ecology of danger in which nuclear weapons remain valid arbiters of threat, the Cold War's end notwithstanding.

“Countries don't have friends. They have interests.”
- Slogan from a security awareness poster,
DOE offices, Las Vegas, Nevada

CHAPTER TWO: ISOLATED IN A WORLD OF THREATS

“Silence,” says historian Jon Hunner, “is difficult to enforce on humans, we who speak even before we walk” (1997: 42). Yet silence, ironically, is a quite salient feature of American democracy: the federal government generates a staggering amount of classified information, creating a “secret world” adjacent to the public sphere in which most of us live our daily lives (Gusterson 1996: 68). The Department of Energy (DOE) alone is custodian to over 200 million pages of classified paper documents, as well as an unquantified amount of information stored electronically on its computing systems (Panofsky 1999: 58). Because of the laboratory’s longstanding involvement in nuclear weapon design and development, a great deal of this information resides at Los Alamos.

The laboratory goes to great lengths to ensure that its employees can and will keep secrets within the safe confines of the classified world. Secrecy, and the silence that maintains it, are among the most important practices that newcomers must acquire if they are to become active, engaged members of the laboratory community. However, security is more than just an elaborate set of rules that discipline members of the workforce into silence. Gradual immersion in the laboratory’s culture of security, and the mastery of its practices, turns individuals into knowing members of an expert community that is perpetually targeted by the perilous desire of other nations. The laboratory’s institutional worldview is marked by a preoccupation with its role in maintaining the stability of the nation-state in a world of threats.

In the following pages, I provide a moderately thick description of the most basic aspects of security at Los Alamos – data classification, the demarcation of physical and cyber spaces into secure places, and the clearance system. I also describe how newcomers are gradually transformed into "marked selves" as they engage with different aspects of the laboratory's security culture. Throughout this discussion, I argue that as laboratory workers practice security, they are quite literally putting into practice a particular, historically situated vision of the world as competitive, anarchic and fundamentally insecure; a world in which nuclear weapons enhance rather than undermine efforts at stability and peace.

THE RULES OF SECRECY

Ethicist Sissela Bok defines secrecy quite simply as the "intentional concealment of information" (Bok 1982: xvi, quoted in Chalk 1985: 29). At Los Alamos, secrecy refers to the goal of restricting access to the DOE's secret information – which includes anything deemed significant to national military purposes and national defense – while security is the blanket term for a portfolio of individual and institutional practices whose purpose is the maintenance of secrecy. The primary laws that control secrecy in the DOE are derived from the Atomic Energy Act of 1954, which created the Atomic Energy Commission (AEC) and subsequently mandated it to prevent the unauthorized disclosure of nuclear weapon related information and materials. The AEC was the predecessor to the Energy Research and Development Administration (ERDA), which became the Department of Energy in 1977. Today, the DOE interprets the Atomic Energy Act, as well as any applicable Executive Orders regarding security that have come from the

President, and codifies its interpretations in a set of DOE orders that govern information and materials management at its facilities.

Security measures ensure secrecy in two ways: some deny opportunities for harmful information transfers, while others interdict or prevent such transfers (Panofsky 1999: 58). Many practices serve both purposes: marking documents as "Secret" sends a clear message about where those documents belong and who is allowed to handle them, simultaneously denying uncleared personnel access to those documents and minimizing the chance that they will be transferred to uncleared areas.

At Los Alamos, the Security Division, or "S Division," is charged with interpreting DOE orders for enforcement in Los Alamos facilities, although the DOE regularly conducts security audits to ensure that the laboratory is complying with security orders. In its ongoing battle to maintain the boundaries between secret and open information, the security machine at Los Alamos constantly monitors, harasses, reminds, observes, encourages, reprimands and re-trains laboratory staff members in the ways of secrecy. Staff members in S Division define, maintain and enforce a strict bureaucratic system that orders relationships among information, people and places at Los Alamos.[1] Different groups in S Division are assigned separate but interlocking responsibilities related to security. For instance, one group is responsible for classifying data, another demarcates physical spaces where secret knowledge is created, stored and protected; another sets up cyber-boundaries to protect vulnerable computing systems, while still

[1] S Division also has a group that is responsible for the monitoring and control of "Special Nuclear Materials" - fissile materials such as uranium, plutonium, and tritium, all of which are used in nuclear weapons. In this discussion, I focus on information rather than nuclear materials: although the latter are as closely controlled as the former, I never participated in any of the training related to materials security.

another ensures that forbidden spaces and knowledge remain off-limits to individuals who have not yet undergone the purifying ritual of a security investigation.

S Division also enforces the laboratory's security regulations, investigating and adjudicating possible security breaches. Enforcement staff can issue reprimands and punishments to employees who break security rules, and may even recommend legal action for egregious violations of secrecy practices. Failing to respect the boundaries between secret and open information can result in disciplinary action, the loss of one's job, even criminal charges, not to mention the loss of collegial trust and respect. Indeed, violating security procedures is such a serious offense that no one wants to be closely associated with a staff member accused of treating secret information carelessly.

Data Classification

In the secure universe of the laboratory, classification is the core practice of secrecy. The classification system, which is common to all DOE facilities, can be thought of as a two-axis matrix that combines levels of secrecy with categories of information. Classified data is placed in one of two levels, Confidential or Secret. Each level of classification has a particular color associated with it – green marks anything that is Unclassified, blue is used for Confidential information, and red marks Secret data.

Levels of secrecy are assigned according to the perceived significance of the information for national security: Confidential information includes data whose "unauthorized disclosure could reasonably be expected to cause damage to national security," while Secret data could cause "serious damage" to national security. There is a Top Secret category – "exceptionally grave damage to national security" – but it is rarely used in the day-to-day workings of the laboratory. Even people cleared to see secret information must undergo additional training and demonstrate a "need-to-know" in order to get access to Top Secret information. After I received my security clearance, I worked frequently with Secret information, but never encountered anything classified "Top Secret." Occasionally, however, I heard people refer vaguely to "black" projects, the contents of which I never heard disclosed, even when I was in meetings that required a security clearance for attendance.

Classified information, both Secret and Confidential, is further categorized into three separate categories that mark types of information. These categories include Restricted Data, Formerly Restricted Data, and National Security Information. Initially defined by the Atomic Energy Act of 1946, Restricted Data (RD) includes any information that "concerns the design, manufacture, or utilization of atomic weapons, the production of special nuclear material, or the use of special nuclear material in the production of energy" (Los Alamos National Laboratory 1999). Restricted data are "born classified," meaning that the laboratory staff must maintain secrecy around anything that contains this information from written inception (in a laboratory notebook, a written document, a web page, e-mail) until a formal review process determines otherwise. Only personnel with DOE clearances are allowed access to restricted data.

Formerly Restricted Data (FRD) are also born classified. The term "formerly" implies that the information has been reviewed and downgraded, but this is not the case. The term was coined when the Atomic Energy Act was revised in 1954, to designate information that was at one point restricted to the Department of Energy employees, but has been released to the Department of Defense. FRD is highly classified, but has a wider circulation among DOD and DOE employees, and includes weapon information that the DOD requires to deploy and operate stockpiled weapons. Lastly, National Security Information (NSI) includes data that are not born classified, but are treated as such because they have bearing on national defense or US foreign relations within a particular program or context at Los Alamos.

Just because a piece of information is unclassified does not mean that it is publicly releasable. There are several categories of unclassified sensitive information, such as UCNI (Unclassified Controlled Nuclear Information), or OUO (Official Use Only). Sensitive unclassified information has a wider range of distribution; for instance, among federal employees, Congressional staffers, members of the armed forces, even foreign diplomats, as long as the DOE agrees that they have reasonable cause for requiring access to the information (Los Alamos National Laboratory 1999).

The laboratory's classification staff use secrecy levels in combination with categories to label documents, computer disks, even computers themselves according to the highest level of information contained within. A document can be classified into any of six primary categories: Secret Restricted Data (SRD), Secret Formerly Restricted Data (SFRD), Secret National Security Information (SNSI), Confidential Restricted Data (CRD), Confidential Formerly Restricted Data (CFRD), and Confidential National Security Information (CNSI). Staff are also required to safeguard sensitive unclassified material, like UCNI and OUO, with the same care they use in handling classified material. In addition to these main categories for sorting data, there are separate, more specific categories known as "Special Access Programs," (SAPs), most of which cover information about foreign intelligence or particular weapons programs. Metaphorically speaking, SAPs are simply another set of bins that further sort data into specialized categories beyond the primary classification system, and that restrict access only to people who work with this information in the course of their jobs. One example of a SAP might be Department of Defense weapons-related data that is processed in conjunction with Department of Energy information by DOE personnel at facilities like Los Alamos.

Rules for Access

Officially, getting access to classified data involves fulfilling two requirements: holding an L-or a Q-Clearance, and demonstrating the "need-to-know" a particular area of information for one's job duties. People who lack security clearances are only allowed access to unclassified, non-sensitive information. An L-Clearance grants access to Confidential RD, FRD, and NSI, and Secret NSI and FRD, but only a Q-Clearance grants the holder full access to Secret Restricted Data, which is the most common kind of classified information in the laboratory's weapons programs.

Every employee at Los Alamos is required to display a security badge that indicates the level of security clearance that the wearer holds. Security badges are a basic marker of membership in the laboratory community, so basic that the laboratory frequently refers to members of its workforce as "badgeholders" rather than "employees." The laboratory requires that employees keep their badges in plain sight, worn between the waist and the neck, with the front of the badge facing outward so that the identity and security clearance information is plainly visible to other people.

When I first came to Los Alamos, the Department of Energy issued green badges to all employees who were American citizens, and red badges to any foreign nationals. In this system, clearance level was indicated by a number in the upper right-hand corner of the badge – a "1" indicated an uncleared employee, a "2" indicated an L-Clearance, which is a middle-range security clearance that opens access to some facilities and types of information, while a "3" indicated a Q-Clearance. When the DOE's weapons laboratories were shaken by allegations of lax security in 1999, the badging system was one of the first practices to come under Congressional fire, with critics arguing that colored badges were more efficient indicators of security clearance than numbers. Today, the green badges with numbers are gone. In their place, the DOE recently instituted a system in which a gray badge indicates the wearer has no clearance, a yellow badge indicates an L-Clearance, and a blue badge means that the wearer has a Q-Clearance. Because I have a Q-Clearance, my badge is blue.

However, a clearance does not provide blanket access to all classified data in the laboratory. A system of fifteen administratively controlled "sigmas" breaks the entire body of classified nuclear weapons information into separate "compartments," so that in addition to a clearance, employees have to request specific sigmas if they require access to nuclear weapons data. Getting a particular sigma or a set of sigmas requires need-to-know approval from one's team or a group leader, who reviews tasks and job responsibilities and assigns each individual the appropriate sigmas. Different sigmas mark different kinds of information: for example, sigma 1 includes anything related to the "Theory of operation or complete design of thermonuclear weapons or their unique components." Some sigmas travel in groups: someone who holds sigma 1 has automatic access to sigmas 1-10, although someone who held sigmas 2, 3, 5, and 10 would not necessarily be granted access to sigma 1 information.

People at Los Alamos guard their information closely, and it is common practice at meetings and in discussions to announce which sigmas are required for attendance. Before I could gather certain kinds of data for the Archives' history projects, or sit in on classified meetings for my own research, people asked me to list which sigmas I held. It is also common practice to call S Division and verify sigma authorization before granting access to weapons information. People who lack specific sigmas are usually asked to leave meetings when restricted information is under discussion. In addition, most classified weapons related documents are marked with sigma numbers to prevent readers without a need-to-know from accidentally accessing restricted reports.

THE GEOGRAPHY OF SECRECY

Strict boundaries around information, people and spaces maintain the separation between open knowledge and secret knowledge by defining and protecting vulnerable points from exploitation. The boundaries that separate classified from unclassified information create a complex geography of secrecy at Los Alamos, in which facilities are categorized as "open" or "secure" depending on the kind on the kind of information that belongs in those places. Secure areas are described as "behind the fence" – an expression that is frequently metaphorical, as not every classified area is surrounded by fencing, though they are all marked by heavy security.

Physical Space

The central area of the laboratory, Technical Area 3, provides a good example of the way the laboratory segregates spaces in buildings to create a complicated geography of classified places where different kinds of secret information are created, reviewed, worked with, and stored. Because the main administrative buildings are located at TA-3, it is a central point of reference for people within and outside the laboratory. Several key public places – the Oppenheimer Study Center, the Otowi Cafeteria, the Badge Office, the Public Affairs Office, the Occupational Health clinic – are located on TA-3.

However, nestled among these public areas, surrounded by barbed wire and monitored by electronic access devices and guards, lies the sprawling Administration Building. Lab employees usually refer to it as the "Admin Building" or by its facility number, SM-43. This is an enormous, multi-story structure with several wings, floors and hundreds of offices that are home to the Director's Office as well as many of the laboratory's division offices and group offices. Although SM-43 is considered the heart of the laboratory, it is a secure area, and employees who lack a security clearance are only allowed to visit the Administration Building under escort. Occasionally, the laboratory holds unclassified, public symposia or presentations in the Administration Building's auditorium, and during these events the checkpoints are pushed back so that uncleared personnel and the public can visit the auditorium without getting access to the rest of the building.

A combination of guards and electronic access devices prevents unauthorized persons from entering the Administration building. Camouflage-clad guards employed by Protective Technologies of Los Alamos (also known as PTLA or the ProForce), monitor access points. These guards ask people passing to remove their badges from their necks so that they can examine them for forgeries. After verifying that the badge is genuine and that it belongs to the person requesting access, the guard returns the badge, opens the gate or door, and allows the person to pass. Guards check the badges of people entering and leaving any exclusion area. The first time I walked into an exclusion area by myself, I followed proper procedures with the guards on the way in, but breezed right by the guard station on the way out. One of the ProForce guards called me back and sternly warned me, "Never walk by us without giving us a chance to verify your badge," and proceeded to explain that they strictly monitor traffic entering and leaving secure areas. I felt rather embarrassed, since it was lunchtime and several people walking in and out of the Administration complex heard the guard lecturing me.

Not all areas have guards posted to them, and even areas that are guarded during the day are simply locked and alarmed at night. In areas where there is no guard, or after working hours, access to secure areas is controlled by electronic locks that verify identity before allowing people to enter the area. These are usually "badge readers" with accompanying hand-print readers or keypads for PIN numbers. These devices control access to narrow turnstiles or electronically locked vestibules through which only one person can pass at a time.

Entering a secure area, whether a building or a fenced and gated space, requires taking the badge off, swiping the magnetic strip on the back through the badge reader – much like a credit card machine – and verifying one's identity, either with a PIN number or by placing one's hand on a palm reader that checks the outline of one's hand. Once the access machine verifies a match between the identity on the badge, the PIN number, and/or the outline on the hand reader, the turnstile or security door gives a loud click, signaling that the lock has been released for one person to pass. Leaving electronically controlled areas does not require re-entering one's badge, PIN or handprint. On the way out, each turnstile is equipped with an enormous red button; hitting it with one's palm releases the turnstile. Sometimes these electronic access points also have a guard assigned to them during the day, monitoring traffic through the area and checking paperwork for uncleared visitors who are being escorted into the area by someone with a clearance.

Even in heavily guarded places like the Administration Building, there are spatial restrictions around classified information that further divide secure areas into smaller zones of increasingly higher secrecy. Employees with an L or a Q clearance might be able to walk freely past the guards, but they do not have access to every part of the building. The geography of secrecy creates a series of boundaries designed to keep knowledge restricted to specific areas of the building. Even secure spaces are never homogeneous because classified information itself is partitioned into different areas of secrecy, and access to these areas is restricted to clearance holders who have need-to-know for a particular area of knowledge. Some of these borders are fixed, while others can be erected or removed depending on the kind of work being performed in a particular area.

The borders around X Division, where the laboratory's weapons designers are located, are one example of a fixed boundary. Electronic badge readers limit access to the floor where X Division's offices are located. Getting access to X Division requires a Q clearance, as well as a) a pre-arranged escort from X Division or b) special need-to-know access permission from the X Division Office. If granted, X Division enters the approved badge into the computer security system that controls the badge readers and electronic doors around the second floor. This system records the name, time of entry, and point of access for everyone who swipes a badge through any of the readers, so that

X Division has a constant record of traffic into its area. Anyone without an escort or access privileges is locked out of the area, and moreover, X Division has posted large red signs warning its employees not to allow "tailgaters" into the area, lest they be disciplined for a security infraction. I never requested access permission, so that even with a Q clearance and all 15 sigmas, I was never allowed to walk though X Division unescorted.

Within the secure spaces of the Administration Building, rules for protecting classified material also create a mutable geography of secrecy, in which employees can temporarily erect or remove borders around information, depending on the kind of work they are performing in a particular area. Security limits the flow of information to employees with a clearance and a need-to-know, and individual workers are required to guard classified information from anyone who does not have justifiable access to their data. Most offices are equipped with a locking, fireproof safe for storing classified documents and computer disks. Only people with authorized access have the combination, and every time a worker opens or closes her safe, she logs her name, the date, and the time on a small open/close log sheet attached to the safe. This log sheet is randomly checked by security guards who walk through the building after hours, checking employee safes to make sure that owners are securing classified data and keeping a record of access to that information.

Taking classified material out of storage and placing it on a desk or a table transforms the office space into a secure area that requires watchful safekeeping; removing the classified from the open spaces, replacing it in the locking safe, likewise returns the room to a less heightened state of security. When working with classified, employees in the Administration building routinely post signs on their office doors notifying visitors that their office space is temporarily restricted to individuals with a Q-Clearance. Employees are warned that they must never, ever leave classified material unattended: if they go to the rest room, they must find someone with a similar clearance to watch the room for them, or they must lock up the information.

Cybersecure Places

Just as security practices create physical places where classified information does and does not belong, it also weaves boundaries in cyberspace, creating a series of cybersecure places where classified information may be created, stored and processed electronically. There are three computing "partitions" at Los Alamos: green, blue and red, in ascending order of classification. The partitions refer to different computing networks, although single user, stand-alone systems may also be designated green, blue or red, depending on the kind of information that people process on them.

The "green" partition, also known as the open network, is directly connected to the internet and serves as the interface between the laboratory's computer networks and the outside world. This is the area where outsiders may freely access open areas of information: the laboratory's public web page, for instance, or the library server. It is a small cyber-place, since only twenty percent or so of the laboratory's unclassified computers and servers are located on the green partition.

The "blue" partition, or the protected network, is the core of the laboratory's unclassified network, the place where most employees perform unclassified computing tasks, such as checking e-mail, drafting unclassified papers, and maintaining employee records. Although all the information on this network is unclassified, the laboratory does not allow any direct connections between the blue partition and the internet to minimize the possibility of hackers breaking into the network, and to prevent the accidental compromise of sensitive unclassified information – personnel histories, for example, or patent proposals – to the public.

Lastly, the “red” partition is the most sensitive area of computing at Los Alamos, separated from both the blue and the green networks by an “air gap,” meaning that there are no hardwire connections that link the red network to the blue or green networks. A firewall also prevents unauthorized access to systems on the red partition. Employees are required to perform all classified computing tasks, regardless of secrecy level, on computers that are connected to networks within the red partition, or on stand-alone computers that are approved for classified processing. Of course, getting access to a classified network requires a security clearance. But it also requires managerial approval, special classified computer training from the group’s security personnel, and several signed affidavits stating that the user understands and agrees to obey all regulations that apply to a particular classified network.

The rules governing classified computing are similar to the ones that apply to classified documents, except that they are additionally designed to prevent employees from (accidentally or intentionally) transferring classified information across the red partition to the blue or green networks. Indeed, the laboratory is very clear about maintaining the separation between classified and unclassified computer systems. Classified computers and all peripherals are labeled with red and white security tape or small placards that read “PROCESSING SECRET RESTRICTED DATA.” Not infrequently, classified computers are equipped with removable hard drives, labeled

"SECRET RESTRICTED DATA" and stored in a locked safe when the user is not processing classified data.

Just as employees cannot store classified information in an open area of the laboratory, neither can they have a classified system in an office that is not approved for classified computing. Most offices in the Administration building, for instance, are labeled with a large sign that indicates whether or not the space is approved to house a classified system. A very few offices are approved to house both a classified and an unclassified system in the same space, but they must be physically separated by a designated number of feet, with totally separate hard connections to their respective networks. Each computer requires its own dedicated and incompatible peripheral equipment: for example, if one computer has a Jaz backup drive, then the other must have a Zip drive, to prevent accidental or deliberate transfer of information across the partition. Moreover, any information that is created or stored on a classified system is by default considered classified at the highest level of secrecy applicable to the system, regardless of content, until a trained classification expert reviews it and changes the classification level.

As is the case with classified documents or discussions, working on a classified computer can temporarily heighten the level of security in an office. All jacks that hardwire individual CPUs to the red partition are hidden in lock boxes that are closed and secured when the classified computer is not in use. A researcher in the Admin building who is working on a classified computer must physically re-connect her CPU to the network every time she processes classified information, and disconnect it when she is finished. She also has to post a sign on her door warning passers-by that she is processing classified data, so that only people with the appropriate clearance will attempt to enter the room. She cannot leave her classified system running while visiting another office, or getting coffee, even if her office door is locked. If she cannot find a similarly cleared officemate to watch her office, she must shut down the classified system, remove the drive and lock it in a safe, lest she face a security infraction.

Secrecy and Cleanliness

Given the almost ritual emphasis that security experts place on boundary maintenance, readers with a background in anthropology may be reminded of Mary Douglas' classic definition of dirt as ambiguity, anomaly, matter that "must not be included if a pattern is to be maintained" (1991: 40). Indeed, security discussions at Los Alamos often use metaphors juxtaposing cleanliness and dirt to emphasize the importance of keeping order in the Laboratory's secure environments. A case in point: my involvement in the Archiving Operation Julin project took me several times to the Department of Energy Offices in Las Vegas, Nevada. This office provides oversight for all operations at the Nevada Test Site, and maintains a vault for many of the drawings, logs, memos, reports and such generated around experiments and projects located at NTS. I expected to find quite a bit of material about the shots I was researching for the Julin project, and sure enough, the DOE archivist answered my request for documents by pulling several cubic feet of dusty files off his shelves. "This ought to keep you busy for a while," he said as we hauled them upstairs to my cramped guest cubicle.

In digging through those documents, looking for information about various aspects of the tests I was researching, I came across some of the activity logs that NTS staff maintained as they prepared to execute nuclear test shots. Among other things, notes in these logs made reference to setting up "secure areas" around the hole where the shot would be executed, to minimize the possibility of disruptive "intruders" coming in from "offsite uncontrollable areas," beyond the boundaries of the test site. As the test date drew closer, the logs called for increasingly frequent "sweeps" to make sure that the area near the shot was "clean," so that security could gather and move intruders and test "interrupters" – a category that seemed to anything from wild horses and coyotes to terrorists and antinuclear protestors. In addition, security staff frequently wrote down jokes that they made during the course of test preparations. For instance, I came across the faux chemical notation H_3NO, which appeared repeatedly in discussions about sweeps and test security. As I read through the logs, I discovered that H_3NO was NTS security shorthand for some common invaders: "No Hippies, Hunters or Herders" on the test site prior to executing a shot.

I do not think it is any accident that NTS staff use metaphors of cleanliness to describe security practices on the test site. As the acronym implies, the "hippies, hunters and herders," of H_3NO are, quite literally, discordant elements from "offsite uncontrollable" areas. These elements do not fit into the recognized and accepted pattern of test operations at NTS, and as such, they are a threatening form of anomaly, a kind of "dirt" to be removed in the security "sweeps" that take place with increasing frequency before a test is executed.

However, the Nevada Test Site was not the only place where I encountered a vocabulary of security filled with metaphors for cleanliness and contamination. As I have discussed, the laboratory is home to an extensive system of rituals – classification review, labeling documents, security clearances, and the like – for maintaining boundaries between secret and open categories of knowledge. Within this system, the "classified" category is the most forbidding one, in the sense that breaking the boundaries of secrecy causes enormous disruption of the prescribed order of knowledge. Predictably, pollution behavior ensues when classified information is found where it does not belong – for instance, when an e-mail containing classified information turns up on a "green" system. People scramble to re-establish the proper order of information: the parties who suspect a transgression notify S Division, which reviews suspect data to determine if it is truly classified. If so, the computer, possibly the entire computer network, are labeled "contaminated," and computer experts begin tracing the electronic path of the information to determine the source of the transgression. The contaminated cyber-places are "sanitized," meaning that technicians remove all traces of the classified material from unclassified places. S Division determines where the information traveled – was it printed? Distributed? – and gathers it to be destroyed or put back into its proper place.

Secrecy is so powerful that it can be contaminating even when no classified material is actually transferred to an open realm. Indeed, the mere possibility of a transgression can affect the classification of an item. For instance, inserting an unclassified computer floppy disk into a classified computing system automatically turns the floppy into a piece of classified material, regardless of whether any information was copied onto the disk or not. Similarly, any document created on a classified computer – regardless of content – is automatically classified, until S Division staff review its contents and designates it as unclassified.

Not infrequently, themes of classification, contamination, and secrecy provide material for jokes. For instance, in 1999 attended a classified meeting that was disrupted by the discovery of a dead mouse on the floor right next to the podium. Hantavirus is endemic in New Mexico and because it is spread through contact with deer mice and other rodents, the laboratory has a special team for rodent removal – the "Mouse Patrol" – that disposes of dead mice and fumigates for the virus. The man who had organized the meeting called Mouse Patrol and was about to continue with his discussion when a voice from the back of the room asked, "Wait, before we get started, does the mouse have a Q-Clearance?" This joke was greeted with chuckles. Later, as the Mouse Patrol left the meeting with dead rodent sealed in a small box, someone else joked loudly, "Better label that mouse! This is a classified meeting!" and as people laughed, another person answered back, "Yup, that rat's got classified all over it. It's become one of those secret rats."

Jokes about intrusive hippies and classified rats notwithstanding, people at Los Alamos take security very seriously. Newcomers to the laboratory are taught that secrecy is of paramount importance to national security, and must quickly master the practices of keeping information secure if they are to become fully vested members of the institution. In this sense, security is more than a set of arcane practices designed to keep secrets within the safe confines of the laboratory: it is a crucible in which new identities are shaped and linked together to create a knowing and watchful community.

SECRECY AND THE SELF

Ethnographers who study knowledge acquisition emphasize that learning must be understood as a process of identity formation: "Learning," write Lave and Wegner, "involves the whole person; it implies not only a relation to specific activities, but a relation to social communities – it implies becoming a full participant, a member, a kind of person" (1991: 53). At Los Alamos, neophytes must master the practices and understandings of secrecy if they are to become fully vested members of the laboratory's secret world, in which the institution's core mission resides. This process of learning is simultaneously a process of identity formation: as Hugh Gusterson has noted, secrecy is the "anvil upon which the identity of new weapons scientists [at Lawrence Livermore] is forged" (1996: 68).

But what *kind* of person does secrecy create? Gusterson's selves belong to a Goffman-esque "total institution" that invades, and scrutinizes, and claims the right to monitor their private lives in the name of national security. They are members of an segregated elite whose beliefs and practices are protected from critical public scrutiny by federally-enforced secrecy. Perhaps most saliently, Gusterson's subjects live a compartmentalized existence, drawing a sharp and irrefutable boundary around their work-selves, so that they are prevented from introducing forbidden knowledge into other realms of public and private experience. Livermore's weapons experts learn to move carefully between work and family, navigating a cognitive and emotional safety zone that separates the mutually exclusive worlds of the laboratory and the home. In doing so, they reinscribe the "putative separation between public and private spheres… [that is] one of the core structural features of American life" (1996: 94).

The selves I observed at Los Alamos exhibit many of the characteristics that Gusterson describes: they are self-monitoring, deferential to government scrutiny; and key details of their work remain largely sheltered from public criticism. However, unlike Gusterson, what I found most striking about security at Los Alamos is the way it inculcates in its practitioners a strong sense that their affiliation with the laboratory marks them as targets for the hostilities and desires of various enemies. In this sense, the formation of a secret, knowledgeable self can also be seen as a community-building process that draws individual subjectivities together, forming a culture whose worldview is characterized by wariness, suspicion, and a looming sense of threat.

The Marked Self

The formation of a knowledgeable, secret self takes time; it does not just blossom into existence when the laboratory grants a security clearance. From their first day of General Employee Training – which is the laboratory's two-day orientation for new employees – badgeholders learn that they are possible targets for terrorism, espionage, even the antipathy of the general public. "Don't wear your badge in public. It advertises that you work for the laboratory. Even if you do not have access to classified information, people might try to get information about Los Alamos from you. Wear your badges on laboratory-operated property and nowhere else," our trainer told us, sternly. The next day, another trainer reinforced the message. "There are a lot of people in Santa Fe who don't like Los Alamos. Don't make yourself a target for their dislike."

The message about guarding one's laboratory self from a hostile world resonates through people's careers at Los Alamos. Much later in my fieldwork, I interviewed an engineer who told me that her favorite tee-shirt commemorated a successful Los Alamos engineering project. The back of the shirt listed all the team members' names. "I never, ever wear that shirt in Wild Oats," she told me, with a serious smile. "I don't want anyone to know that I work at Los Alamos, or to know the names of any of the people that I work with. I don't want to put myself or any of my team members in jeopardy."

With a few exceptions – such as DOD employees who hold a security clearance, or DOE employees transferring to Los Alamos from other secure DOE facilities – most newcomers arrive at Los Alamos without a security clearance. Depending on their career trajectories, these newcomers might never need one, since many of the laboratory's work groups do not deal on a routine basis with classified information. For instance, a biologist pursuing genome research in the Biosciences Division may never need a security clearance, as long as she continues to work with unclassified data. Similarly, a technician joining a physics group in an unclassified area might not need access to secure information for his work, nor would a compensation and benefits analyst joining Human Resources. Many people at Los Alamos pursue research, administrative, and blue-collar careers in unclassified areas, and do not get a security clearance because their jobs require no access to classified data or to secure facilities such as the Administration building.

When I came to Los Alamos in 1997, I had neither a clearance nor the promise of one. Being uncleared was only a minor annoyance because none of the work assigned to me involved classified information. Most of the people I was interviewing for the mentoring program were happy to meet me in the laboratory cafeteria. Moreover, my office was located in an "open" or an "uncleared" building where no one worked with classified data. Security was not noticeably more stringent there than it had been at my office at the University of New Mexico, though everyone was required to wear an identification badge in plain view. If I lacked a security clearance, so too did most of my colleagues, because their work did not require access to classified materials.

Still, because staff at Los Alamos do classified work in so many areas of the laboratory, it is much easier to move freely from place to place with a security clearance. As an uncleared person, I was never allowed to visit offices in buildings "behind the fence." However, the problem goes beyond mere physical access. Nuclear weapons programs are the core of the LANL's mission, and remaining uncleared ensures continued peripheral status vis-à-vis the laboratory's most powerful projects and divisions. Hence, many long-term employees do eventually get at least an L-Clearance, if only to navigate some of the laboratory's secure spaces without needing an escort. And if their career trajectories bring them to a point where it is impossible to pursue an established track without a Q-Clearance, the laboratory will usually request one on their behalf. For instance, the biologist pursuing genome research might move into a management position that brings her into contact with secure data, or requires that she have access to secure facilities. In this case, the laboratory might request a Department of Energy security clearance so that she can engage more fully with other areas of the institution.

However, it is not only scientists, engineers, and weapons technicians who need security clearances: anyone performing any kind of work in a classified building or facility needs a clearance to move freely through secure spaces. A contract electrician who starts working in an unclassified area will probably never work with classified data in the same way that a scientist would. However, his employers might pursue a security clearance for him, if the laboratory needs electricians with his particular expertise to perform wiring in a secure area such as X-Division's offices. Similarly, a secretary working in the laboratory's plutonium facility needs a security clearance simply so she can have access to her office. She may also process classified information in support of her group's research efforts, though she herself may never engage with the material in the same way that technicians, engineers and scientists will.

Other newcomers are hired directly into classified groups with the expectation of a security clearance. For instance, I interviewed several younger researchers in X-Division who came to Los Alamos without their Q-Clearances. Usually, they had already filled out the clearance application before coming to Los Alamos, or did so upon arrival. However, they could not join their co-workers in X-Division until the DOE had notified the laboratory that it had approved their security clearances – which can require six months to a year, depending on the intensity of the investigation. In situations like this, X-Division provides neophytes with office space and unclassified computers "outside the fence," sometimes in trailers or in old warehouse bays equipped with cubicles. As long as a new staff member is uncleared, s/he cannot contribute to X-Division's classified work. However, several of these interviewees mentioned that their groups provided a mentor, an experienced designer who provided unclassified work assignments to help newcomers develop some of the skills and understandings required of a weapon physicist. Working on related unclassified projects also provided newcomers a chance to meet their co-workers and to become accustomed to rhythms and pace of laboratory work-life.

In addition, this period of peripherality teaches newcomers important security lessons, delimiting quite severely and dramatically the place where the open world ends and the secret world begins. The new worker is excluded from her group's secrets, an experience that reinforces the existence of a boundary that separates the secret world from the unclassified one. Being uncleared, she becomes an object for her co-workers' security practices: she might be able to attend group meetings in the Administration building, but only under escort, and she will probably be asked to leave at some point as classified information enters the agenda. No matter what her area of expertise, her co-workers will not consult with her on classified projects. They may routinely remind her that she cannot access different areas of data or secure computer networks because she is still uncleared. Lacking a security clearance, she is quite constantly, perhaps even painfully, aware of her peripheral status vis-à-vis the rest of her cleared co-workers.

I have implied that newcomers are never given office space in secure areas, which is not entirely true: in fact, I was an exception to this rule. I joined Los Alamos as an uncleared staff member in Human Resources, and had I stayed there, I probably would never have gotten a Q-Clearance, since the division does so little classified work. However, my work trajectory changed dramatically when I moved from Human Resources to LANL's Archives in February of 1998, after I had decided to pursue ethnographic research on the weapons programs. With a very few exceptions, none of my new co-workers in the Archives performed classified research. However, the Archives were housed in a clerical group, CIC-10, which was responsible for storing and maintaining classified records from all over the laboratory. CIC-10 did not have any unclassified office space and had to bring me directly into the records storage center when I was hired. Although all the classified information was securely hidden from view in opaque files and boxes, my status as an uncleared person in a secure area made my work days regularly humiliating. Everyone who worked in the building was forced to create and maintain a boundary around me, to prevent me from coming into contact with secret information that I was not authorized to see. For most of the time that I was uncleared, I worked frequently at home or in the laboratory's library, because my presence to everyone in the Archives was such an inconvenience to everyone around me.

Whenever I did require access to the Archives, I worked in a moving bubble of security. Whenever I entered the building, one of my co-workers had to drop their work to escort me to my supervisor's office, picking up the loudspeaker to announce "UNCLEARED PERSON IN THE BUILDING! UNCLEARED PERSON IN THE BUILDING!" Before I could enter a room, my escort would again shout a warning, so that anyone working with classified data had a chance to put their documents out of my line of sight. My co-workers posted signs around any area I visited – big black and red signs that read UNCLEARED VISITOR IN THE AREA. In addition to my regular green laboratory badge with my picture on it, I wore a red "Escort Required Badge," so that everyone in the building would realize that I was to be under someone's surveillance at all times. I frequently joked about developing a sense of empathy for medieval lepers (in fact, one of my friends who had worked at the Pantex Assembly Plant in Amarillo later told me that the office area for uncleared staff is known as the "Leper Colony"). I could not go to the bathroom or get a cup of coffee without dragging one of my extremely patient officemates from their work to walk me through the building – which, incidentally, is an enormous warehouse with several large storage bays between the office where my supervisor worked and the rest room.

If becoming a trusted, Q-cleared member of the laboratory begins with peripheral status and routine humiliation, it also begins with errors. Security mistakes, such as breezing by a security guard in the Administration complex, nearly always result in mild-to-moderate public embarrassment. I have never seen a laboratory employee hesitate to point out even the tiniest breach of security etiquette. For a neophyte, being corrected on even the simplest mistakes – like wearing one's badge backwards, or calling the wrong branch of S Division with a classification question – is not simply a reminder of ignorance; it is a call to learn proper security behavior and to adjust one's comportment accordingly.

Only once, during my first week of work at the Archives, did I make the unforgettable mistake of moving out of my escort's line of sight. I was paged to take a telephone call in the next office and walked out of the room before the co-worker watching me could get away from her own phone. As I picked up my call, I heard her drop the line, and she rushed into the room behind me, furious that I had moved even ten feet without waiting for her to walk with me. She stood glaring at me, hands on her hips, as I meekly cut my conversation short and hung up the telephone. "Never, ever, ever go anywhere without someone watching you," she said angrily. "You are not cleared to move around this building without supervision. You don't go anywhere without me." I realized later that escorts who lose sight of their uncleared charges face a security infraction, possibly a security violation, for not following proper security procedures.

Discrepancies like this one are important learning experiences because they "…trigger embarrassment, audience anxiety, shyness, or shame… and trigger newcomers who actively seek information to make sense of what is going on, to revise the activated cognitive schema, and to plan action accordingly" (Fuhrer 1996: 180, 202-203). Embarrassment is a significant part of the learning process, teaching newcomers how to move properly through secure spaces. After that incident, whenever I went into the Archives, I did not move to or from any space unless someone had agreed to walk me through the building. After a while, one of my officemates used to joke that she would buy one of those oversized plastic eyeballs that children get at Halloween and hang it around my neck. "That way we would always have an eye on you," she would laugh, a punch line that invariably brought Jeremy Bentham to mind.

As extreme as this experience may seem, the experience of being a security "leper" is common to nearly everyone who arrives at the laboratory without a security clearance. It is the introduction to the system of decentralized surveillance through which the laboratory monitors its employees, training "the moving, confused, useless multitudes of bodies" into a disciplined workforce (Foucault 1984: 188). Moreover, being uncleared gives newcomers the chance to experience firsthand the system of demarcations that separate the cleared from the uncleared, the classified from the unclassified, the pure from the dangerous (Douglas 1991).

Getting A Clearance

Becoming a cleared member of the laboratory community is the next step in developing a secret self. However, the Department of Energy does not grant its workers easy access to its secrets. The fewer the number of people who hold clearances, the less classified information is dispersed throughout the laboratory, and the easier it is to maintain secrecy. Roughly one month into my fieldwork at LANL, when I was still new at Human Resources, I made the extraordinarily naïve mistake of asking the group secretary if she would give me the clearance paperwork so I could fill it out. She looked at my as though I were crazy. “Who’s getting you a security clearance?” she asked. When I told her that I had decided to request one because it would make it easier to talk to weapons personnel about their work, she patiently explained that I could not simply apply for one as though I were applying for a job. “Someone has to justify it for you,” she explained. “And it takes months to process a clearance.” Moreover, since Human Resources Division rarely handled classified information or required access to classified offices, the laboratory allocated very few clearances to its personnel. “If you can find a champion outside the Division, someone in one of the weapons divisions that will push the issue, you might have a long shot at one,” she told me, doubtfully.

As I learned that day, only laboratory managers can request clearances for their employees, and getting an employee into the clearance “queue,” as it is called, requires demonstrating the need for that individual to have access to classified information as part of their daily work. Moreover, since the clearance investigation process is expensive – roughly $3,000 per person – the Department of Energy only allocates a limited number of clearances to each of its laboratories. When the DOE formally issues its annual allocations across the complex, laboratory managers scramble to justify as many clearances as possible for their divisions and groups, since in areas where classified information is plentiful, it is difficult to hire new people and incorporate them productively into the group's work unless they are cleared for classified. Getting a clearance, then, is a significant event, since it requires that an employee's managers justify using up one of the precious few clearance investigations that the laboratory gets every year. Managers deal with the paucity of Q-Clearances by using L-Clearances as an interim measure, so that many cleared people start their careers in the classified world with an L-Clearance before getting a Q-Clearance. After I joined the Archives, I held an L-Clearance for six months before the DOE granted me a Q-Clearance, based on the fact that I worked in a secure area and that my research required access to classified information.

Being cleared, or "getting a Q," is a difficult, invasive process that requires voluntarily offering one's personal life to the scrutiny of several large federal bureaucracies – namely, the Department of Energy, the Federal Bureau of Investigation, and the Office of Personnel Management. The encounter between the self and the government begins, not surprisingly, with paperwork. The Department of Energy issues prospective applicants a "QSP," or Questionnaire for Sensitive Positions packet, that contains fingerprint forms, legal releases and acknowledgements, and perhaps most importantly, the "Questionnaire for National Security Positions," or QNSP. The introduction to the QNSP tells the applicant how and why the United States government conducts background investigations for certain positions. Background investigations, it says, are

> …conducted to develop information to show whether you are reliable, trustworthy, of good conduct and character, and loyal to the United States? Inquiry is also made about a person's adherence to security requirements, honest and integrity, vulnerability to exploitation or coercion, falsification, misrepresentation, and any other behavior,

> activities, or associations that tend to show the person is not reliable, trustworthy, or loyal (United States Government, QNSP 1998: 1).

Ascertaining strength and quality of character requires that the government collect a good deal of life history information from the individual under scrutiny. The QNSP bears some resemblance to a standard job application, albeit an extensive one that requests some extremely personal information. For an L-Clearance, employees are asked to provide information going back five years into their lives, while a Q-Clearance requests ten years worth of information. One series of questions asks for basic information – full name, one's date and place of birth, citizenship, places lived, schools attended, employment activities, military service. Then the more personal questions begin: provide a list of relatives and "close associates" including current roommates, in-laws, marital and/or current sexual partners. What foreign countries have you visited, why and when? Do you have foreign property or have you acted as a foreign consultant? Dishonorable discharge from the military? Any history of psychiatric consultations? Have you ever been fired from any job? Criminal record, including any traffic tickets over $150? Any illegal use of prescription drugs or controlled substances? Declared bankruptcy or experienced severe financial problems?

Needless to say, a QNSP can require several hours of research – contacting relatives for bits of personal information, verifying dates of school attendance, looking up employers' addresses. S Division warns applicants not to lie about anything, to be open in revealing even painful areas of one's life, such as a history of drug problems, criminal offenses, or mental illness; cautioning that providing false information is "adequate grounds for denial or revocation of a security clearance" (United States Government

QNSP 1998). After filling out the QNSP, I racked my brain to make sure that I had not missed anything significant, wondered if my answers would match what the investigators would find: I did not check my credit report. What if investigators found erroneous information? Would they think I had lied? What if no one could verify that I had held an internship in Kenya? Would my handful of experiences with marijuana constitute a threat to national security?

Applicants submit the QNSP and the other documents in the DOE's Questionnaire for Sensitive Positions packet to S Division's Personnel Security office, where a staff member checks all the documents for completeness and accuracy, then takes the applicant's fingerprints. Sometimes, S Division staff will call applicants back into their offices to make minor corrections to the QNSP: for instance, I had forgotten to initial all the places where I had used white-out to make corrections to my application, and about a week after I turned in the QNSP for my L-Clearance, I was called into the Personnel Security office to scratch my initials next to every correction I had made on the form.

Once the packet is complete, S Division forwards the QNSP to the Office of Personnel Management (OPM) or the Federal Bureau of Investigation (FBI), depending on which agency the government currently mandates to conduct federal background investigations. This is when the investigation really begins. I turned in the QNSP for my L-Clearance in April of 1998, and in June I received a letter from the OPM notifying me that I was officially under investigation, that all the information I had provided on the QNSP would be validated, that an investigator would be contacting my friends and family, that soon I would receive a phone call from an investigator to "ensure that the information you provided on the clearance forms is complete, current and accurate." I wondered who the investigator was talking to, how s/he chose which people would be appropriate to verify which aspects of my existence, what my portrait would eventually look like.

Usually, L-Clearances proceed fairly quickly because they are less extensive and do not usually require an investigation, unless there are problems with the application that can only be resolved in an interview. The application for my L-Clearance was just one of those situations. Tired when I filled out the form, I had absentmindedly stated that my periodic use of marijuana took place between "1987 and the present," rather than the correct time period – "1987 and 1994." This flagged me (erroneously) as an individual who might have an ongoing problem with illicit substances, so I was called to the Security Office for an interview with a DOE investigator. When I explained the nature of my mistake, she raised an icy eyebrow and began asking questions. "So, did you ever sell drugs? Purchase drugs? Do you associate with people who deal in illicit substances? Do your friends regularly use drugs? Precisely which substances do you use on a regular basis?" Finally, exasperated with the extensive questions for what I saw as a purely innocent typing mistake, I asked her to explain the association between my infrequent experiences with marijuana and the threat I might pose to national security. "Well, this behavior shows that you are willing to interpret federal laws in your favor," she told me. "And we figure that anyone who's capable of bending the rules in one area of the law might find it easier to do so in another one – for instance, with national security information."[2]

Experimentation with illicit substances notwithstanding, the DOE granted me an L-Clearance in the summer of 1998. Later, in August, the laboratory approved my inclusion in the 1998 pool of Q-Clearances. At this point, I had to add five years' worth of information to my L-Clearance application. However, the process went much more smoothly, perhaps because I had already been investigated, and on February 26, 1999, I received an e-mail from S-Division notifying me that my Q-Clearance had been approved.

As Gusterson has noted (1996), the clearance process bears strong resemblance to an initiation ritual, particularly in the way it strikes fear of failure into applicants. My co-workers at Los Alamos congratulated me, as though the government's vindication of my honesty represented a significant accomplishment on my part, though I had done little more than fill out paperwork and answer questions. In all likelihood, federal rejection would have cost me my job, not to mention access to my field site. The most striking change in my laboratory life was a sudden ability to move freely through the laboratory: as soon as I collected my Q badge, the physical boundaries that seemed so fixed and exclusionary when I was uncleared blurred into permeability. I could finally walk in and out of my building without constant supervision. The fences and guardposts around the Administration building, the giant combination lock on the door to the Archives, the concertina wire around the plutonium facility all seemed less forbidding. Signs that were

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[2] The issue came up again when I was under investigation for my Q-Clearance, although in a totally surreal way: an investigator called me asking names of people that could that I actually had smoked pot. "No one remembers you using drugs," he told me. "Can you think of anyone to verify that you've used marijuana?"

once exclusive – “Q-CLEARANCE REQUIRED FOR ENTRY” – now welcomed me as a legitimate member of the classified community.

The Knowledgeable Self

Uncleared individuals learn the importance of security long before they receive their security clearances. From their first day of work, members of Los Alamos’s workforce learn that their physical and knowledgeable selves are targets for acts of terrorism and espionage simply because they are associated with the laboratory. However, this lesson intensifies when the laboratory grants individuals a security clearance, an event that draws them more deeply into the laboratory’s realm of secrecy.

Protection begins with awareness. Perhaps most importantly, the laboratory actively teaches its employees to recognize the existence of the knowledgeable self; to police it, so as to prevent the accidental “leakage” of secret information; and how to recognize and mitigate situations that threaten the self and its knowledge. Before newly L- or Q-Cleared employees can pick up badges indicating their new status, they are required to attend an hour-long security training session, during which a representative from S Division emphasizes the importance of vigilance among people who have access to classified data. In all these settings, S Division not only teaches its learners about the practical aspects of security – marking classified information, getting access to a classified computing environment, hand-carrying classified documents from one facility to another – but about the world in which the laboratory does business.

At Los Alamos, people who work with classified information learn that they must maintain strict boundaries around this knowledgeable self because it poses a threat to national security. As one of the LANL's Operations Security training brochures warns its readers,

> The most potentially damaging intelligence source is "us." We may, unknowingly, provide intelligence information to adversaries through carelessness or lack of concern for OPSEC measures in the workplace and in daily contact with others. We may talk in public places about subjects best discussed only in the office with authorized personnel. We may also relate detailed accounts of our daily activities to family members without regard to what they might tell friends or acquaintances (Los Alamos National Laboratory 1994: 13).

S-Division's security trainers suggest that certain environments are dangerous to this self: for instance, the self might encounter threats while traveling abroad. Don't chat about work on airplanes, they suggest, since it is impossible to know who might be eavesdropping on a conversation. Be careful when socializing with foreign nationals, particularly if they appear overly interested in developing a close friendship.   Most importantly, if you are approached by anyone trying to get details about your work, report the incident to Security immediately. If the incident takes place abroad, contact the American Embassy or the FBI. "If you think it's suspicious, it probably is," our trainer told us. "Don't take risks – cut off the contact and report the incident to Security."

It is not long before secrecy becomes a practice that is carried directly from the workplace into the home. For instance, right after I got my Q-Clearance, I attended my first classified briefing, a discussion about nuclear proliferation. At one point during the presentation, I thought about how much my some of my classmates from Georgetown would have enjoyed the topics and issues being discussed – only to realize a split second later that sharing any of my impressions, any of the discussion with them, might violate federal law because none of them have a security clearance. Later in my fieldnotes, I wrote, “In a way, I think I will hate having a clearance, because it means I have to learn to police myself, that I can’t share parts of myself with the people I share everything else with.”

I was surprised to discover that even two-clearance couples keep their classified selves segregated from their marriage partners. One of my interviewees told me that his wife works for the military, and despite the fact they both hold security clearances and work in parallel fields, they never share information about their jobs. He said emphatically,

> We never, ever talk about our work. I mean, never. It’s not that she wouldn’t understand what I do every day – we’re both engineers and we’re both cleared to handle top secret information. But sharing work information is risky because I don’t want to divulge anything classified, and neither does she. So for the sake of security, we just talk about other things.

Ultimately, learning the practices and sensibilities of security causes people to see themselves as potential targets for espionage, even in the home, because they have become repositories for information about nuclear weapons.

Conflicts And Contradictions

Los Alamos’ security policies were the focus of intense government and public scrutiny throughout the last eighteen months of my fieldwork. A recurring theme in the seemingly endless debates about security at Los Alamos concerned the putative conflict between science and secrecy. For instance, a recent New York Times article drew the following conclusion from Los Alamos’ security debates:

> As secrets on nuclear terrorism and miniaturized warheads apparently slip out of the Los Alamos National Laboratory and other federal labs, the message here would seem to be

> obvious: do not give dangerous secrets to scientists (Glanz 2000).

Putting aside for the moment the fact that neither of the two alleged security incidents at Los Alamos involved a scientist trying to inject classified information into an open scientific debate, these kinds of conclusions do make sense – on a philosophical level. There is an "inherent contradiction" between "the ethos of science and the absolute need for the preservation of national security" (Chalk 1985: 29). Concealment, after all, runs contrary to the primary norms of science: the openness of ideas and the exchange of information. Among scientists, secrecy and "secret practices [are regarded as] idiosyncratic and individualistic styles of behavior rather than the professional norm… tolerated but not endorsed by the profession" (ibid 30).

George Marcus has suggested that anthropologists attempting an ethnographic critique of the nation-state system seek cracks in mainstream discourses, locating "experiences within the culture of the mainstream about which there is self doubt" (1999: ix). Like many outsiders to Los Alamos, I thought the conflict between state secrecy and scientific openness would provide me with just such a fissure, ripe for ethnographic critique. But the more I watched my subjects at work, the less I could acknowledge any such contradiction *in practice*. As my fieldwork progressed I realized a simple fallacy in the science-versus-secrecy argument: To accept that professional norms drive scientists to share information, almost compulsively, is to assume that they will not be able to keep any secrets at all, which is patently not true. After all, secrecy and nuclear physics are very old bedfellows. Nazi Germany was the catalyst for early and voluntary censorship efforts: Hungarian physicist Leo Szilard asked his colleagues to refrain from publishing their discoveries, lest Hitler use the information to design an atomic bomb. Scientific giants like Nils Bohr and Enrico Fermi shared Szilard's concerns and backed his efforts. Soon after, the National Academy of Sciences and the National Research Council formalized Szilard's plans by creating a wartime committee to help review scientific publications for militarily significant information. Secrecy, then, has been a voluntary part of physics culture longer than the atomic bomb has been in existence.

If there is a conflict between scientists and security at Los Alamos, it involves clash between state surveillance and civil liberties. Examining the culture of national security at Los Alamos, particularly in light of the laboratory's mission to develop weapons that ostensibly, at least, defend Western democratic institutions, reveals a web of ideological contradictions. Both Hunner (1997) and Hales (1997) have pointed out that during the Manhattan project, the government instituted security practices that routinely violated basic constitutional values, as both private and public speech became subject to government surveillance and restriction. Similar contradictions hold true today: for example, if "innocent until proven guilty," is a core tenet of the American constitution, then the requirements of security turn this mandate inside out, so that the state regards uncleared individuals as *potentially* guilty of gathering and transmitting classified secrets, perhaps compromising national security. This is why uncleared persons are so carefully kept outside the walls of secrecy until the government can adjudicate the individual's potential risk to the nation-state.

Moreover, during a security investigation, individuals waive basic rights of privacy in allowing agents of the state to indiscriminately explore the nooks and crannies of their lives. As Hugh Gusterson puts it, "In the name of state secrecy, the membrane of personal secrecy around the individual self is stripped away… the social processes of secrecy make traditional matters of private discretion into public affairs of state" (1996: 84). Clearance investigations are an odd sort of exoneration, in which agents of the state pry open and scrutinize the self for any evidence of impurity. The process, ostensibly, is voluntary: no one is forced to undergo a clearance investigation, or to give personal information to the government. However, not having a security clearance limits one's ability to move freely through the spaces of the laboratory and may even close career paths. A case in point: I met one woman who was hired into one of the weapons divisions with the promise of a security clearance, only to be fired three months later because her group had run through its allocation of clearances more quickly than expected.

Individuals may refuse to answer an investigator's questions, but the QNSP warns that doing so might "affect your placement or security clearance prospects," while S Division encourages applicants to "answer every question completely and accurately." Even the most personal areas of one's life are bit above scrutiny; one woman told me of being questioned until she was in tears about a recent divorce, while another recounted a tale of being asked to provide personal details about her recovery from an emotionally abusive relationship. When the latter bristled at this line of questioning, the (male) investigator explained apologetically that he was required to record information about psychological conditions that could possibly serve as a basis for blackmail and thus pose a threat to national security.

Security is burdensome for clearance holders in other ways. One of my interviewees, a highly respected designer with over twenty years experience at the laboratory, described the stress of having a classified career in the capricious post Cold

War era. He reacted angrily to a suggestion on the part of laboratory managers that weapons scientists pursue dual career tracks to maintain a presence on the open job market, snorting, “They have absolutely no idea how intensely consuming weapons work is, and we’re supposed to be maintaining the health of the stockpile!” He told me in no uncertain terms that he doubted his ability to get a position in a university. “I simply can’t talk about my research with someone who doesn’t have a security clearance. How do you think I’d come across in an interview?” His feelings are captured in a cartoon that has been circulating throughout the laboratory since the Cold War ended, an ink drawing of a tall, shaggy looking man with ragged clothing and bare feet, standing by a highway and holding a sign that reads, “WILL BUILD WEAPONS FOR FOOD.”

Yet despite the fact that security is invasive, humiliating and restrictive, staff members at Los Alamos acquiesce to its practices, for the most part, with relatively little resistance. This is not simply a matter of enlightened self interest, although the Wen Ho Lee case demonstrated very clearly that an employee accused of a security transgression faces the loss of a security clearance, perhaps job and livelihood, even personal freedom. And as Gusterson has pointed out, being socialized into the secret world of the laboratory causes individuals to internalize this fear in the form of a sense of amorphous surveillance. Never certain when they are being watched, laboratory staff acquire a Foucauldian tendency to police their own behavior, lest they be caught and punished for a security violation (1996: 82-87).

However, adherence to secrecy involves more than a Pavlovian dynamic of reward and punishment. There is a powerful ideological component to security, a belief system that provides a context in which its humiliating and invasive practices are not only justifiable, but a necessary condition for the maintenance of democracy. As Michel Foucault once pointed out, power in its repressive form "can only take hold and secure its footing where it is rooted in a whole series of multiple and indefinite power relations that supply the necessary basis for the great negative forms of power… power produces; it produces reality; it produces domains of objects and rituals of truth" (1984: 64, 205). At Los Alamos, the praxis of secrecy not only "..segregates scientists as isolated elite... and inculcates a sense of group loyalty" (Gusterson 1996: 68); it draws individual workers at into a much larger discourse about value of nuclear weapons; and in doing so, it makes every staff member personally responsible for maintaining the sovereignty of the American nation-state in a world of threats. In other words, the discursive field that validates the practices of security at Los Alamos is a close relative of the discursive field that endows nuclear weapon technologies with such powerful significance as the ultimate deterrents to conflict.

A WORLD OF THREATS

Claiming that the local practices of secrecy at Los Alamos reinscribe international power politics might seem farfetched, until one considers that Los Alamos' longstanding nuclear weapons mission places it squarely in the middle of the international security community. As anthropologist Joseph Masco has pointed out, Los Alamos may reside geographically in northern New Mexico, but by virtue of its mission, its sociopolitical location and concerns lie well "…[beyond] U.S. territorial borders, far away from New Mexico" (1999: 210). For over fifty years, weapons scientists and engineers have maintained American military hegemony by minting nuclear explosives, the most valuable currency of threat in the international system (Turner 1997: 24). This mission positions the laboratory's weapons experts as key advisors to military and foreign policy strategists, who rely on LANL to provide technical judgments about not just the American nuclear stockpile, but also the nuclear capabilities and nuclear potential of other states.

For many people at Los Alamos, the laboratory's weapons mission is a source of institutional pride, and its staff members – particularly those who worked in the weapons programs during the Cold War – see themselves has having played an important role in the dissolution of the Soviet Union. This sentiment was expressed in a memo that one senior laboratory manager recently distributed to his staff:

> You can and should be proud… of the role played by Los Alamos in securing the peace for over 50 years while holding in check the tyranny that almost engulfed the world. Until the advent of nuclear weapons, Moscow had acquired an average of 35,000 square kilometers of territory – an area equivalent to Holland – every year for 150 years. It was weapons developed here and in our sister laboratory [Lawrence Livermore] that stopped that expansion and secured Western civilization… We are part of this historic enterprise and I'll be damned if I will hang my head in shame.

Or as one retired nuclear engineer told me, emphatically, "I'm damn proud of my career in nuclear weapons. People in Santa Fe can protest nuclear weapons like they do because my work kept the Soviet Union the hell out of our democracy."

Despite the fact that the Cold War is over, however, very few of the laboratory's weapons and security experts would argue that the world has been permanently secured for American democracy. From the perspective of the laboratory, and indeed from the perspective of many international relations experts, the world remains a threatening place, perhaps even more threatening now that the world is not tidily organized into spheres of influence. The Cold War's end replaced one gargantuan threat – the Soviet Union – with a plethora of smaller, more capricious threats to American sovereignty in the form of free-agent terrorists and rogue nations. As former CIA Director James Woolsey said, "We have slain a large dragon, but we live now in a jungle filled with a bewildering variety of poisonous snakes" (United States Senate, Committee on Governmental Affairs 1997: 1).

Security experts at Los Alamos argue that efforts by so-called "rogue nations" to acquire nuclear capability are perhaps the most destabilizing trend the international system currently faces. Recently, Los Alamos screened for its workforce a public relations videotape produced by the DOE to highlight the political importance of America's weapons laboratories in the post Cold War era. The video opens with politicians and diplomats describing how the nature of threat has changed since the late 1980s. James Schlesinger tells viewers that the end of the Cold War represents an extended period of security for Western democracies, but cautions that this environment could change at any point. George Schultz, who was Secretary of State during the Reagan administration, is more specific:

> The Cold War is over, so we don't have the bipolar structure that we had in those days. But there is a widely dispersed power, and one of the difficult things about the world that we live in is that you don't have to be big to get a weapon of mass destruction like a nuclear weapon. So it's an uncertain world, you don't know where your threats are going to come from.

As Schultz makes this observation, the video shows newspaper headlines drifting by: "The World's Expanding Nuclear Club: Nations Scrambling to Join." A map follows with the world's declared nuclear powers in blue (Pakistan and India are included in this category) as well as potential proliferant nations – Iraq, Iran, North Korea, Libya, Israel – highlighted in yellow (United States Department of Energy 1998).

Within this capricious new world order (or disorder), the laboratory's mission remains essentially the same: to maintain America's nuclear deterrent capability, so that potential aggressors will think carefully before launching an attack, conventional or otherwise, against the United States. At the same time, LANL's post Cold War mission statement, "Reducing the Global Nuclear Danger," calls upon the laboratory to contribute its expertise to preventing the proliferation of nuclear weapons to non-nuclear nations. Ironically, in the wake of the Cold War's end, the proliferation of threat seems to have had the dubious effect of reinforcing the laboratory's commitment to its weapons mission.

This is where the "whys?" of secrecy come into play. Members of the laboratory workforce, regardless of whether or not they hold a clearance, must understand that security maintains a critically important veil of secrecy around the laboratory's classified weapons information. If compromised, this information could facilitate the proliferation of nuclear weapons to other countries, and could also undermine the deterrent effect of the nation's nuclear stockpile.

Learning Threat, Learning Security

It is this discourse of international threat that the laboratory's security experts invoke when they explain the importance of secrecy to the workforce. All the laboratory's employees, from janitors to accountants, chemists to electricians, secretaries to physicists, are required to attend security awareness training. Of all the messages that

S Division tries to inculcate in the workforce, security awareness is unique in the powerful messages it sends about the importance of a personal commitment to international security.

During my fieldwork at Los Alamos, I attended many sessions of security awareness training. As I noted earlier, General Employee Training includes a unit on security awareness for new employees. In addition, every division requires that its employees attend annual security refresher briefings, which usually consist of an afternoon's worth of lectures and discussions about possible threats to security. People who hold security clearances receive additional security awareness training from S Division, both at the time that they receive their security clearances and in annual refresher briefings for clearance holders.

During these seminars, employees spend an afternoon, perhaps an entire day, immersed in a discursive field that emphasizes their role in protecting secret information about nuclear weapons, the technologies that play such an important role in mitigating the inherent violence and anarchy of the nation-state system. One senior manager in the weapons programs is famous at Los Alamos for reminding the workforce: "The President defines nuclear weapons as a Supreme National Interest. That means that our core mission is vital to the interests of the United States." Another S Division security trainer often describes Los Alamos as a "jewel" in America's military "crown." One seminar I attended featured a rather flamboyant guest speaker from the FBI who opened his presentation with a patriotic music video that set footage from the Gulf War to a Huey Lewis rock song about Vietnam. When the video was over, he praised his audience for ending the Cold War: "Everyone knows that you're the best of the best," he told us.

"Your technology keeps us from going to war. And that's why every country in the world, ALL OF THEM, want to get to know YOU!"

It is the precisely laboratory's scientific reputation that, ostensibly, makes it a popular and frequent target for foreign agents, computer hackers, and other infiltrators. Employees are warned to watch carefully for signs that their workplace has been targeted: strange vans or vehicles parked outside their building for a long period of time, evidence that a computer system has been hacked, unfamiliar faces in the hallway. Strange packages, particularly with misspelled addresses, wires hanging out, or oil stains might be bombs, warned one trainer. Do not open the package, but call Security instead. Most of all, employees are warned to watch for the insider threat posed by trusted co-workers who might misuse their access to classified information. People who are defensive about their work, regularly stay extra hours, request access to classified information that they do not normally require, pursue a lifestyle beyond their apparent economic means – all of these are clues that the individual might be passing classified information to unauthorized parties.

In addition to formal training, S Division staff also spend considerable effort reinforcing employee awareness of possible threats to security. Take, for instance, "ThreatGrams," a daily electronic newsletter that compiles international headlines about economic unrest, border skirmishes, terrorist activity, political upheaval, and any other event or trend that might be construed as posing a threat to international stability. S Division does not compile or edit the newsletter; rather, it subscribes to ThreatGrams and encourages LANL staff to add their e-mail addresses to the list of recipients. The same office that provides ThreatGrams occasionally sends laboratory-wide notices about events that might pose a more immediate threat to the safety and security of laboratory personnel. At times it takes genuine imagination to construe these threats as locally significant: for instance, when Serbian officials responded to the NATO bombing campaign with death threats to American military personnel, S Division sent urgent electronic notices throughout the laboratory warning people to watch for possible terrorist activity on laboratory property.

Still, I found that many of the people I worked with, interviewed, and observed, took these messages about threat to the knowledgeable self quite seriously, even supporting the validity of these warnings by swapping stories, perhaps apocryphal, of security incidents. For instance, a colleague mentioned to me casually that a friend of his on travel to Russia had been approached by a foreign agent, who asked him to carry a package to the United States. "The agent knew he was from Los Alamos, he knew what he did, can you believe that?" my friend said, shaking his head. "But I guess things like this happen all the time. That's why security is so important."

S Division reinforces these messages by hanging security awareness posters throughout the hallways and offices of Los Alamos. In addition to slogans like "Security Begins With ME!," security experts employ some strikingly creative visual punchlines to encourage workforce vigilance against threat. One of my favorite posters is a drawing of a shadowy figure driving a car, with a pair of sinister yellow eyes looking back at the viewer from the read-view mirror. The caption at the bottom is the acronym for one of the laboratory's awareness seminars: DICE, or Defense Information Counter Espionage. Another poster announces simply, "Countries don't have friends. They have *interests*." The map in the background depicts the British Isles; this poster is a not-so-subtle reminder that staff members should never share information indiscriminately with any foreign contact, even from nations whose interests and ideologies are closely aligned with those of the United States.

What is perhaps most striking about discourses of security at Los Alamos is the way they minimize the possibility that nuclear weapons *themselves* might constitute an immediate source of threat. People who are unfamiliar with the nuclear weapons community may be surprised, even horrified, to learn that themes of nature and naturalness are smoothly woven into local discourses about nuclear weapons. Not infrequently, I heard weapon designers describe nuclear fission and fusion as "happy accidents of nature." Other metaphors for nuclear weapons that I have encountered at Los Alamos include "buckets of buried sunshine" or "stars brought to earth." One designer explained to me that laboratory's role is merely to exploit certain aspects of nature for "useful military applications."

There is also the issue of deterrence theory. As an anthropologist, I was always bemused by the way that weapons and security experts at Los Alamos claimed a simple, primordial link between nuclear deterrence, nuclear weapons and the "natural" human prerogative to defend the self. For instance, in 1998 I attended a lecture on nuclear proliferation given by one of the laboratory's top security experts. He opened his presentation with a drawing of a Neanderthal-like figure grimacing and brandishing a club. In his introductory remarks, he told an audience of laboratory employees that deterrence at the level of the nation-state is a natural extension of the human species' reliance on weapons to prevent enemy incursions into one's territory. The message, of course, is a classically Hobbesian one: nature is violent and brutish; humans co-exist in self-interested anarchy; and stability can only be achieved through force. In essence, Los Alamos offers a tightly rationalist solution to the inherent violence and capriciousness of human nature: nuclear weapons, whose ability to curb human aggression endows them with an almost redemptive power.

The combination of discourses – that nuclear weapons and deterrence are just one order removed from an imagined state-of-nature, and that threats abound from desiring rogue nations who plant wiretaps and employ foreign agents – makes for some topsy-turvy definitions of what constitutes a “threat.” For instance, while visiting the DOE offices in Nevada, I found myself staring at a poster that highlighted a drawing of a cell phone in a circle with a large X through it. In the background – which was painted an ominous red – several mushroom clouds rose to the sky. The caption read, “WEAPONS OF MASS DESTRUCTION/WEAPONS OF MASS DISCUSSION.” In the secure world of the laboratory, cell phones are more dangerous than nuclear weapons. This apparently bizarre assertion makes sense only in the paradoxical logic of post Cold War deterrence: nuclear weapons information is safe in the confines of rational, Western democratic states, but dangerous in the hands of rogue nations. In this paradigm, cell phones pose a more immediate threat to the stability of the international system than do nuclear weapons.

CONCLUDING DISCUSSION

As a bundle of understandings and practices designed to keep classified information from compromise, security plays an important role in reproducing at the level of the laboratory a "culture of insecurity" (Weldes 1999). Rarely, if ever, did I hear anyone question the assumptions that underlie accepted definitions of national security, the utility of deterrence theory, the appropriateness of maintaining a nuclear stockpile in the post Cold War era. Whenever I attended a security briefing at Los Alamos, I often thought of a passage from Cynthia Enloe's commentary on a similar discourse of threat that permeated the Iran Contra hearings:

> 'We live in a dangerous world.' No one questioned this portrayal of the world as permeated by risk and violence... The vision that informed these male officials' foreign policy choices was of a world in which two super-powers were eyeball-to-eyeball, where small risks were justified in the name of bigger risks... (Enloe 1989: 12).

Similarly, at Los Alamos, none of my informants ever questioned the value of nuclear weapons as mechanisms to deter war, despite the fact that the bilateral framework in which deterrence was developed had fallen apart nearly ten years before. Rather, my colleagues at Los Alamos tended to treat the construction of the world as a threatening, anarchic place as received knowledge.

Yet there is a good argument to be made that the practices that mitigate threat at Los Alamos simultaneously *reproduce* an ecology of danger. The laboratory's weapons and security experts do not simply borrow uncritically from foreign policy and security discourses to validate the deterrent role of nuclear weapons. Rather, they are active contributors to disciplines like international relations and security studies that provide a discursive framework in which nuclear weapons are valuable military technology (Gusterson 1999). Los Alamos often plays host to international relations scholars, and has historically maintained its own security think tanks, such as the now-defunct Center for National Security Studies and its current incarnation, the Center for International Security Affairs. Moreover, the career trajectory of the DOE's weapons researchers frequently extends beyond the laboratory to policy-making institutions in the nation's capital, so that experts from Los Alamos can and do "…play a vital role in shaping the government's options, priorities and… the direction of national policy on all aspects of nuclear weapons development and arms control" (Schwartz 1996: 154).

Indeed, security practices and research practices are so intertwined at Los Alamos that at times I found it difficult to separate the two. I close with an ethnographic vignette: one afternoon last summer, I spent a few hours watching a young secondary designer as he sat as his desk in a cramped, airless office in X Division, running a brightly colored model and describing to me what the model told him about a particular device in the stockpile. I sat just behind him, next to a heavy file safe locked with a large combination dial and marked with "SECRET" magnets on every drawer. The designer's computer had bright red-and-white security warning tape wrapped around the monitor and the CPU, and a large red-and-white "PROCESSING SECRET FRD/RD" security placard sat on top of the monitor. His door was closed to passers-by in the hallway, despite the fact that his office was located in a heavily restricted area of the lab. As he talked about his model, he tied his observations back to the health of the stockpile, his own ability to make judgments about long-term weapon performance, the importance of maintaining confidence in the nuclear deterrent. I remember his sense of political responsibility, the seriousness with which he approached his research, and the clear markings of secrecy all over his office. The science, his air of mission, and the security barriers surrounding his work were so closely married that it was impossible to separate one from the other; indeed, I thought as I watched him, weapons research is a project that – at Los Alamos at least – unifies science and secrecy in unexpected ways.

"TRUTH is generated HERE."
- Jay Norman, former LANL Test Director,
on the Nevada Test Site
ca. 1987

CHAPTER THREE: CYCLES OF COLD WAR KNOWLEDGE PRODUCTION

Nuclear weapons – or more precisely, the knowledge that goes into the design and production of nuclear weapons – are the "jewel in the crown" that makes the laboratory so valuable to the United States. Security experts teach the laboratory's staff that the nation's nuclear deterrent is a key pillar supporting American democracy and global stability. Illegitimate attempts to gain access to the laboratory's weapons knowledge will not only destabilize the military-political authority of the United States, but could jeopardize the stability of the international system as well.

However, espionage is not the only problem that Los Alamos faces: in the wake of the Cold War's end, time and inactivity have emerged as significant, if less sinister, threats to the laboratory's knowledge base. In the weapons community, knowledge erosion is a common topic of discussion; senior experts worry openly that without an active program of designing and testing nuclear weapons, fifty years' worth of weapons-related knowledge might simply evaporate as experienced Cold Warriors retire from the laboratory. Indeed, knowledge loss rivals espionage at Los Alamos as a topic of concern among weapons experts, from technicians to construction engineers to experimental physicists (Fialka 2000).

This chapter is actually less concerned with weapons per se than it is with knowledge. More precisely, I am interested in exploring the assertion – made by many weapons experts – that it is difficult, if not impossible, to make accurate judgments about weapon behavior without direct experience in the design and testing of nuclear explosives. This line of questioning is similar to that raised by Donald MacKenzie and Graham Spinardi (1995) who recently suggested that a test ban regime could, in some limited sense, bring about the gradual "uninvention" of nuclear weapons as the knowledge required to build them disappears. However, MacKenzie and Spinardi use nuclear weapons science as a provocative case study to challenge the idea that scientific knowledge is universal and explicit. They do so by emphasizing the importance of tacit knowledge in nuclear weapons research, pointing out that it is difficult, if not impossible, to develop a nuclear stockpile using explicit information alone. My intentions are slightly different than MacKenzie and Spinardi's: like most anthropologists, I am interested in people as knowing beings (Barth 1995: 66); and rather than evaluate the tacit-versus-explicit content of weapons-related knowledge, I seek to understand how engagement with nuclear explosives reproduced an actively knowing community of weapons experts at Los Alamos.

In this chapter, I relate what I have learned about the laboratory's Cold War weapons mission and how its experts went about fulfilling it. Not only was the weapons community at Los Alamos responsible for designing and developing nuclear explosives, it was charged with maintaining confidence in the nation's nuclear deterrent. The community did so by conducting experiments, in the form of nuclear and non-nuclear explosive tests, to gain insight into the performance characteristics of nuclear explosives. In doing so, the weapons community established detailed technical understandings about the nation's nuclear weapons, information that was essential to policymakers who based America's defensive posture on nuclear deterrence.

The laboratory's experimental activities produced an enormous body of knowledge about nuclear weapons; but in this chapter, I focus on the social functions of what I refer to as the laboratory's Cold War "design and test cycle." To execute its elaborate experiments, the laboratory relied on a multidisciplinary community of experts who brought skills from a variety of backgrounds: engineering, experimental physics, geology, mathematics, to name a few. The design and test cycle acted as an engine for the ongoing integration of expertise and the social reproduction of the weapons community; indeed, experimental activity was critical in organizing social relations among the hundreds of staff members involved in weapons work at Los Alamos.

One of my goals in this discussion is to challenge the commonly held perception that nuclear weapons research is primarily the pursuit of weapon designers in X Division (e.g., Gusterson 1996, Fialka 2000). Although weapon designers are key figures the nuclear weapons programs – they are so central, in fact, that I often heard them referred to as the laboratory's "Brahmin class" – the research programs they outlined required the support of the entire weapons community. Therefore, I have tried to incorporate perspectives from a variety of fields at Los Alamos, including weapons design, experimental (or "diagnostic") physics, and engineering, to describe how the laboratory's many distinctive ways of knowing were integrated in the process of conducting a full-scale nuclear test. Ultimately, I hope to show that Cold War weapons knowledge existed as a form of situated action (Suchman 1987), located in a nexus of relationships that linked many different kinds of weapon experts to each other and to the nuclear artifacts they created.

One final caveat: that the experimental environment of the Cold War has been mostly defunct since 1992, and exists now primarily as a function of the weapons community's collective memory. Therefore, my description of the laboratory's Cold War knowledge environment is largely culled from my fieldwork experience: interviews with retired weapons experts, informal discussions about the state-of-the-weapons-community, visits to the Nevada Test Site, some reading, and a great deal of osmosis from eighteen months of immersion in the laboratory.

DETERRENCE AND CONFIDENCE

Throughout the Cold War, Los Alamos' research goals were closely entwined with the principles of nuclear deterrence. In developing and maintaining the weapons that made up the nation's stockpile, the weapons community played a key role in national security by translating deterrence theory into working technologies (Apt 1988 1; Garrity, Pendle and Selden 1988). At the same time, the laboratory was in the business of *nuclear confidence*: certifying to the world, beyond reasonable doubt, that the weapons it designed for America's nuclear stockpile would work to precise performance specifications whenever required to do so.

The laboratory's unique Cold War mission was a direct outgrowth of its World War II mission. Founded in 1942, Los Alamos was the third of the federal government's three "atomic cities," each of which was established as a research and production center for World War II's Manhattan Project. Los Alamos's sister facilities, located in Oak

Ridge, Tennessee and Hanford, Washington, were (respectively) responsible for developing uranium and plutonium. In contrast, staff at Los Alamos were charged with weaponizing these materials, and for developing a deliverable bomb that could take advantage of the explosive properties of the uranium and plutonium produced in the Manhattan Project's reactors. As such, scientists and engineers at Los Alamos had a much wider range of responsibilities, including every aspect of weapon design, from the nuclear explosive, to arming, fusing and firing the bomb itself.

However, Los Alamos' role changed at the end of World War II, after Congress had passed legislation creating the Atomic Energy Commission (AEC) as a civilian organization responsible for all research related to the atom. The establishment of the AEC formally institutionalized the rough wartime system that divided labor into interlocking centers of expertise during the war. In 1946, postwar laboratory director Norris Bradbury wrote in a letter to the AEC commissioners, "It is the belief of senior technical personnel at Los Alamos that this laboratory should not attempt to carry out… purely ordnance engineering aspects of atomic weapons development…the Los Alamos Laboratory may be most effective if its concern is limited to the nuclear components of atomic weapons…" (Bradbury 1946, in Truslow and Smith 1983: 441). The AEC heeded Bradbury's request and moved Z Division, Los Alamos' ordnance engineering division, to the smaller Sandia Laboratory in Albuquerque. This had the dual effect of streamlining Los Alamos' mission to focus on nuclear explosive research and development, while simultaneously creating a need for new facilities to take over the laboratory's wartime production efforts.

Throughout the 1950s, as the nuclear stockpile developed both quantitatively and qualitatively, the AEC poured massive amounts of money into developing a complex of highly differentiated facilities, each with a specific role in the development, testing, and production of full-scale nuclear weapons. At the time the Cold War ended, the DOE's nuclear weapons complex consisted of roughly a dozen separate research, manufacturing, and assembly facilities. Within this system, Los Alamos (and later the Lawrence Radiation Laboratory established at Livermore, California) maintained a relatively narrow focus on the design and development of nuclear explosives throughout the Cold War. For most of its postwar history, then, Los Alamos has designed, engineered, and tested experimental nuclear devices, prototype "physics packages" that form the destructive core of a nuclear weapon.[3]

Broadly speaking, most weapons in today's nuclear stockpile are two-stage weapons, meaning that they are composed of a fission trigger, or a *primary,* as well as a thermonuclear *secondary*. The primary is ignited when a spherical shell of high explosives compresses a mass of fissionable material (such as plutonium) into an explosive, or supercritical, configuration. The energies released by the primary, in turn, set off an even more powerful thermonuclear (fusion) reaction in the secondary. Throughout the Cold War, all nuclear weapons included some type of primary device;

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[3] The Manhattan Project marks the birth of what historian Thomas Hughes refers to as a "sociotechnical system," defined as a sprawling configuration of people, institutions and technology loosely but purposively organized around furthering a public goal – the distribution of electricity, for example; or in this case, the design and development of nuclear weapons (Hughes 1983: 15). Their characteristics include "....related parts or components.... connected by a network, with central controls [exercised to] optimize the system's performance and to direct the system toward the achievement of goals" (1983: 5). For a more thorough discussion of Los Alamos' postwar mission and the emergence of a well-defined "division of labor" across the nuclear weapons complex, see Truslow and Smith 1988, Fehner and Holl 1997, Schwartz et al 1999.

most – but not all – have included a secondary as well.[4] For fifty years, scientists and engineers at Los Alamos designed and certified both primary and secondary devices to fit weapons systems created by the Department of Defense.

It is impossible to overemphasize the technological differences between an experimental device – whether a primary or a secondary – and a deliverable nuclear weapon. As I discuss below, weapons physics is a highly descriptive enterprise, one that relies on the triangulation of computer models, empirical data and expert judgment to build understandings about the complicated physics processes that take place in a split second of transition, when firing energy from a fuse or trigger enters a stable system – the explosive device – and causes its parts to move, shift, change, and finally blow apart (designers refer to this as “disassembly”). In order to describe the physics of a nuclear weapon, and to make confident statements about its performance, designers must characterize the physics of this transition in minute detail. Doing so requires large amounts of data from the explosion.

Hence, a test involved far more than burying and detonating a nuclear bomb. It did, in fact, involve enormously complex systems for gathering information. Nuclear experiments required layer after layer of technology – a nuclear device, fiberoptic cables, diagnostic imaging devices, witness plates, et cetera, all woven together in a massive steel rack, so that no single aspect of the experiment would interfere with any other. Successfully performing an experiment required exquisite coordination and timing, so that each piece of equipment would function perfectly, gathering and transmitting precious data in the few billionths of a second before energies coming off the explosion

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[4] See especially Richard Rhodes’ *The Making of the Atomic Bomb* (1986) and *Dark Sun* (1995) for a detailed historical account of the development of fission and thermonuclear weapons.

destroyed every piece of instrumentation. Similarly, deliverable nuclear weapons, the kind that the U.S. military deployed throughout the world during the Cold War, lack the hundreds of components that the laboratory used to provide empirical data about weapon behavior. Of course, the laboratory relied on its experimental devices, and the knowledge it gathered from testing them, to create and modify designs for new warheads. However, once Los Alamos and Sandia had jointly certified a particular design for production, the two laboratories transferred the design onto the Department of Energy's facilities for mass production, assembly and stockpiling.

Los Alamos manufactured neither warheads nor bombs; but through its experiments with nuclear explosives, it did produce an immense body of knowledge about nuclear weapons (Hecker 1988; Cochran, Arkin, Norris and Hoenig 1987; O'Neill 1998: 43). Because of this, I prefer to think of Los Alamos as a knowledge production facility rather than as a bomb factory, since the laboratory provided political and military leaders with empirically validated, theoretically based understandings about how those devices behaved. In other words, weapons experts at Los Alamos were responsible for maintaining the nation's nuclear deterrent in two ways: a) through its role in designing nuclear weapons for the US stockpile, and b) in producing *confidence* in the stockpile, in the form of statements about the safety, security, and reliability of the physics packages inside those weapons.

Nuclear Deterrence

In his ethnography of weapons designers at Lawrence Livermore, Hugh Gusterson argues that nuclear deterrence – the idea that nuclear weapons prevent conflict – is the grounding moral axiom for Livermore's weapons community (1996). The same is true at Los Alamos: indeed, it is impossible to understand nuclear confidence as a critical component of the laboratory's mission without appreciating deterrence theory as a philosophical framework in which nuclear weapons, and therefore the laboratory's weapons mission, make sense. I am not going to engage in an extended critique of nuclear deterrence, save to point out two things: first, that deterrence theory is very much a product of the twentieth century; and secondly, to explain what nuclear confidence is, and to describe its critical role in making tenable the oddly paradoxical logic of deterrence.

The laboratory's security experts may rhetorically imagine deterrence as primordial human nature writ boldly at the level of the nation-state, but the emergence of nuclear deterrence theory can be traced to a precise historical moment: the invention of the modern airplane in the early twentieth century. Technological innovation opens the door for the emergence of new social and political structures, as people "construct their social worlds using the resources at hand" (Pfaffenberger 1992: 500). Airplanes, which were almost immediately perceived as potential instruments of military power, paved the way for the development of deterrence theory by widening the focus of military strategy to include civilian populations as targets for attack. Indeed, airplanes captured the collective imagination of the European military theorists, who spent the years between World War I and World War II developing theories of strategic bombardment that depended on air power to deliver bombs deep into the enemy homeland (Freedman 1989). These theories predicted a swift victory for the side that first gained control of the skies.

World War II provided an opportunity to put strategic bombardment to the test; and although it did not entirely fulfill its promise, airplanes and rockets played a major role in the war. Perhaps more importantly, the development of the atomic bomb – "whose association with the termination of the Pacific War surrounded it from the start with an air of decisiveness" (Freedman 1989: 5) – seemed to offer a stronger technological means of realizing the potential of strategic bombardment as a recipe for ending war quickly and conclusively.

By the end of World War II, weapons of mass destruction offered more than a means of realizing the promised potential of technology to gain a decisive victory. For postwar military strategists like Bernard Brodie, Herman Kahn, and Jacob Viner (Fetter 1988: 160), nuclear weapons offered the first truly viable means of *preventing* war by "…compelling men to do what their moral and political inventiveness alone have never been able to do…": To consistently seek non-military resolutions to conflict by making the price of aggression too high, so that "…to the believers in deterrence, [nuclear weapons] technology [existed as a benevolent] despot presiding over the destinies of men and nations" (Tucker 1988: 1).

As hostilities between the United States and the Soviet Union burgeoned in the postwar era, nuclear weapons became the primary currency of threat as both East and West used theories of strategic bombardment to support a policy of developing and diversifying nuclear arsenals to deter enemy attack. During the Cold War, deterrence policy went through several formulations: minimum deterrence, extended deterrence, massive retaliation, flexible response, mutually assured destruction (Freedman 1983). Yet a key paradox ran consistently through all these formulations: that the sole purpose of posturing threat with massively destructive weapons was to negate the very possibility of their use (Shreffler 1975; Fetter 1988: 162; Hecker 1988; Walker 1993: 94-96, Schwartz 1996). Read as a text, nuclear weapons are an ironic technology, one that offers security by creating a state of perpetual insecurity; that threatens a destructive power so great as to be militarily worthless; and whose usefulness is only demonstrated when it remains perpetually unused. In this discursive "twilight of logic" (Taylor 1998: 303; also 300-305), the value of these weapons lies in their destructive potential. Under a regime of mutual threat, when each opponent perceives itself as vulnerable to a devastating nuclear attack, neither side is likely to act aggressively, and thus war is unthinkable because the costs are incalculable (Wasserstrom 1985: 25).

The efficacy of the nuclear deterrent, then, was largely a matter of perceived threat; and the credibility of the nuclear threat, in turn, rested on two declarative pillars: stockpile reliability and stockpile confidence. These concepts are interrelated but refer to two qualitatively different ways of talking about weapon performance. Reliability is an "objective measure of the average fraction of weapons [in a nation's stockpile] that will perform properly," and is rooted in systematic testing of different components of a weapon system. Confidence, on the other hand, is "a subjective measure based on the perception of those people responsible for the stockpile that the weapons are reliable" (Fetter 1988: 70-71). Though subtle, the difference between the two is hardly trivial, as scholar Steve Fetter has pointed out:

> …[because] deterrence is more a matter of perception (confidence) than reality (reliability). If American leaders are convinced of the reliability of their weapons, and Soviet officials, observing this confidence, are also convinced of the potency of the US arsenal, then the requirements of deterrence are satisfied independent of the actual reliability

> of the weapons" (Fetter 1988: 70, emphasis added; see also Cimbala 1998).

The guiding axiom of the United States' national security policy was to prevent conflict by maintaining a credible nuclear deterrent capability; and nuclear confidence was the principle that transformed the recursive, dead-end logic of nuclear deterrence into a workable foundation for defense policy (Rosenthal 1990).

Nuclear Confidence

Although nuclear weapons are a quintessentially military artifact, they were designed – and are still maintained – by the Department of Energy (DOE), which is a civilian organization that has historically pursued weapons research on behalf of the Department of Defense (DOD). Throughout the Cold War, DOD strategists worked closely with weapons experts in the DOE's design laboratories to specify the military characteristics for new nuclear explosives. Generally, the DOD would design the delivery vehicles – a ballistic missile, for instance, or a gravity bomb – and the DOE's design laboratories would create a nuclear explosive to fit the DOD's weight, size and yield specifications. The Department of Defense and the DOE's design laboratories organized weapon development activities through an eight-phase weapons acquisition cycle, which the DOE relied upon to coordinate all aspects of a weapon's lifetime, from its earliest design stages, through refinement, production, stockpiling, maintenance, retirement and disassembly.

Los Alamos' most intense levels of activity occurred during the first three phases of this acquisition cycle, in which the laboratory explored the feasibility of particular concepts, transformed these concepts into prototype technologies for possible use as nuclear weapons, and refined and certified new nuclear explosives for the stockpile. But LANL's involvement with the stockpile did not end there: the laboratory's weapons experts retained oversight responsibility for every device they had designed, as the systems moved out of the experimental phases into mass production, storage, deployment, and eventually retirement. As experts with an intimate understanding of the device's workings, the laboratory's designers and engineers might be asked to field literally hundreds of questions about the device at different stages of its lifetime, from issues that arose during mass production, to stockpiling, to military deployment, to retirement (Hecker 1988: 4-7). As long as LANL's experts could certify that a particular device would work to design specifications, military planners could state confidently that the weapon system housing that device would fulfill the particular deterrent capability assigned to it. When the laboratory's weapons designers and engineers declined to certify the performance of one of their explosives, the military would remove the device from the stockpile and the laboratory would replace it with a new, updated version (United States Department of Energy, Albuquerque Operations Office 1984; Hecker 1988).

Much of the weapons community's knowledge was directly related to device performance: reliability, weapon safety, assurance that the device not detonate accidentally; and security, guaranteeing that the device not be vulnerable to unauthorized (e.g., terrorist) detonation. However, the laboratory's expertise extended beyond weapons to include a variety of related policy fields: military strategy, foreign policy, arms control, and civil defense. The DOE's weapons laboratories supported the Department of Defense in experiments designed to characterize the effects of nuclear explosions on military hardware, communications systems, civilian structures, and other nuclear weapons, all of which had implications for strategic and tactical military planning and civil defense. Within the field of arms control, the laboratory's experience with nuclear weapons development positioned its experts to comment on issues of verification and to develop technologies that would assure compliance with arms control agreements. The laboratory also conducted research in health physics, developing understandings about the effects of radiation on the environment and human health. Taken together, this enormous and constantly evolving body of knowledge provided the basis for the laboratory's claims to social and political power in the form of expertise (see Apt 1988, Hecker 1988; Gusterson 1996, Nader 1996, Schwartz 1996, Collins 1997, Schwartz 1998).

It was in this dual capacity – as researchers and designers who provided the nation with working nuclear explosives, and as experts making judgments about the long-term safety, security and reliability of those explosives – that scientists and engineers at Los Alamos were responsible for maintaining confidence in the nation's nuclear deterrent. In making technical assessments of the physics and mechanics of nuclear weapons, the laboratory sent political and military leaders throughout the world a precise message about the United States' deterrent capabilities. It is no exaggeration to say that the technical statements of the laboratory's scientists and engineers underwrote nuclear confidence, and by extension, nuclear deterrence.

The laboratory's success in this mission required impeccable credentials on the part of its experts. "Whenever a scientist has a very serious message to convey," writes Mary Douglas (1991: 230), "he faces a problem of disbelief. How to be credible?" The issue of credibility was particularly important at Los Alamos, since the laboratory's weapons-related judgments were as much the bedrock of nuclear deterrence as were the weapons themselves. As one of the laboratory's senior policy analysts explained,

> The heart and soul of any successful policy of mutual nuclear deterrence is the certain belief of national leaders, beyond reasonable doubt, that their own and their adversaries' nuclear forces are … deliverable and will function as intended under any circumstances… [this belief] rests solely on the assurances given to those leaders by scientists, and by the credibility that those scientists have with the leaders (White 1987: 2; emphasis added).

Or in the words of former laboratory director Sig Hecker, "…the credibility of the U.S. nuclear deterrent policy rests indispensably upon the credibility of the three DOE nuclear weapons laboratories" (1988: 4-6).

Throughout the Cold War, American military planners and political decisionmakers had little reason to question the laboratory's judgments, its credibility or the competence of its experts. Los Alamos was – and remains – one of the few places in the world where scientists, engineers and technicians devoted entire careers to developing and characterizing nuclear explosives. Moreover, the weapons community had a long and successful track record of designing and testing very functional nuclear devices. The many successful nuclear tests that Los Alamos conducted during the Cold War provided ample evidence that *both* the weapons and their creators "worked," that weapons experts could reliably be expected to produce functioning devices and detonate them without mishap in the Nevada desert (Gusterson 1996, Pinch 1991).

To a great extent, the laboratory's expertise, its competence and credibility, underwrote nuclear confidence. However, confidence was not simply a quantitative estimate of weapon performance. The laboratory did not create confidence by testing a statistically significant sample of weapons in the stockpile and placing error bars around different aspects of weapon performance. Rather, weapons experts relied on their own experience in developing a wider body of weapons-related knowledge to make judgments about the performance characteristics of their designs. Traditionally, weapon designers in X Division have played a lead role in making these judgments, and, as such, had extensive responsibilities in relation to the stockpile. Not only did they pursue new weapons concepts, they identified problems with existing designs and developed experiments – both nuclear and non-nuclear – to address them.

In an ongoing effort to characterize the complexities of weapon physics, designers generated questions, collected and studied data, and generated more questions to be explored in further tests. As the integrators of knowledge, the design community represented the beginning and the end of the laboratory's experimental loop, and to a great extent acted as the voice of nuclear confidence. Confidence in the stockpile rested in great part upon the credibility of the laboratory's designers. Their credibility, in turn, emerged from a complex interplay among many parties: the designers, their predictive computer simulations, or models; the devices themselves; and an enormous multidisciplinary community of experts who realized the designers' ideas as working devices. Produced in a nexus of relationships among experts and machines, nuclear confidence was not simply a body of received knowledge, unveiled in the course of careful scientific probing. Rather, confidence was a deliberate product of the weapons community's collective research efforts. As I discuss below, in designing, building, and testing nuclear devices, and working data into further experimental iterations, the

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laboratory – not just weapons physicists, but *the entire weapons community* – very actively constructed confidence.

Because confidence was so deeply rooted in the experience, competence and collective expertise of the weapons community, to explore how this community established knowing relationships with these devices is simultaneously to understand nuclear confidence as constructed knowledge. When I describe nuclear confidence as a "construction," I am not making a radical challenge to the epistemological status of weapons science. I do not want to imply that confidence was feigned, or illusory, or purely the result of social negotiation, nor do I want to challenge the content of weapons experts' claims to truth (Held 1996: 202; Cole 1996: 206-280). To do so would be patently ridiculous, even "grotesque" (Gusterson 1996: 225), given that the destructive power of nuclear weapons has been amply demonstrated in hundreds of nuclear tests and, most poignantly, in Hiroshima and Nagasaki. Rather, I use "construction" to emphasize the active role of weapons experts themselves as knowing, purposeful subjects who continuously reinscribed confidence through action: that is, in the process of designing, engineering, and testing experimental nuclear explosives.

THE SOCIAL CONSTRUCTION OF NUCLEAR CONFIDENCE

Experts at Los Alamos produced many credibly threatening weapons, and an immense body of weapons-related knowledge, through hundreds of iterations of a local experimental cycle that consisted of designing, engineering and testing prototype nuclear explosive devices. (see Congressional testimony and white paper briefs by Hecker 1987, 1990; Birely 1987, White 1987a, 1987b, 1988). The laboratory's design and test cycles provided experts the chance to develop an enduring, active, and knowing relationship with nuclear explosives, a relationship through which confidence emerged.

The laboratory's experimental "design-build-test" model for knowledge production has its roots in World War II's Manhattan Project. In fact, it is not inaccurate to describe the wartime project as the first iteration of the design and test cycle that would continue to structure postwar knowledge production activities at Los Alamos throughout the Cold War. Nuclear physics was a highly theoretical field in 1943, and the scientists and engineers who came to Los Alamos during the war had little experience in developing engineering processes that would transform physics principles into material artifacts. However, the exigencies of the wartime project required that they produce a working, reliable artifact in a very short period of time, a pragmatic goal whose fulfillment depended upon the rapid translation of very general physics principles into material form. This goal, in turn, required that the project's organizers had to gather together many different kinds of experts, including ordnance technicians, theoretical physicists, mathematicians, engineers, chemists and others, creating multidisciplinary research teams organized around the invention of a functional nuclear explosive. As Lillian Hoddeson points out, the project's pragmatic orientation meant that the success of both experts and artifacts was tightly bound to a material demonstration of the device's performance (Hoddeson, Henriksen, Meade and Westfall 1993: 403). The Trinity test, which took place on July 16th, 1945, yielded approximately one thousand tons of explosive power and provided a very tangible demonstration of the project's success. As such, it also provided epistemological justification for the wartime laboratory's multidisciplinary, practical, tinkering, science-and-engineering approach to invention (ibid 1993: 404-407).

Throughout the Cold War, the laboratory relied on this experimental cycle to answer a portfolio of questions about nuclear weapons; and in doing so, established an enormous body of sophisticated knowledge concerning the workings of nuclear explosives. A single iteration of the laboratory's design and test cycle might begin with an idea for a new design, or a modification to an existing design; a question about the effects of a nuclear explosion on a particular piece of military hardware, or the performance of a stockpiled design under hostile conditions. Most tests were so called *physics experiments* for weapon development that occurred at various stages in the long process of creating and certifying a new nuclear device for military purposes. However, the DOE's weapons laboratories also supported the Department of Defense's Defense Nuclear Agency (DNA), which conducted a parallel *weapons effects* research program to gauge the impact of nuclear explosions on military hardware and equipment, both American and that of other countries. The DNA established its own research priorities, but the weapons laboratories helped devise appropriate experiments, designed the nuclear explosive for the experiment, and provided field support at the Nevada Test Site. The laboratories also conducted *safety and security tests* that allowed weapons experts to certify that their devices would not accidentally detonate, dispersing environmentally toxic plutonium and other hazardous nuclear materials. Less frequently, the DOE conducted *confidence tests* of systems already in the stockpile to ensure that nuclear explosives would perform to certification (Wolff 1984: 1; Brown 1986: 1-3; Norris and Cochran 1994: 12-17).

In the course of developing a new concept, modifying an existing one, or solving a physics problem, designers would identify one or more questions about the processes taking place during an explosion. How did the materials in the device move and change? Where and when did the radiation flow? At what point would the secondary begin to implode? During the initial stages of an experiment, computers were the primary tools for exploring these kinds of questions. Designers relied on massive calculational codes to build one- and two-dimensional visual models of the processes under study.[6] By comparing several models generated with different codes – designers refer to this as conducting "numerical experiments" (Hendricks 1994) – they could fine-tune their predictions about a particular aspect of device physics. To a point, that is: even the most elegant model would require some form of real-world validation in the form of a test (LANL 1976: 4).

Nuclear tests were not the first stage of empirical validation, although they were certainly the most dramatic. Rather, the validation process often began with a less expensive, less risky *hydrodynamic test*,[7] a high explosive experiment that would provide

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[6] Towards the end of the Cold War, design physicists were beginning to develop three-dimensional models as well, although the laboratory only recently ran a full 3D simulation of a design problem. Completing the model required over six weeks of time on the laboratory's massive computers, which are currently among the fastest and most powerful machines in the world (Fleck 2000).

[7] In engineering, *hydrodynamics* is the branch of study that deals with the movement of fluids under applied forces.

a limited empirical benchmark for the designer's model. Often referred to as "local shots" because they were conducted at Los Alamos proper, hydrodynamic experiments approximated full-scale nuclear tests insofar as they tested a mock-up of the design under development.

Compared to a full-scale nuclear test, doing a local shot during the Cold War was a relatively simple process that moved from concept to test fairly rapidly, within a few months. When a designer or a design team decided to pursue a hydrodynamic experiment, they in effect moved their experiment from the confines of X Division into the wider laboratory community. They did so by issuing a "design release" that served notification to laboratory to begin preparing for a new test, drawing together experts from several different weapon divisions. Experimental physicists, engineers and technicians reviewed the designer's goals and developed appropriate diagnostics: detectors, imaging equipment and recorders to gather and capture data about the detonation. At the same time, weapon engineers, technicians and machinists from the weapon engineering groups worked with the designer to turn the experimental concept into a working high explosive device. Finally, firing-site personnel at Los Alamos staged the test, which could send a loud boom rumbling through Los Alamos' canyons and mesas. After the shot was over, the designer and diagnostic physicists would collect and review the resulting data, while the experimental team that had fielded the experiment disbanded and its members moved into new projects.

Local shots used no nuclear materials, just high explosives and inert components, and returned only data about implosion dynamics: e.g., how would a particular part move and change as the high explosive detonated? As such, they did not provide the level of validation that a designer might seek in a full-scale nuclear test. However, they provided designers with an important source of data to begin checking their models. In cross-comparing different simulations, and in comparing these simulations with hydrodynamic data, designers "normalized" their models by "turning knobs" within the codes (LANL 1976: 5).

"Knob-turning" and "normalization" are metaphors that describe a critical aspect of the design process and require some explanation. Paton and Meyer (1992) have observed that experts within a knowledge system often develop a set of shared metaphors to describe their work. Pulling apart metaphors, they suggest, offers a way of grasping the underlying logic of a particular epistemic community. In this case, the metaphor is quite obviously that of a radio or a television set. When designers talk about twiddling a knob to normalize a code, they are adjusting the model to bring it into closer alignment with observed data by adjusting one or more parameters in a code, just as I might turn a knob on my stereo to improve reception of a radio station.

There is a catch here: only a few areas of device behavior can be used as knobs. With over 50 years of testing, the weapons community has thoroughly documented many aspects of weapon physics. As several of my interviewees told me, no area that is scientifically well-characterized can be used as a knob, since a designer who did so would be knowingly introducing error into the model to make it fit the observed results. However, there are several areas of high-explosive and nuclear physics that remain imperfectly understood; and being imprecise, they are adjustable. One designer described knobs as highly sophisticated fuzziness: "You don't know exactly what's going on," he said, "but you've got a hunch that if you tweak a knob, the model fits the data better. You can't explain exactly why it fits. It's intuition." Another designer told me, "Knobs are a very useful kind of ignorance."

Between tweaking the knobs in one or more codes, and making adjustments to the design itself, a designer could bring the performance of the model into closer alignment with the observed performance of the hydrodynamic device – to a point. Hydrodynamic tests and cross-comparisons of models only provided a certain amount of useful data. To get at the subatomic physics of a nuclear explosion, the designer or design team would eventually have to pursue a full-scale nuclear test, to be conducted in the wide alluvial plains of the Nevada desert.

Despite the fact that designers relied heavily on nuclear tests for empirical validation of their models, it was not easy to get a nuclear test, particularly as the Cold War wound to a close. No matter how much effort they put into preparing an experiment, designers never had a guarantee that a concept would make it onto the laboratory's shot schedule. For one thing, tests were expensive, running into tens of millions of dollars per shot. Hence, X Division chose its experiments carefully, and peer reviews among designers could be intellectually and emotionally intense. One of Gusterson's interviewees talks about seeing "men all in tears" during peer reviews at Livermore (1996). I never encountered tales of tears, but I have read several brutally critical design reports, and a couple of my designer interviewees talked of factions, controversies and enmities that developed around shot proposals.

Peer review from X Division was only the first step in getting a shot onto the schedule. Primary and secondary designers might spend years reviewing their ideas, refining supporting models, and marshalling collegial support for their experiments, but there was never a guarantee that even a well-researched, well-supported shot proposal would win approval for a test, because each experiment also required approval from Laboratory senior managers, from the Department of Energy, and from the President, who issued the final “detonation authority” that authorized the weapons laboratories to conduct each test. Moreover, as nuclear tests became widely contested towards the end of the Cold War (Gusterson 1996), it was increasingly difficult for the DOE to maintain a high level of test activity. Getting a shot approved, then, required more than a carefully considered design supported with empirical data and credible models. The experimenters also had to justify the significance of their project within one or more of the laboratory’s core programs – weapon safety, for instance, or the Strategic Defense Initiative.

However, once the laboratory’s senior weapon managers had approved an experiment for inclusion on an upcoming shot schedule, X Division issued a “design release” for a full-scale nuclear shot. Usually, the designers would already have consulted with experts in other areas of the laboratory long before this point, but the design release announced to the community that a concept was ready for development into a full-scale experiment. As such, it created a de facto timeline that mobilized and organized the efforts of hundreds of staff members in preparation for a test. Test preparations required carefully coordinated effort among many people, as the design team drew upon the expertise of various subcommunities in the laboratory to turn its plans into a full-scale nuclear event. A very complicated test, one that entailed a set of interrelated experiments, could entail several years of preparation from the design release to the actual test shot.

To obtain their data, the design physicists relied heavily on the expertise of diagnostic physicists, who reviewed the design release and chose appropriate detectors, fiberoptic cables, and oscilloscopes based on what data the designer required. Frequently, highly complex tests would require that the diagnostic physicists tinker with established detectors, or even come up with new ways of collecting data, so that the designers would get the empirical feedback they required from the test.

As the diagnostics were under development, the laboratory's weapons engineers reviewed the device specifications with the designers to get a sense of their experimental intent, then worked closely with other engineers, machinists and technicians to determine if the designer's intent could be successfully translated into a working nuclear device. Occasionally, designers received feedback from the engineering groups that indicated problems with the device specifications. When this happened, designers and engineers would sit down and negotiate alternatives that would allow the device to be built without sacrificing the designer's experimental intent (LANL 1976: 7). Throughout this process, the engineering team also maintained open lines of communication with the diagnostic physicists, who often requested that the engineers include sensors and tracer materials in the device as it was being built.

As the experiment was taking shape among the designers, diagnosticians, and engineers, the laboratory's field test personnel – the people who actually deployed the shot in Nevada – were also making preparations for the test. At Los Alamos, mechanical engineers in the field test groups consulted with the designers, weapon engineers and diagnostic physicists to create a rack for each test. This rack was enormous steel frame with a metal canister at its base to house the nuclear device and any accompanying experiments. Above the canister, the rack engineers designed platforms, niches and holes to house several tons of accompanying equipment: diagnostic detectors, fiberoptic and/or coaxial cabling to transmit diagnostic data to the surface; power cables; electrical wiring to send signals that would arm and fire the device. They also designed and machined a harness of massive wire ropes that would be used to lower the rack and all its attachments thousands of feet into the test hole after the experimental equipment was set up.

Figure 3-1. Setting up test rack at the Nevada Test Site. Note bundles of cabling attached to rack. Photo Courtesy Los Alamos National Laboratory.

Meanwhile, at the Nevada Test Site, Los Alamos maintained an extensive complement of test personnel. Some were employed directly by the laboratory, while others were employed by the Department of Energy or one of several subcontractor engineering and construction firms. These were the people who actually put together and fielded the experiment; and as the day of the test drew closer, they became increasingly involved in planning the test (Wolff 1984; Machen 1988).

The laboratory's containment experts were among the first of NTS personnel to become involved in a test. The containment team usually consisted of one or more nuclear physicists, geologists, geological engineers and technicians from Los Alamos and Nevada. This team reviewed the designer's predictions for device performance, and used these predictions to choose an appropriate hole that could "contain" the explosion and prevent the release of radioactive material into the atmosphere. They also designed a stemming plan – layers of fine and coarse filling material and epoxy plugs to fill the hole after the device was inserted, and to prevent radioactive material from venting to the surface after the test was completed (United States Congress, Office of Technology Assessment 1989).

Safety studies were also a significant part of test preparations. Test planners submitted a detailed summary of their program to two panels: a Containment Evaluation Panel (CEP), which assessed and approved geological and engineering plans for containing the explosion underground, and a Nuclear Explosive Safety Study, or NESS, which reviewed every step of the test involving nuclear explosives, to minimize the possibility of an accidental nuclear explosion before the scheduled test. Both the CEP and the NESS were composed of experts from several federal agencies, to ensure a thorough and impartial review of the containment plan and test operations.

Test preparations grew increasingly intense at the test site as the date for the shot drew closer. Approximately three to six months before the test was scheduled, NTS event engineers started preparing the construction site around the hole: bringing in heavy construction equipment, such as cranes and bulldozers; ordering and setting up trailers to house arming and firing systems, computers, and data recording instrumentation; receiving equipment from the laboratory and other subcontractors who provided equipment for the test, and setting up the rack, which would be suspended in the air above the hole by a massive crane.

Figure 3-2. The test rack, suspended here by crane, is housed in a tower until the device is emplaced. The cabling is snaked between the trailers (outer edge) and the tower until lowered into the ground. Photo courtesy Los Alamos National Laboratory.

Figure 3-3. Close-up shot of tower housing test rack prior to device emplacement. Photo courtesy Los Alamos National Laboratory.

After setting up the rack, the NTS event engineer stepped back to let the diagnostic teams, engineers and other technicians install their equipment onto the rack. A particularly complicated test could require several months of on-site preparation. However, once the diagnostic equipment and experiments were installed, it was time for the insertion of the nuclear device into the rack and the emplacement of the test rack into the ground, a "point of no return" in the test process. Just before this point, a team of assembly engineers flew from Los Alamos to Nevada and assembled the full nuclear explosive in a special bunker at the Nevada Test Site, to avoid transporting a full-scale device across long distances. Once assembled, the device was transported under heavy security to the hole and inserted in the canister at the bottom of the rack. The event engineer then supervised the lowering, or "emplacement" of the rack into the hole, checking the cables and electrical connections as he did so and stemming the hole according to the containment plan. There could be no interruption in these operations, since the device would only be secure from tampering or theft once it was buried underground.

Figure 3-4. Crane lowering rack, device and cables into the test hole. Photo courtesy Los Alamos National Laboratory.

After the device was emplaced and the hole stemmed, the next few weeks before the shot would be filled with dry runs, as different teams checked and re-calibrated their equipment.

Shot day was the apex of the design and test cycle. The device would be detonated from the Control Point, a heavy concrete bunker located on a hill above the floor of the desert valley. Firing the device consisted of sending a microwave signal to computers in one of the trailers at ground zero, which unlocked the system, sent a high voltage charge to the firing unit, and detonated the device.

Figure 3.5. Los Alamos staff at Control Point, NTS. Remote cameras allowed test staff to monitor the shot from a safe distance. Photo courtesy Los Alamos National Laboratory.

A large explosion could send an enormous wave rippling through the desert floor, sending the data recording trailers at ground zero into the air and warping the asphalt highways that crisscross the test site. More visually startling was the sudden collapse of the desert surface above the point of the explosion. Usually, but not always, a massive subsidence crater appeared in the wake of the test, as the underground cavity formed by the explosion cooled and collapsed from the weight of the earth above, pulling the surface down with it. Crews usually waited for the crater to form before attempting to pull the data-collection trailers from ground zero, since the subsidence could occur without warning.

Figure 3-6. Packaging drillback samples for laboratory analysis. Photo courtesy Los Alamos National Laboratory.

Later, after the cavity had cooled, radiochemical analysts from Los Alamos supervised the drillback, which consisted of drilling a long shaft at an angle to the underground cavity and sampling the debris inside. The drillback was a kind of postshot forensics that helped the diagnostic physicists and chemists more accurately reconstruct the processes of the explosion.

Nuclear tests did not take place in a political vacuum. Rather, the DOE's laboratories had a powerful audience of defense analysts, politicians, funding agents, and decisionmakers who tracked the experimental progress of the nation's nuclear weapons laboratories. Immediately after the test was completed, Los Alamos' designers and diagnosticians pored over the initial data and hurriedly authored a preliminary report that provided high-level political and military parties, including the President, with a sketchy account of the test results. Later, the experimenters returned to the laboratory to begin the long task of analysis: assessing the quality of their data, using the empirical information gathered during the test to characterize the device under development and to refine theoretical models, defining new questions, and – as necessary – using the data to prepare for further tests. Occasionally, if the designers and diagnosticians had time and funding to do so, they would use this data to author a more detailed postshot report for the laboratory and the DOE. Postshot reports usually consisted of between forty and fifty pages of technical review detailing the entire experiment, weighing its successes and failures, discussing any important spin-off benefits – for instance, improvements in a particular diagnostic method – and posing possible follow-on research questions.

There were other channels of communication that advertised the laboratory's research prowess to military and political decisionmakers. At the end of every fiscal year, the Department of Energy requested summary discussions of all nuclear tests. These summaries were distributed among managers and researchers in the laboratories as well as policymakers at the DOE and the Department of Defense. In addition, all three of the DOE's weapons laboratories published classified research journals filled with scholarly accounts of various aspects of the weapons programs. These served as important channels of communication among researchers at Los Alamos, Sandia and Livermore, as well policymakers at DOE and DOD headquarters. Classified conferences, such as the semi-annual Nuclear Explosive Design Physics Conference, or NEDPC, brought together weapons experts from Los Alamos, Livermore, Sandia, and the British design laboratory at Aldermaston.[8] These conferences allowed weapons personnel from both countries to present and critique each other's projects and provided a platform for the weapons programs to discuss their experimental progress before an audience of military and civilian funders and policymakers.

Throughout the Cold War, the laboratory's experimental cycles waxed and waned but never really stopped. Every test shot had its own pace although each followed a similar pattern: from an adagio in the designers' earliest conceptual stages, the pace of activity gradually quickened to an allegro as test preparations drew different experts from throughout the laboratory into the project. As the shot date grew closer, the designers, diagnostic physicists, rack engineers, NTS field staff and other experts narrowed their focus to concentrate on the upcoming event. The intensity of work peaked in the days leading up to the shot, reaching a crescendo on shot day, and ebbing away as the experimenters gathered their data and returned to the laboratory to mull over the results of the test. There was always another test just on the horizon, another iteration of the design and test cycle reaching fruition: iteration after iteration, like a series of waves, slowly rising and building towards an end point, then breaking into memory to make

[8] Britain lacked an appropriate site to conduct nuclear tests, so throughout much of the Cold War, the United States allowed the British nuclear program to use the Nevada Test Site, while the DOE's laboratories provided the British nuclear program with technical and logistical support for its tests.

space for the next event. There was a constant flow of work, a re-cycling of the same process, year in and year out, so that the activities and skills involved in testing were constantly being exercised on the various experiments that were ongoing at any one point.

THE DESIGN AND TEST CYCLE AND THE INTEGRATION OF KNOWLEDGE

The laboratory's experimental cycles served obvious scientific functions, generating empirical data about device behavior, and helping designers – in particular, but also experimental physicists, weapons engineers and other experts – refine their understandings about the workings of nuclear explosives. However, the design and test cycle also served a portfolio of social functions that were intricately connected to the production of nuclear confidence.

In this regard, it is important to realize that nuclear confidence was, and remains, largely dependent upon the competence of the experts who designed, engineered, tested and made judgments about nuclear devices. This relationship between confidence and competence has significant implications for understanding how the laboratory produced nuclear confidence: perhaps most importantly, the laboratory's ability to produce nuclear confidence was directly related to a) the efficacy whereby it reproduced competence in each of its expert subcommunities – from design physicists to weapons engineers – as well as b) the fluency with which these experts could integrate their many bodies of knowledge to produce a working set of artifacts.

The experimental cycles of the Cold War produced far more than nuclear explosives for bombs and missiles. Rather, every iteration of the design and test cycle allowed the weapons community to reproduce itself by producing a heterogeneous, yet integrated, body of knowing selves. This topic provides the theme for the remainder of this chapter. I explore the weapons programs as a complex knowledge environment requiring the involvement of many different kinds of experts, each with a particular set of skills and understandings shaped according to their role in the testing process. Within this environment, the weapons community faced a consistent challenge: how to communicate the purpose of an experiment across a vast and heterogeneous population of experts, so that ideas were accurately transformed into working artifacts. I explore the processes of negotiation through which weapons experts solved this problem, paying particular attention to the vehicles used to communicate ideas throughout the weapons community.

It is important to note that this negotiation did not occur haphazardly. Rather, locally significant constructions of time played an important role in the translation of intent in two distinct ways: by providing common language in which to negotiate meaning, and by providing a pace, a tempo, that coordinated action among participants in the design and test cycle. Lastly, I close with some reflections on the mutually constituted nature of nuclear devices, nuclear confidence, weapons experts and the weapons community as a whole, arguing that, as a unified enterprise, weapons science is a kind of knowledge that "sits in places" (Basso 1996): namely, in the hundreds of craters scattered across the arid lowland desert of the Nevada Test Site.

The Structure of the Cold War Knowledge Environment

The process of designing and testing a nuclear device was an extraordinarily complicated one that involved many different kinds of experts, each with a particular set of responsibilities within the laboratory's experimental cycles: physicists, pipefitters, engineers, drillers, technicians, geologists, machinists, chemists, metallurgists, electricians. Given the complexity of the field, I can safely say that no single individual understands everything there is (or was) to know about the designing and testing of nuclear weapons. Rather, as Bob Simpson has pointed out, "…knowledge, like so much else in society, is socially distributed…. [In complex societies] individuals participate in a partial and inchoate project in which knowledge and access to knowledge are variably distributed and expressed" (Simpson 1997: 44). This observation, made in a discussion of ritual change among the Berava drummers of southern Sri Lanka, is quite apropos of the laboratory's weapons community, where hundreds of staff members worked semi-autonomously on different aspects of a much larger experimental project.

The social distribution of knowledge in the Cold War weapons community was explained to me during an interview with Larry, an upper-level manager in the weapons programs. A radiochemist by training, Larry had spent most of his career working in the Los Alamos testing program, designing experiments that used tracer materials in the nuclear device to characterize the transformations that had occurred during the explosion. His work required that he spend a great deal of time in Nevada, supervising the drillback to sample material from the underground cavity and analyzing the debris for evidence of various nuclear processes. Like many senior weapons experts, Larry's chosen career path was abruptly truncated when the testing program ended, and in the wake of the Cold War's end, he became deeply concerned about the idea that Los Alamos might lose most of its expertise. By the time I met him in 1999, he was supervising a portfolio of efforts across the laboratory to capture and archive as much data as possible from the Cold War testing era.

Sitting in his office one gray and chilly January morning, I asked Larry to explain to me some of the challenges the laboratory faced in archiving its knowledge. He replied, "Figuring out who needs access to which information, and how to give it to them," and pulled from a drawer in his filing cabinet the "Spheres of Need-to-Know" diagram (see following page). In the weapons programs, he explained, “need-to-know” is operationally synonymous with the compartmentalization of weapons-related knowledge, insofar as individuals working on a particular project are only allowed access to the information they require to fulfill their role in that project. During the Cold War, the transfer of necessary information among groups, and the boundaries that separated the various areas of expertise, occurred and were maintained as part of the process of designing and testing a nuclear device.

For example: when designers required radiochemical data to diagnose the performance of their device, they provided Larry and his team with the necessary information about their project, so the radiochemistry team could arrange the proper radiochemical experiments and analysis. Larry emphasized that the radiochemistry team did not require access to all the designer’s information, only a small portion of it. The designer, on the other hand, needed to know a great deal about the radiochemistry team’s experiments, if the design team was to make sense of the data returned from the shot. Hence, in Larry’s “Spheres of Need-to-Know” diagram, the radiochemist’s sphere of activity sits well inside the designer’s sphere, but not vice versa. As Larry explained, “I didn’t need to know everything about the design team’s work, but they needed to know a lot about mine if they were going to use my data in their calculations.”

During the Cold War, the segregation of information occurred informally in process of designing and testing a nuclear device: as they engaged with each other to solve particular experimental problems, groups necessarily shared whatever information

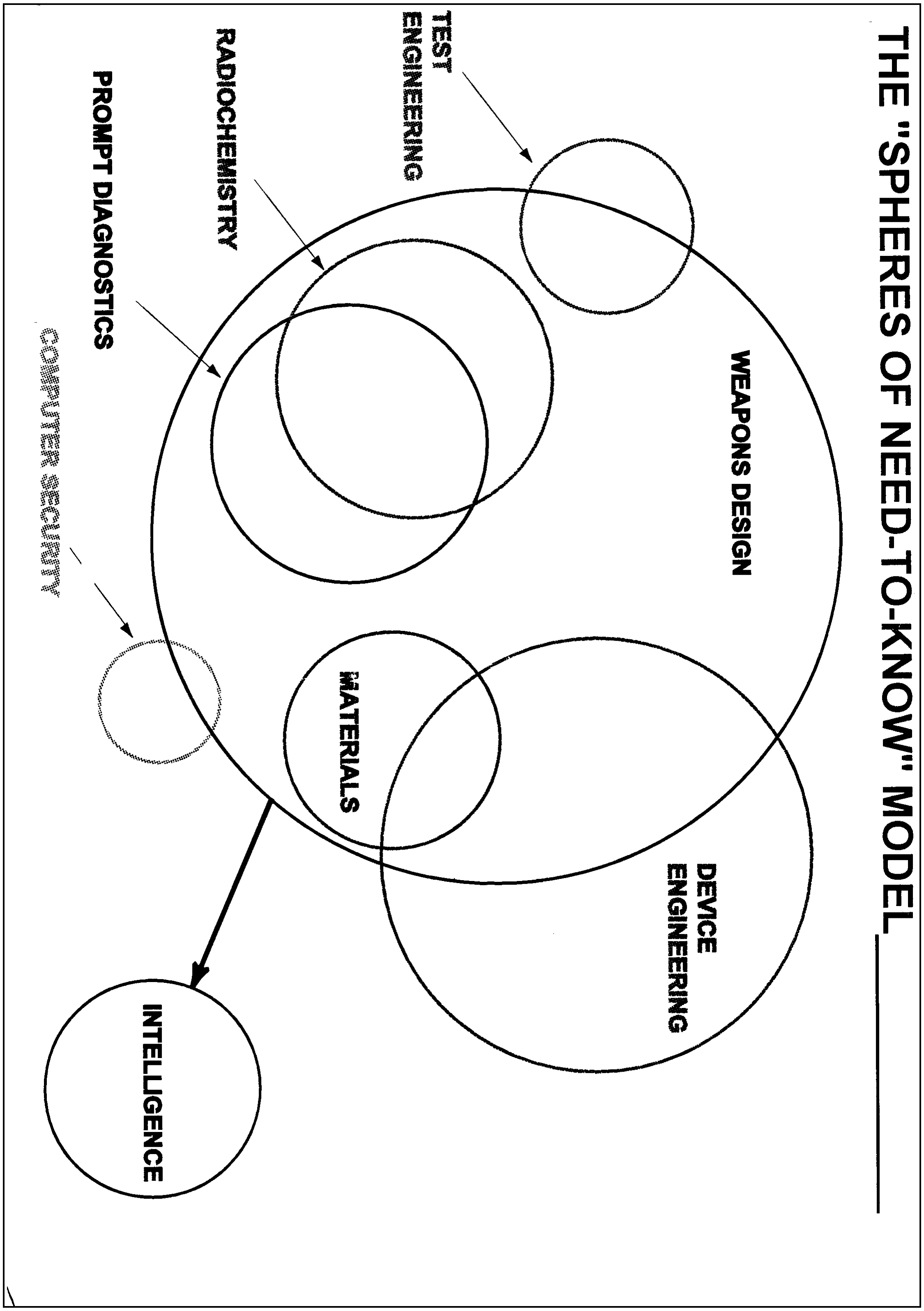

Figure 3-7. "Spheres of Need-to-Know" Diagram.

they needed to share in order to complete their experiments, and tended not to share information that was superfluous to the completion of a particular task. The problem in the post Cold War era, Larry explained, was formalizing the informal need-to-know boundaries that had existed during the days of designing and testing nuclear explosives.

As I listened to Larry explaining his need-to-know problem, pointing at spheres on the diagram and describing the relationships that had connected each of these disciplinary areas during the Cold War, I realized that he was describing not just the segregation, but also social distribution and *integration* of knowledge in the weapons programs. The boundaries of each circle in the diagram demarcate the responsibilities of different subcommunities within a single iteration of the design and test cycle, illustrating the information that different individuals and/or groups of experts *needed to know* in order to fulfill those experimental responsibilities. As the spheres indicate, certain subcommunities at Los Alamos had a wider range of understanding – and responsibility – than others, simply by virtue of their roles in the weapons programs.

The relationship between diagnostic physicists and nuclear weapon designers provides a good example of this point. The largest circle in Larry's Venn diagram belongs to designers in X Division. As the integrators of experimental and theoretical knowledge, the designers' field of vision was widest because their work was holistic. Designers followed their concepts through every stage of translation, making sure along the way that their experimental intent was adequately translated into working material artifacts. As such, the designer's role required familiarity with the roles and responsibilities of many other staff members, including that of the diagnostic physicist.

Hypothetically speaking, a designer trying to answer a question about explosive yield might request that the diagnostic physicists provide a particular array of detectors. But making that request required some familiarity with the knowledge area of the diagnostic physicist: what kind of tools are available for gathering particular kinds of data, any equipment limitations that might affect the quality of data, etc. However, this does *not* mean that the designer was a competent practitioner of experimental physics. With his fuller expertise in designing and fielding data collection systems, the experimentalist might answer the designer's request by modifying an array to capture the data more elegantly.

It is important to recognize also that the relationship between the designers and the diagnosticians – or, for that matter, any other expert subcommunity – was rarely fully reciprocal. For example, in order to obtain the data they required, and to judge their quality, designers had to know a great deal about the diagnostic array being fielded in an experiment: e.g., its performance, timing, any limitations to the data returned by the diagnostic array. In contrast, the diagnostic team fielding the detectors did not require equally comprehensive knowledge of the designer's responsibilities in order to fulfill their data collection responsibilities successfully. Moreover, diagnosticians could reap benefits even from a test that did not perform to a designer team's expectations. For instance, a diagnostic team that successfully modified an array of detectors might have great success with their modifications, collecting data that perhaps had been difficult to gather in previous tests. However, those data might quite clearly indicate a problem with the device. In other words, a test that sent a designer back to the proverbial drawing table could be seen as a resounding success for the laboratory's experimental physicists, because it provided an opportunity to successfully expand the range of diagnostic equipment and techniques available to the weapons community.

Designers did not build their own devices, or field their own tests. Instead, they had to rely on the competence of many other experts within the weapons community to a) grasp the purpose of the experiment and b) translate it into a set of functional artifacts – not just the nuclear explosive device, but a vast array of accompanying apparatus. Moreover, a single test could include several different kinds of experiments "driven" by the energies of a nuclear device, facilitating in one fell swoop the development and furtherance of knowledge in many different fields, from experimental physics to rack engineering. As such, tests often represented the convergence of many different interest groups: designers, nuclear chemists, experimental physicists, all coordinating their work so that a single test could provide data to answer several interlocking sets of questions.

The Translation of Intent, Boundary Objects, and the Negotiation of Meaning

Like any multidisciplinary team organized around a central task, the weapons community faced a complex challenge: the fluent communication of knowledge, as well as the successful integration of many different "ways of knowing," in the course of solving a complex set of interrelated problems. Broadly speaking, each iteration of the design and test cycle brought the entire weapons community face-to-face with the same problem; that is, the *translation of intent* across many different groups of experts. In relation to the nuclear device itself, this burden rested most obviously with the laboratory's weapon engineers, machinists and technicians: in order for the designer to analyze the alignment between predictions and empirical data, the experimental device had to accurately represent the system being modeled, and vice-versa. The success of a test, then, rested not just with the designer, but on the ability of a multidisciplinary team of experts to grasp the designer's intent – as expressed in the design release, in memos, in drawings, in discussions – and translate it into a set of exquisitely timed and choreographed working artifacts.

The translation of intent was critical in all aspects of the design and test cycle. Diagnostic physicists, for instance, had to understand at a fundamental level which data the designer required, so that they could choose the proper diagnostic instrumentation, locate it in the rack, and time it to function at a precise moment in the split second implosion. Similarly, the diagnostic physicist relied on rack designers, engineers, and technicians to translate diagnostic intent in the process of engineering and machining specialized data collection equipment. In Nevada, containment physicists, geologists and engineers reviewed the design release, developed a sense for what the designer expected from the test, and created a suitable containment plan to prevent the release of radioactive material in the atmosphere. Geological engineers then translated the geologists' intent into a step-by-step process and materials for stemming the hole after the device was lowered into the ground.

As I talked with designers, engineers, and diagnostic physicists about their interactions, and later watched engineers solving experimental problems, I gradually came to think of the translation of intent as a problem of negotiating meaning, as experts from various groups engaged with each other to develop a consensus on the many technical issues related to an experiment. When I refer to the translation of intent as a process of negotiating meaning, I am thinking specifically of Etienne Wegner's assertion that meaning is a necessary condition for the production of knowledge, in the sense that none of us can engage deeply with any particular aspect of our worlds – be it an idea, a person, a document – until we can locate it in relation to a wider context, so that it resonates with our lived experience (1998: 51-53). At Los Alamos, if an experimental concept were to come to fruition in the form of a full-scale nuclear test, it would have to be meaningfully grasped and realized in the work-experiences of many other experts.

Since there is no direct way of communicating a concept among individual minds – short of mental telepathy, of course (Hutchins 1996: 60) – the problem of translating intent becomes one of devising representations that have the "capacity to embody processes occurring at one time and place in a form that can be reproduced in another" (Suchman and Trigg, 1996: 146). Latour (1987: 227; 236-237) refers to this property as "immutable mobility," meaning that the representation fixes the emergent knowledge in a transportable form: as a drawing, a proposal, a model, a set of equations. These representations of knowledge are a critical vehicle for capturing and conveying meaning across people, places and time periods. Following Wegner, I refer to these representations of knowledge as *reifications*: objects that individuals create to congeal the experience of knowing into thing-ness. Reifications are "evocative shortcuts [that represent]…the tip of an iceberg, which indicates larger contexts of significance realized in human practices" (Wegner 1998: 58, 61).

In the multidisciplinary, heterogeneous environment of the laboratory, reifications must convey meaning not just among members of a homogeneous knowledge community (e.g., physicists, mechanical engineers), but across disciplinary boundaries –

for instance, from a Ph.D. theoretical physicist to a machinist who may have no formal education beyond high school. In this kind of task setting, reifications become “boundary objects,” or representations that fix and carry active human cognition from one epistemic community (such as that of the weapon designer) to another (the weapons engineers or a high explosive machinist). As a kind of reification, boundary objects can take many forms: for example, they can exist as material objects, as paragraphs on a page, three-dimensional models, or drawings.

One good example of a boundary object was the design release that X Division sent to the weapons community when decided to conduct an experiment. Using narrative discussion, images from computer models, and numerical equations, the designer or design team created the release to fix experimental ideas in a stable form. This form emerged from X Division as a vehicle for communicating intent to other knowing individuals who would be asked to contribute their expertise to the experiment: diagnostic physicists, assembly engineers, containment geologists. During the Cold War, the design release also sent a signal to the larger weapons community that another iteration of the experimental cycle was about to begin.

However, merely sending out a design release did not complete the process of transmitting meaning from the designer to other groups in the weapons community. As Collins has pointed out, if scientific knowledge were truly universal, it would be easy to replicate experiments using explicit instructions alone. However, the actual “doing” of science requires a great deal of tacit knowledge and skill that cannot be completely encoded in explicit format (1992). No matter how detailed, the design release was of limited utility, because no reification can capture the entire base of tacit understanding that the designer has drawn upon in developing her/his concepts; nor can it anticipate or answer any questions that other experts – diagnosticians, geologists, engineers – might have as they interpret its message.

This is a significant limitation because different parties had, quite literally, very different ways of knowing the experiment: engineers, for example, think differently about an explosive system than do designers, while diagnostic physicists come to an experiment with a different set of concerns than do the engineering groups. This problem was solved through activity: in discussions and negotiations, both formal and informal, that took place around the design release, as the parties involved worked to arrive at a consensus about the tasks and responsibilities that each expert would have to fulfill to transform the experimental vision into reality. In this process, each expert also created her/his own reifications to communicate intent to other parties involved in the project: engineers, for instance, often built a mock-up of the device in question, to demonstrate to the designer that they had grasped the intent of the experiment. In the process, the engineers also transformed the design release into detailed engineering drawings, and in turn used these drawings to communicate the project's details with the high explosive technicians responsible for machining parts for the device.

The initial design release was critical in the translation of intent and the negotiation of meaning, because it served as a springboard from which other experts, such as the laboratory's weapons engineers or its diagnostic physicists, could begin to negotiate their vision of the experiment with the designers. The design release was a call to participation; it invited knowing selves to contribute to the process that it suggested, to engage meaningfully with the ideas represented in its pages. In doing so, it provided a point for different experts to begin integrating their many ways of knowing: the styles, practices, and sensibilities through which they experienced and acted upon the world around them. Indeed, it is fair to say that a design release was not "alive" in the wider context of the weapons community, unless it became a point around which experts began to engage with each other in the process of solving a particular set of problems. Metaphorically speaking, boundary objects like a design release acted as grains of sand in an oyster, as catalysts to the active engagement of individuals in the ongoing production of meaning.

Time as a Boundary Object

Reifications are not just documents and drawings: they exist in other forms also, as concepts, terms, words "…through which we project our meanings into the world and then perceive them as existing in the world, as having a reality of their own" (Wegner 1998: 58). In this sense, language is a reification that acts as a boundary object: as Peter Galison (1997; in Gusterson 1999: 320) has pointed out, practitioners in multidisciplinary fields (or intellectual "trading zones") must develop locally significant dialects ("pidgins") if they are to effectively transmit and receive ideas across disciplinary chasms. Similarly, Frank Dubinskas has noted that in scientific communities, linguistic representations of time are critical in the negotiation and production of meaning:

> Times are key symbols around which multiple meanings coalesce, and they are embodied in different media like speech and writing, narrative accounts, and argumentative interchanges. These condensed symbols of time provide a kind of 'handle' with which meaning can be grasped, modified, and exchanged with others (Dubinskas 1988: 24).

At Los Alamos, scientific constructions of time were critical boundary objects, insofar as they provided a conceptual point of reference around which different groups of practitioners could begin to negotiate meaning. This kind of time – which I think of as "measurement time" – is similar to the "marker time" that Sharon Traweek identifies among high energy physicists:[9]

> Time in a non-relativistic setting is simply a marker. It is only a milestone marking a sequence of events in space… This space can be any designated, arbitrary spatial reference frame, including a three dimensional world in which humans act… the experiments conducted by scientists in the laboratory occur in a non-relativistic setting; that is, time functions as a marker (1988: 77).

Many of the weapons community's experts, from electricians to engineers to physicists, relied (and still do rely) on marker time to structure and integrate their experimental activities in the design and test cycle. However, I encountered Traweek's marker time most vividly among the weapon designers and diagnostic physicists that I met.

Administratively, diagnostic physicists are housed in the laboratory's Physics Division, or P Division. The frequently hold advanced degrees in experimental physics; yet despite this fact, they tend to align themselves with the engineers and technicians of the field test groups rather than with the theoretical design physicists in X Division. This is partly due to the fact that – unlike weapons design – diagnostics physics is a highly

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[9]Although there are significant differences between the culture of high energy physicists and that of weapons science, both communities inhabit many of the same realms: As Sylvan Schweber has noted, being a weapons scientist requires the "…ability to translate [one's] understanding of the microscopic world into useful macroscopic devices" (Schweber 1992: 174; quoted in Hoddeson et al 1993: 404). Both Traweek (1988, 1996) and Krieger (1992) have pointed out that high energy physics is a largely theoretical activity whose practitioners eschew applied research, particularly anything involving military funding. To that observation, I would add that particle physicists do not routinely destroy expensive experimental equipment in the course of an experiment, nor do they engineer explosive devices that channel massive energies and pressures in precise ways. In contrast, weapons design is a pragmatic, utilitarian enterprise dedicated to military applications. To paraphrase Lillian Hoddeson, it is a field in which abstract topics not amenable to experimental study tend to be ignored; reliability replaces elegance; and several American traditions, including engineering, tinkering, craftsmanship, theory and experimentation, are fused to produce working devices: "the emphasis is on artifacts, not ideas" (Hoddeson et al 1993: 404-415).

experimental, hands-on, empirical enterprise. But it is also historical: during most of the Cold War, diagnostic physicists worked in J Division, the field test organization that oversaw all the laboratory's test operations in Nevada. Even today, diagnostic physicists are housed in the same building as their NTS field counterparts – roughly a quarter-mile away from the Administration building, where X Division resides. Diagnostic physicists played a key role in providing the experimental expertise that designers lacked. In consultation with their colleagues in X Division, the diagnosticians in P Division chose instrumentation to characterize different aspects of an explosion so that the experimenters would get the data they required: pressure, temperature, radiation measurements. Frequently, diagnosticians used these experiments as an opportunity to develop new techniques and sensors for gathering data. Diagnostic equipment could become very complicated, very quickly, particularly if other researchers wanted to piggyback additional experiments onto a test shot. The more complicated tests required that diagnosticians, designers and the shot engineers pay careful attention to spatial and temporal relationships among different experiments and diagnostic equipment, so as to prevent interaction effects from ruining an entire suite of (very expensive) experiments.

Insofar as diagnostic physicists provided equipment to gather data about what happened during a nuclear explosion, their role was a visual one. My forays into P Division, and my conversations with diagnostic physicists, frequently reminded me of Donna Haraway's analysis of vision as a key component of knowledge production in Western science (1991). Visual metaphors abound in diagnostic physics. For example, "line of sight" pipes provided a conduit for energies that flowed from the exploding device, up the pipe, to a detector at the top of the pipe. Not infrequently, these detectors were a type of sophisticated camera. The camera's shutters were designed to be triggered by a specific kind of energy or particles traveling at high speeds from the explosion. Essentially, diagnostic cameras took pictures of different kinds of energy traveling up through line of sight pipes, then sent data up to the surface through a series of fiberoptic or coaxial cables. These were connected to data recording equipment, usually an array of sophisticated oscilloscopes that were housed in a trailer a few thousand feet from ground zero. Diagnostic physicists also used "witness plates," large, flat metal plates covered with detectors, such as fiberoptic pins, and mounted at carefully calibrated distances from the point of the explosion. As a wave of pressure or traveling particles hit the plate, it triggered the detectors, which also sent an electronic signal to the recording trailers on the surface.

Diagnostic physicists and technicians provided the weapons community with an array of visual instruments: carefully calibrated technologies of detection, each of which bore witness to a particular kind of energy or physics process. "The 'eyes' made available in the modern technological sciences," Haraway writes, "shatter any idea of passive vision; these prosthetic devices are active perceptual systems… each with a wonderfully detailed, active, partial way of organizing worlds" (1991: 190). In other words, the information the weapons community relied upon to establish understandings about nuclear devices was highly mediated by the extraordinarily complex technologies used to gather data. Metaphorically speaking, the diagnostic technologies developed by experimental physicists in P Division were like mechanical extensions of the human eye, visual prosthetics that allowed designers to peer down the hole, to "see" different kinds of signals generated during the explosion, and to collect data that could be translated into static visual images – grids, graphs, even photographs – for more careful perusal in the wake of the test.

That weapons science relied on mediated knowledge is not so remarkable; as Gregory Bateson once pointed out, no scientist ever has access to "pure" data:

> Data are not events or objects, but always records or descriptions or memories of events or objects. Always there is a transformation or recording of the raw event which intervenes between the scientists and his object…. Moreover, always in inevitably, there is a selection of data because the total universe, past and present, is not subject to observation from any given observer's position. In a strict sense, then, no data are truly "raw" (1972: xviii).

For me, the most astounding aspect of design and diagnostic physics is the minute regimes of time in which these nuclear visions were created. Designers and diagnosticians worked in a fleeting world in which time was measured in millionths (micro-) and billionths (nano-) of a second. In fifty years of testing, and in over one thousand nuclear test shots, Los Alamos and Lawrence Livermore collected several tons' worth of empirical data about nuclear weapons. Astonishingly, all this data was generated in less than one second, total, of nuclear reaction time. Nuclear explosions occur very rapidly, far too rapidly for direct human comprehension. In nuclear physics, a second is a very, very long period of time. As one diagnostic physicist told me, "I sat down and tried to figure out a ratio that would make sense to you. It was kind of surprising, even to me, when I realized that a nanosecond is to one second, what one second is to thirty years."

Just as sixty seconds make up a minute, ten nanoseconds make up the basic unit of nuclear explosion time: a shake.[10] This is not an arbitrarily designated unit of time. Rather, ten nanoseconds is the amount of time required for one atom to split and emit the neutrons that will cause another atom to split. In other words, a shake is a locally significant temporal construction that measures one "generation" of fissioning atoms. When the weapons community locates the physics processes that occur during a nuclear explosion, it placializes these processes in terms of "shakes" during explosion time. In a nuclear test, the explosion began at "zero time," the point of detonation, when input energy from the firing system set off the high explosive shells that, in turn, set off the nuclear explosive in the primary. The primary then ignited the secondary – if the test included a secondary, that is; many tests did not. Zero-time, then, marked the beginning of the device's transformation from a stable mechanical configuration to a massively energetic, nonlinear series of physics processes. Within microseconds from zero-time, the entire system will "disassemble." Weapons physicists are primarily concerned with the interim period between zero-time and disassembly: as input energy reached the materials of the device, they released different kinds of energies and particles, which moved through and reacted with different parts of the system.

Shakes are used as markers that pinpoint measurable physics reactions in relation to zero time, as well as to each other. For weapons designers and diagnostic physicists, measuring and locating subatomic events in the number of shakes from zero-time provided a conceptual framework against which they could organize their understandings of the complex processes of a nuclear explosion. As a marker, this kind of time served as

[10]I have heard, perhaps apocryphally, that "shake" was coined by one of Manhattan Project physicists from "two shakes of a lamb's tail."

an integrating mechanism to help designers communicate their expectations to diagnostic physicists, who acted on those expectations by placing an array of carefully calibrated detectors around the device and any accompanying experiments. Likewise, diagnostic physicists translated data gathered with their detectors and provided designers with carefully refined information about device processes. Diagnostic results were frequently mapped against precise temporal grids, so that designers could "see" what processes were happening at which points in the brief, quickening moments before disassembly.

Time and the Structure of Knowledge Production

Time can act as a reification, a conceptual point around which members of different epistemic communities integrate their ways of knowing. However, ethnographers who study high technology organizations also emphasize the importance of time as a framework to organize action (Dubinskas 1988, Buccarelli 1988, Traweek 1988): "…time," writes Alfred Gell (1996: 315), "provides the means for the relative unification of otherwise diverse categories of processes." In this sense, time not only acts as a point for the negotiation of meaning; it also provides a tempo, a rhythm, an overarching structure for the coordination of activity.

In her ethnography of high-energy physicists at the Stanford Linear Accelerator (SLAC), Sharon Traweek identifies six different constructions of time that "…organize the laboratory [so that] power in the laboratory is based upon these experiences of time." She groups these temporal constructions into two primary types of time: *calendrical time* is time that slips away; it is ephemeral, "…marking a nearly irreversible sequence of events (such as the decay times of detectors, ideas and physicists)" (Traweek 1988: 76). *Replicable time*, on the other hand, is accumulated over the course of a career in the form of experiments; as such, it represents the base of experience from which SLAC's researchers make claims to expertise.

At Los Alamos, calendrical time beat the rhythm of decay, as days, months, and years marked the passage of temporal forces eating away the longevity of the stockpile. In her ethnography of strategic defense experts, Carol Cohn perspicaciously pointed out that nuclear weapons – and not people – are the active and lively subjects of hypothetical nuclear warfighting scenarios devised by military planners (1987). Similarly, Hugh Gusterson has noted the "startling" pattern of birth metaphors used to characterize different stages in the development of a new nuclear weapon system (1996: 161-163). At no point did my fieldwork experience resonate quite so strongly with Cohn's and Gusterson's than the day when I realized that, all around me, members of the weapons community were describing the stockpile in metaphors that vividly evoked images of a human lifetime. Designers, experimental physicists, engineers all spoke to me about the "lifetimes" of nuclear weapon systems; stressed the importance of maintaining a "youthful" stockpile, worried about determining a suitable "retirement" for "aging systems" without being able to test their "vitality." Metaphors like this are not unique to Los Alamos, but are used throughout the nuclear weapons complex. During the Cold War, weapon systems were only expected to remain in the stockpile for fifteen to twenty years, after which point military planners in the DOD deemed them "too old." Age can affect the performance of materials and parts that make up a nuclear explosive; and, moreover, the requirements of the larger international strategic landscape were constantly shifting. Hence, the stockpile was in a state of constant decay and required continual updating if it – and the nuclear deterrent – were to remain "healthy."

Paradoxically, then, calendrical time at Los Alamos also served as a force for the *renewal* of both weapons and expertise, as the laboratory's weapons experts experimented year in and year out to develop new systems, to maintain the health of the stockpile in the face of its perpetual decay. Weapons designers and engineers at Los Alamos worked closely with military planners in the Department of Defense to determine when a stockpiled system was ready for retirement. At that point, they would remove the system from the stockpile, dismantle it, and replace it with either an updated version of the retired system, or an entirely new weapon system. Because it could take years to certify a new or an updated nuclear device, the laboratory was constantly engaged in a process of renewal; experimenting, refining, researching, developing new nuclear explosives for inclusion in the stockpile. Moreover, as the laboratory re-produced the stockpile by engaging in iteration after iteration of the design and test cycle, so too it renewed its own knowing ties with the nuclear explosives it designed. Just as weapon systems age and retire, so do weaponeers; and the laboratory relied on each iteration of the design and test cycle to provide younger members of the weapons community with an opportunity to forge new ties among themselves and with the devices under development. As long as the arms race demanded new weapons, the laboratory's weapons community was collectively able to maintain an active and knowing relationship with nuclear devices, and the average age of the stockpile remained a steady and healthy ten to twelve years of age.

This ongoing process of renewal was formally structured in cycles of activity. The fiscal year was the major cycle of renewing time at the laboratory, beginning on October 1^{st} and ending on September 30^{th}. Throughout the Cold War, individual nuclear tests –

that is, events that marked the peak in an iteration of the design-and-test cycle – were scheduled in tandem with the fiscal year, as the Department of Energy allocated monetary resources annually towards the testing program. All test events, whether Livermore, Los Alamos or military weapons effects tests, were grouped into "Operations" each fiscal year. The last full-scale nuclear test operation was Operation Julin, which began in October 1991 and ended in September of 1992.

Within the larger structures of a test operation, each test event – each iteration of the design and test cycle – had its own allocation of time. Knowledge production occurred cyclically, in iteration after iteration of the design and test cycle; but once the experimental planning had reached a point where a test could be formally scheduled, time *within* each experimental iteration became tightly linear, a track of finite duration, culminating in a test, whose execution required individual groups to work in tandem with others in the weapons community, if the test was to be completed "on time."

Within an iteration of an experimental cycle, time was tightly linear, acting as a track or a guiding structure for events and activities. The linearity of experimental time was most clearly displayed in the massive scheduling charts that coordinated project activities among many different groups of experts in the weapons community. In these charts – which were essentially enormous Gantt charts – time was represented a one-dimensional, one-way track that running like an arrow from the project's beginning to its end. Test planners placialized action-in-time (Casey 1996), drawing a "critical path" through their scheduling charts, linking individual "milestone" activities into a sequence of events that had to happen in tandem with each other if the test was to be executed by its scheduled date. In addition, time within an experimental cycle was conceived as a *resource*, a commodity to be spent, saved, wasted, budgeted, lost. The test coordinator and his assistants set the schedule, which told the many different subgroups in the community how to "spend" their time if the project was to be completed by its target date. In doing so, the test coordinators budgeted time/money, delineating points at which different tasks had to be completed if the project was to stay "on time and on budget." In many instances, they directly equated time with money, since delays in completing a project added to the total cost of the research.

The pace of the community's activity, or its tempo, played an important role in developing and maintaining a schedule, and in integrating the activities of different groups involved in a test. In developing a schedule, the test coordinator had to take tempo into account, since the completion of one group's contribution frequently depended on two or three other groups getting their parts done on time. For instance, the laboratory's engineering and technician teams could not finish putting the explosive package together if the other groups failed to deliver important pieces of equipment to the engineers and technicians "on time." The test coordinators not only had to understand points of articulation and interdependence among different groups; they also required a sense for the timing of activities in the event process, so that they could schedule realistic points for the integration of each sub-group's contribution into the larger experiment.

People executing a test shot frequently referred to "slips" in the schedule, another term that indicated the role of resource time in structuring and integrating the action of the community's many different constituencies. When an event "slipped," it usually meant that one team was having problems completing its part of the project. Sometimes, slips were locally confined, meaning that the delay only impacted one small group.

However, a serious slip in a key process, like getting the rack built, coping with cabling problems, or developing the diagnostic equipment, could have a ripple effect, delaying other parts of the test shot and perhaps even throwing the entire event off schedule. During the Cold War, serious scheduling slips would not infrequently push entire test events out of one fiscal year and into another.

Because it could easily take a decade of research to develop and certify a new system, the laboratory was constantly engaged in research activities related to stockpile renewal: diagnosing the performance of existing systems; developing and certifying parts to update the stockpile; creating and certifying new systems to replace aging ones. Every one of these goals required that the laboratory further its knowledge by engaging in the design and test cycle. In this sense, the design and test cycle not only provided *resource time* in the form of a schedule for executing a test, but was also analogous to Traweek's *replicable time*, since every iteration reinscribed and extended the base of experience that the laboratory's experts used to make judgments about the stockpile.

Throughout the Cold War, the temporal rhythms of the Cold War acted as a force for the renewal of both weapons and experts: the design and test cycle provided a framework for the ongoing integration of the weapons community's many ways of knowing, as it continuously renewed the knowing ties that bound the laboratory's experts to each other and to devices that they created.

Identity, Participation, and the Making of Artifacts

During the Cold War, reifications were critical to the production of weapons knowledge insofar as they provided points around which different parties could interpret information and negotiate meaning. Indeed, the entire design and test process can be thought of as an ongoing interplay between the creation of boundary objects and the negotiation of meaning around reifications of knowledge, as members of the weapons community engaged with each other in solving the problems particular to an experiment.

Ultimately, the activity that took place during individual iterations of the design and test cycle would culminate in a set of material artifacts: the experimental assembly that would be detonated at the Nevada Test Site. Every material object created in the course of the design and test cycle – the nuclear device, the test rack, the arming and firing system, the stemming plan – represented some specific area of the weapons community's expertise. Each part of the experiment was in a very real sense a reification, the material endpoint, of some specific "way of knowing" in the weapons community. Indeed, nuclear experiments and nuclear experts were mutually constituted, so that the creation of an integrated set of working artifacts, in the form of a nuclear experiment, was simultaneously the reproduction of an integrated community of practice. Bryan Pfaffenberger makes this point when he argues that the process of creating technological artifacts should be understood as inherently laden with meaning, so that we can appreciate "… the nonproductive roles of technical activities in the ongoing, pragmatic constitution of human polities and subjective selves" (1992: 501). In a very real sense, individuals in the weapons community identified themselves as experts in relation to a specific process and/or a set of material artifacts that existed as part of the larger cycle of designing and testing nuclear devices.

In the course of interviewing weapons experts, I realized that one way to discern how different experts understand themselves in relation to the design and test cycle is to ask them to describe failure scenarios. Initially, Gusterson's description of Livermore's testing process had led me to believe that the weapons community as a whole conceived of "failure" in terms of the experimental device itself: "Whereas many of us worry that a nuclear explosion will occur at some point in our lives, Livermore scientists worry that one won't" (1996: 160).

However, during my interviews, I realized that definitions of failure vary to a great extent, depending on one's location and responsibilities within the larger process of designing and testing a nuclear device. For instance, towards the beginning of my fieldwork, I interviewed a Los Alamos event engineer who had spent most of his career setting up tests at the Nevada Test Site. In the course of our conversation, I asked him to "…describe his worst nightmare – would it happen when a device doesn't go off?" To my great surprise, he replied thoughtfully,

> Whether or not the device performs really isn't my problem…. In my book, the worst thing that could happen ever would be dropping the rack and device down the test shaft. The guy that broke me in, he said, 'Do whatever you have to do to keep that from happening.' Because Livermore did that, and politically speaking, that was a bad thing for their laboratory.

During the Cold War, the NTS event engineers had a very specific area of responsibility: they were in charge of setting up the site for the experiment, which is essentially a civil engineering-type project. Insofar as event engineers did not design or build the device, the performance of the experiment did fall into the purview of their responsibility – unless the event engineer made the terrible and irrevocable mistake of dropping the experimental assembly down the test shaft. Similarly, I interviewed two containment geologists who explained that a serious venting accident, one that released significant quantities of toxic material into the atmosphere, would constitute a nightmare scenario for the containment scientists and engineers. One of these experts spent several hours showing me videotapes of historic venting accidents at NTS, explaining to me exactly how, why, and where each containment plan had failed.

Just as individual artifacts in the experimental rack embodied the expertise of individual groups, the test assembly *as a whole* existed as material evidence that the weapons community had successfully integrated its many ways of knowing. Sharon Traweek has described how the subatomic particle detectors that experimental physicists build at SLAC capture the signature research styles and practices of their creators:

> Detectors serve as a mnemonic device for thinking about various groups' models for scientific method: how to elicit traces from nature that are both significant and reproducible. Detectors… supply a system for classifying modes of discovery. Each is the material embodiment of a research group's version of how to produce and reproduce fine physics, how to gain a place for the group's work in the taxonomy of knowledge (Traweek 1988: 72).

Similarly, the artifacts produced by the LANL weapons community embodied the community's distinct research practices; and in doing so, acted as a "signature" of the Los Alamos weapons community. Often, when I asked my interviewees to describe how weapons science as practiced at Los Alamos was different than that practiced at Lawrence Livermore, they frequently discussed differences in artifacts and techniques. For instance, at the Nevada Test Site, Los Alamos and Lawrence Livermore each had distinct techniques for fielding a test. Simple differences in technique marked the two laboratories as distinct communities of practice: for instance, how its engineers measured diagnostic cabling, or whether they used metal ropes (Los Alamos) or pipes (Livermore) to lower a device into the ground.

Moreover, the identity of each community was embodied in the devices it created and put in the stockpile. Los Alamos designed ten of the fourteen nuclear devices in the current stockpile of weapons, a point of pride for most designers, who attribute Los Alamos' popularity with the military to its reputation for building reliable, solidly built nuclear devices. One Los Alamos designer characterized Livermore's designs as "Rube Goldberg" work, telling me that the military preferred Los Alamos' designs because "our weapons are simpler and more reliable." Another retired designer that I interviewed, however, was less impressed by Los Alamos' dominance in the stockpile. "Los Alamos was never as creative as Livermore," he told me, with a dismissive gesture. "We have a lot of designs in the stockpile because we always played the straight and narrow."

The Emplacement of Expertise: The Nevada Test Site

The Nevada Test Site played a particularly important role as the place where the laboratory's many ways of knowing came together in the form of a full-scale nuclear test. For most of the Cold War, the NTS was the laboratory's laboratory, the place where the weapons community validated its understandings about nuclear explosives and reinscribed its claims to expertise. As such, it played a key role in bringing the community together: it was the place where weapon designers and engineers assembled, buried, detonated, and diagnosed the performance of their experimental devices taking knowledge generated in each test event back to the laboratory to be incorporated in the community's larger stock of understandings.

I visited the test site four times in the course of my fieldwork; and to my great surprise, I found myself fascinated by the strange loveliness of its pockmarked landscape, by the arid geography of the lowland desert. I was captured by the fragility and heat of those spaces, the shimmering expanses of white-blue sky; the massive alluvial plains stretched taut between chains of desert mountains; the twisting forms of lonely Joshua trees that stand like contorted sentinels on a dry battlefield.

But even more striking, perhaps even contagious, were the intense cognitive and emotive connections that seemed to bind members of the weapons community to the test site. Each time I visited the test site, every time I asked senior weapons experts to recount their NTS experiences, I was reminded of Hugh Gusterson's description of weapons testing as a unifying ritual in which "a weapon is destroyed and a community reborn" (1996: 214). Nuclear tests not only demonstrated mastery over an arcane and deadly body of knowledge; they emerged as points of unification where members of the weapons community joined their many different ways of knowing. In a very real sense, I discovered, the laboratory's identity as a center for weapons expertise was embedded in every single one of the test holes, the craters that pockmark the arid plains of the Nevada desert.

The first time I visited the Nevada Test Site, I did so in the company of a retired experimental physicist, a stocky, grizzled diagnostician who had spent most of his career setting up nuclear tests in Nevada. We walked quietly along Frenchman Flats, a dry lakebed where the United States conducted its first series of atmospheric tests in 1952. It is empty now, save for odd remnants from early defense experiments that tested the effects of atomic blasts on different kinds of structures: collapsed concrete bunkers, broken bank vaults, pieces of railroad track. I looked at the ruins, then at the sky, the mountains, the flat hard clay of the dry lakebed below my feet. Finally, I broke my contemplative silence. "I like the desert," I said awkwardly to my tour guide. He looked over at me and I saw him smile under his beard. "Got to be a desert rat to like this," he said, gruffly. "Always loved this place myself. I had some of the best experiences of my life here." I nodded, and we continued walking along the ruined rebar and smashed

Figure 3-8. Aerial photo of subsidence craters at the Nevada Test Site. Photo courtesy Los Alamos National Laboratory.

concrete, pacing silently back towards our car parked at the sandy edge of the Flats.

Weapons experts almost always use tests to anchor narratives about their experience, to tell stories about their colleagues, to identify turning points in a particular experimental program (because linking tests together in a series is classified information, I use letters rather than specific test names): "X shot nearly killed his career, but Y was the follow-on test that proved to everyone that he had the right idea," a designer told me, in describing how a colleague had learned from a failed experiment. Similarly, a diagnostic physicist used tests to mark the history of a particular detector: "We initially fielded this diagnostic on X, but it wasn't until Y a few years later that it really worked, and after Z – well, it became standard for this kind of experiment."

Listening to weaponeers describe how particular tests furthered their individual careers and the collective expertise of the weapons community, I realized that every crater at the test site should be understood as a unique place, a product of the active engagement of experts with devices. The process of conducting a test transformed the undifferentiated sites in the Nevada desert into places, creating stable containers for the collective memory of the weapons community.

This transformation began a set of geographic coordinates in the desert, undifferentiated from the infinite points that surrounded it, until geologists and geological engineers began to characterize the site as a possible place for testing. They looked for surface features, faults, and subterranean rock formations that might contribute to a venting accident. If none were immediately apparent, the geological teams might send a drilling crew to bore a hole into the ground for a future test. Characterized by geologists and marked with a hole, the once-undifferentiated site now had the potential to be transformed into an experimental place. However, the weapons community did not use all its holes for tests. NTS personnel drilled holes long before any test was planned for them, so the DOE's testing program always had several extra characterized sites ready for an experiment. A hole might sit empty for several years, unnamed, save for a number that designated its location on the gridded plains of the test site.

An anonymous hole only acquired a working identity once the laboratory's geologists and test personnel matched it with a planned experiment. At this point, the hole became meaningful as a geographic point around which members of the weapons community organized action. The name given the experiment became associated with the number for the hole: "X test, to be conducted at Y hole." NTS field crews lowered cameras into the hole to characterize its layers and features, as geologists designed a unique stemming plan to contain the explosion.

For everyone involved in a test, "Insertion and Emplacement," day, or I&E, was one of the crux points in the experimental process, its significance second only to the actual test itself. On I&E day, the engineers inserted the nuclear device into the rack, lowered all the experimental artifacts into the ground, and stemmed the hole. Insertion signified that the device had been attached to the rack; but emplacement in particular marked unification, when the community had finally joined each individual element – device, rack, cabling, hole, stemming – into a single experiment. In the process of testing, the weapons community quite literally emplaced its expertise, burying it for demonstration beneath the surface of the earth.

Every experiment that made it to the Nevada Test Site was assigned a name: Lubbock, Victoria, Junction. Like many outsiders to the weapons community, when I first came to Los Alamos, I wondered about the process of naming a test. Were names chosen to connote some quality of the device being tested? I looked for hidden jokes: when a series of tests bore the name of Texas towns, perhaps this implied the symbolic destruction of New Mexico's overbearing neighbor. Gusterson writes that as Livermore named a series of tests after wines, Los Alamos named one after cheese, and the two laboratories joked about holding a wine and cheese party (1996: 138): could inter-laboratory rivalry be encoded in testing nomenclature?

However, the symbolism of test names is not so straightforward, as I learned when one of the laboratory's retired test directors explained the naming system to me: Every fiscal year, he said, the Atomic Energy Commission (AEC) chose two themes for test names and gave each of the design laboratories a list to choose from. Sometimes, the laboratory itself would suggest a class of words (birds, insects, cheeses) and offer it to the AEC as a source for test names. Weapons experts, I learned, are a pragmatic bunch, and preventing duplication was the overriding concern in naming tests: "We didn't want to use the same name twice. If there were two 'Shiloh' events, how could we tell the difference between the two?" he told me. Naming, he assured me, was generally a matter of unique identifiers, and generally there was no a priori connection between the nature of the test and its handle. "It would be too easy to reveal classified information about an experiment if we always linked the name to something we were trying out," he pointed out.

However, symbolism is not simply a matter of a priori connections. Meaning, after all, resides to a great extent in shared experience, and each iteration of the design-and-test cycle marked the re-engagement of the community's members with each other and with a particular set of artifacts. Test names, then, *acquired* meaning in the process of fielding an event. Arbitrary at first, each test name came to signify a particular experience in an individual's career, as well as a learning event in the long history of the weapons community.

One of the most interesting things about the testing program is the amount of memorabilia generated in the course of a test: stickers, t-shirts, hats and other souvenirs generated during the course of an event, much like t-shirts bought at a concert or a poster from a rodeo. The laboratory did not provide the memorabilia; rather, the people involved in each event used the test name as a springboard for developing an icon and a set of souvenirs for each test. Symbolically, individuals "collected" testing experience in the form of these souvenirs, and displayed them to their peers. Regardless of field – design, engineering, diagnostics – I could always tell when an office was occupied by an experienced member of the weapons community, since the door or walls would be decorated, sometimes covered, with stickers and/or small posters bearing the name of the event as well as a cartoon or a drawing related to the name. Similarly, at the test site, personnel often attach small pins and patches to their work clothes, or wear baseball hats sporting the name of a particular test. One of my tour guides at the test site explained the souvenirs by referring to the camaraderie that the intensity of a test engenders among test participants. "You become a team, like any other team," he told me. "These help you remember what it was like to be part of that particular team." For this physicist, the many t-shirts, hats, pins, stickers, certificates, and posters he had collected over years of involvement in the testing program served as markers of his identity as an active member of the weapons community.

Edward Casey has argued that place "is the condition for all existing things" (Casey 1996: 54). Weapons knowledge, like any other way of knowing, "sits in places" (Basso 1996), which are, moreover, a necessary for the existence of memory: without places, Casey says, human memory would be an undifferentiated sea of experience.

"Memory does not thrive on the indifferently dispersed; it thrives, rather, on the persistent particularities of what is properly *in place:* held fast there and made one's own" (Casey 1996: 187). The knowledge derived from testing resided in the collective memory of the weapons community, and individual tests serve to organize memory, binding knowing and experience into unique and memorable occasions. Conceptually and geographically, nuclear tests were place-containers that maintained and retained the community's memory and knowledge, rather than dividing or dispersing it (Casey 1996). Every crater at the test site marked the somewhere and the somewhen of a point in the community's knowledge history. In the landscape of the laboratory's past, every test – its name, its symbols, its crater, its data – should be seen as landmarks, the trail signs of a collective trajectory, the concrete precipitates of the community's ongoing efforts to build knowledge about nuclear weapons.

CONCLUSION: WEAPONS SCIENTISTS, NUCLEAR CONFIDENCE, AND MEANING

Culminating in a nuclear test, every iteration of the laboratory's design and test cycle produced a body of scientific and technical understandings that served as the underpinnings for America's nuclear deterrent. It is in this sense that we can think of nuclear weapons science as an enterprise laden with symbolism and meaning. Moreover, we should see nuclear weapons experts themselves as individuals whose work plays an important role in reproducing the context in which the devices themselves have such powerful currency as arbiters of conflict.

For the most part, social and psychological critiques of the nuclear weapons complex tend to engage in complex explorations of the subconscious of weapons scientists (Rosenthal 1990), and take nuclear weapons as direct technological metaphors for powerful and mysterious aspects of human existence: e.g., male sexual power (Caputi 1989, Caldicott 1984) or Christian visions of the apocalypse (Mojtabai 1986). But as colorful and scintillating as these critiques can be, I tend to agree with Hugh Gusterson's more pragmatic perspective, which is grounded in a close ethnographic reading of the Livermore weapons community. Nuclear weapons, and the people who create them, must be read and appreciated as part of the social and political context of twentieth century nuclear deterrence. If nuclear weapons are meaningful, he argues, it is because they exist as technological reifications of the moral, social and political principles embedded in deterrence theory. Nuclear weapons are symbolically powerful because they carry messages about the just use of threat to prevent conflict; about the power of technology to curb the inherent violence of human society; about the Machiavellian rightness of means that effect a particular end. In other words, by insisting on reading nuclear weapons as sexually and religiously charged metaphors for life and death, we may be inadvertently drowning out the voices of weapons experts themselves, who tend to tell a far different story about their work and its value.

It is critical to understand that nuclear weapons science is not just about the physics, radiochemistry and engineering of nuclear weapons; and as such, it cannot be fully understood as a meaningful enterprise outside the larger social and political context in which it is embedded. The weapons experts that I interviewed at Los Alamos expressed pride in their efforts to design and certify reliable nuclear devices in support of the nation's nuclear deterrent. As one retired weapon physicist told me, emphatically, "Los Alamos has maintained a culture of quality. There is no industry in the United

States that could afford the kind of quality, the guarantee that we give our weapons. We've always offered the damndest guarantee of weapon reliability."

Although I was initially horrified when I heard statements like these from my interviewees, I quickly learned not to read them as evidence that the laboratory's weapons designers and engineers look forward to the day when their claims about the reliability of their devices will be vindicated in a nuclear war. On the contrary, the weapons community makes these claims loudly and clearly in the firm belief that nuclear confidence offers the best possible means of *preventing* conflict. This belief is so very strong that most of the scientists and engineers that I interviewed during my fieldwork had a difficult time envisioning a scenario in which the United States would deliberately use nuclear weapons. As Randy, an engineer visiting Los Alamos from DOE headquarters in Washington, told me, "Our country knows so much about nuclear weapons that it's almost impossible to imagine using one. Because we know exactly what these weapons can do, because we know how terrible they are, we understand the responsibility."

The precision of the laboratory's statements, the scientific accuracy with which they were made, was a critical component of the nuclear deterrent. In the bizarre and arcane world of nuclear deterrence, expertise is as much the coin of the realm as are the nuclear weapons themselves. The weapons community's understandings; its close and continuing relationship with nuclear devices; the knowledge embodied and practiced in the living, subjective selves of its experts – these provided the underpinnings for a kind of confidence that, from the perspective of the laboratory at least, transformed military conflict into a non-option for directly resolving political and economic disagreements between East and West.

The mutual constitution of nuclear weapons, nuclear confidence and the subjective selves of weapons experts was dramatically illustrated for me during one of my visits to the Nevada Test Site in the spring of 1999, when I attended a tour that the laboratory had organized as part of a larger training exercise. One of our guides was an electrical engineer who had designed arming and firing systems for Los Alamos' nuclear tests. The tour took us into the middle of the NTS, to the site of an experiment that had been abruptly abandoned when the laboratory's testing funds were cut unexpectedly in 1992. In the wake of the moratorium, the laboratory and DOE officials at the test site decided to leave the equipment standing and use the site as an educational stop for NTS tour guides, who in turn use the site to illustrate the process of fielding a nuclear test. As our group stood around the abandoned equipment, our tour guide explained where and how the electrical equipment was mounted onto the rack, how the firing signal was sent and received, the swift process of detonating a nuclear device. Listening to him describe his work and field technical questions with fluency and expertise, I realized that he had probably worked on dozens of nuclear experiments. As his audience dispersed to explore the rusting equipment scattered around the event site, I stayed back and asked him if he missed working on nuclear tests.

"Testing?" he barked. "Of course I miss testing." He looked at me impatiently, a little sharply, as you might eye a child who has interrupted an important conversation, and gestured to the rest of the tour group. "Over here, folks, I'll show you the trailers where the arming and firing systems went." He started to walk away but I swung into step with him, my steel-toed safety boots pushing hard against the sand as I matched his long stride. "Why?" I asked. Without pausing, he turned around to look at me, took a few half-steps backward. "Why? Because it's a powerful thing, seeing a crater collapse into the ground." He turned forward again and kept walking, firmly, towards the trailers on the other side of the crane. "Well," I thought. "I've been dismissed." But just as I was about to fall back with the rest of the group, he continued talking more loudly, not looking at me but looking ahead, towards the horizon, as though he were talking to someone else. "I know what these things can do," he said, sounding almost frustrated. "I've seen them send a ripple a hundred feet high across the desert. Goddammit, I'd bring every world leader here if I could, I'd blow one up and make them watch that ripple. Just to show them. So they don't ever, ever forget what they're dealing with." And he marched ahead of me, alone, shaking his head, the wind lifting thin gray strands of hair off his forehead and pushing his worn nylon jacket tightly across his barreled chest.

"The process of acquiring insight
produces awareness of one's own way of life
as something worthwhile,
and the mastery of skills builds self esteem."
- Henning Siverts, quoted in Antweiler 1998: 469

CHAPTER FOUR: ACTIVITY AND EXPERTISE IN WEAPONS ASSEMBLY ENGINEERING

The Los Alamos National Laboratory's (LANL) weapons programs have traditionally drawn upon the expertise of engineers trained in civil, mechanical, chemical, and electrical engineering. Engineers had various areas of responsibility in relation to the Cold War design and test cycle: for instance, civil engineers supervised construction operations in Nevada, while electrical engineers created firing sets to arm and detonate the experimental devices. As important as all these engineering functions were, weapons assembly engineers played a particularly important role in the design and test cycle because they specialized in transforming the designers' concepts into real-world, working nuclear explosive systems.

Until the Cold War ended, weapons assembly engineers supervised groups of assembly technicians in the hands-on process of putting together experimental "packages:" the high-explosive experiments for the local shots, as well as the full-scale nuclear explosive devices for tests at the Nevada Test Site (NTS). Weapons assembly is a highly specialized form of mechanical engineering, and since it is practiced only in a few places – namely Los Alamos and Lawrence Livermore – hands-on training in the context of the weapons programs was (and remains) critical to the perpetuation of the craft.

In this chapter, I focus on the role of engagement and activity in shaping the knowing sensibilities, understandings and intuitions of neophyte weaponeers. In doing so, I rely on ethnographic data that I collected in the spring and summer of 1999, while doing participant observation among weapons assembly engineers in the Weapons Engineering group of the laboratory's Engineering Sciences and Applications Division (ESA-WE), and their counterpart assembly technicians in ESA-WMM, Weapons Materials and Manufacturing (see organizational chart). In the following pages, I discuss the present-day role of assembly engineers vis-à-vis the larger weapons community, describe the assembly process, and explore how neophyte members of ESA-WE become established assembly engineers. In doing so, I emphasize the importance of knowing-in-context, insofar as specific problems offer points of focus around which individuals can engage productively with each other, and with specific aspects of the material world. In addition, I generally use the feminine pronoun to refer to the assembly engineers I am describing. Although most assembly engineers are men, the assembly team that I observed most closely was comprised of two women. Moreover, women have a relatively strong managerial presence in ESA. The ESA-WE assembly engineering team is directed by a woman engineer, and ESA Division itself is currently the only major research organization in the Los Alamos weapons programs to have a woman as a division leader.

WEAPONS ASSEMBLY ENGINEERING AT LOS ALAMOS

Assembly engineers themselves describe their location in the laboratory's experimental environment as a significant point of articulation in the process of translating an idea for an experiment into a functioning experimental device. A recent ESA Division's strategic planning statement describes the mission of the assembly engineering group: "ESA-WE is responsible for converting theoretical designs into safe, usable systems that can be fabricated, tested, produced, and maintained at reasonable cost and for design and production of test systems for local and Nevada tests" (Ortiz et al 1994). Or, as one engineer explained to me, "We're a liaison between [the weapon designers] and what actually comes out of the shops and, finally, the assembly bay. We're the conduit of information to make sure that the designer's intent actually gets incorporated into what comes out of our assembly bay as a final product."

Until 1993, the laboratory's assembly engineering functions were located in one of LANL's most powerful and established divisions: WX, Weapons Engineering, which has existed at the laboratory under various names since 1948. Engineers and technicians in WX played a key role in the weapon design process: the engineers advised primary and secondary designers about materials, shapes, and machining and assembly processes, while the technician teams performed the hands-on, precision work of device assembly at Los Alamos. WX engineers also supervised the assembly of the nuclear device at the test site, as well as its insertion into the test rack.

Several of my interviewees both inside and outside of ESA told me that, historically, weapons engineers at Los Alamos were nearly as powerful as the weapons designers themselves, because the engineers translated the designer's concept into a material, working device. As one of the laboratory's technical reports explained,

> The theoretical designer has only approximated the actual device…. Computer designs take no account of manufacturing processes and the variations they introduce into a product… Weapon engineers must deal with the differences between "real" and "computer" worlds and provide a product that comes as close as possible to the theoretical design (Los Alamos National Laboratory 1976: 7).

Frequently, "dealing with differences" meant that weapon designers would have to re-think aspects of their paper design if the WX staff advised that a part or a joint would be too difficult to machine. At the same time, WX engineers could introduce designers to possibilities they had perhaps not imagined:

> Designers aren't the only ones who come up with breakthrough ideas. An engineer might come to you and say, "By the way, I can make this metal light as air," and suddenly you can do something you never thought of before (X Division designer Jas Mercer-Smith, quoted in Bailey 1995: 81).

Moreover, in making refinements to weapon designs under development, WX engineers looked beyond their own assembly operations to the DOE's production facilities, taking into account the fact that the laboratory's designs – which could be quite esoteric – would at some point require mass production in facilities outside Los Alamos (Los Alamos National Laboratory 1976: 7).[11]

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[11] Late in my fieldwork, I interviewed Harold Agnew, who joined the Los Alamos during the Manhattan Project and eventually became laboratory director in 1970. He was one of several retired interviewees who asserted that engineers at Los Alamos had a much greater say in the design process than engineers at Livermore. "Our engineers were constantly thinking of the production end of things," he said. "They made sure the production facilities could handle the designs we gave them." Stylistically, Livermore's weapon

The more I learned about the weapons programs, the more I had the sense that WX engineers were less helpmates than equals of the designers, a position that gave them considerable political power in the weapon design process and in the larger realm of the laboratory. Not all designers would agree with this, however: upon hearing that I was observing assembly engineers and technicians putting together high-explosive experiments, one of my designer acquaintances in X Division said, "Well, you have to realize that they've got the easy part, that they just put things together. We have to think of the designs, we have to know so much more than they do."

My friend's characterization, I think, reflects a commonly-held perception among scientists and philosophers of science that, as a discipline, engineering derives most of its understandings from science, so that the relationship between science/theory and engineering/technology is seen as one in which the knowledge belonging to the latter is completely subsumed by the former (Laudan 1984, Vicenti 1990, Bucciarelli 1994). As engineer Walter Vicenti writes, "Modern engineers are seen as taking over their knowledge from scientists and, by some occasionally dramatic but probably intellectually uninteresting process, using this knowledge to fashion material artifacts" (1990: 3).

However, as Rachel Laudan points out (1984: 83), technological development, while based on scientific research, represents a "special case of problem solving," whose puzzles are quite different from those that drive advances in scientific research. For one thing, science seeks knowledge for the sake of knowledge, while engineering is a discipline that seeks knowledge as a means to a utilitarian end – the creation of material artifacts. Technological advances might entail the application of scientific knowledge, but

designs were far more creative than LANL's, he said, "but ours were a lot easier to mass produce because our engineers had so much say in the process."

the process of engineering working devices and mechanical systems requires artful, creative thinking to bridge the gap between scientific research and technological product. "The creative, constructive knowledge of the engineer is the knowledge needed to implement that art" (Vicenti 1990: 4).

The engineers I interviewed described themselves in much the same way that Vicenti and Laudan describe their profession: creative, practical, down-to-earth. To the extent that ESA's engineers and technicians build devices on behalf of X Division, most of my ESA-WE interviewees agreed that they worked "for" X Division and described weapon designers as their primary "customers." In doing so, however, they stressed that engineers master a different kind of knowledge than designers – not a less sophisticated one. ESA engineers tend to dismiss any negative characterizations of their work as evidence that outsiders (like my X Division acquaintance) lack familiarity with the hidden complexities of weapon engineering. Several of my interviewees pointed out that designers never actually have to build their own devices. And I will never forget the time I asked an ESA engineer to characterize the difference between weapons design and weapons engineering, and he rolled his eyes, saying, "Oh, designers, they're pie-in-the-sky, they think stuff up and in their models it all works *so* beautifully." Then he laughed and said, "I hope this doesn't offend you, but it's like they have wet dreams," implying, of course, that engineers work in the real world – and thus have "real sex."

Assembly Engineering Today

These days, many of the engineers who worked in WX are located in ESA division, which itself is the product of a major post-Cold War reorganization that LANL managers undertook between 1992 and 1993. This reorganization brought together several different, formerly separate engineering groups – including WX – under a new administrative umbrella, creating ESA as a division with a mosaic of functions that included weapons design and assembly engineers, mechanical and electrical engineers, as well as two groups of personnel involved in tritium research, including production alternatives (Ortiz et al. 1994: 2). ESA-WE and ESA-WMM are two of eight specialized engineering groups in ESA division.

These days, because the nuclear testing program is largely defunct, neither the ESA-WE assembly engineers nor the ESA-WMM technicians work as routinely with full-scale nuclear explosives as they once did. Nevertheless, assembly engineering remains a critical component of the laboratory's current research program, because Los Alamos continues to perform high explosive shots: i.e., in order to validate their models, designers still rely on data from non-nuclear hydrodynamic experiments – analogous to the local shots of the Cold War – that are conducted at firing sites in Los Alamos. In addition, since 1995, the DOE has been pursuing a new experimental program at the Nevada Test Site, one that allows researchers to gather physics-type data without full-scale nuclear tests. This "subcritical" program – so called because the experiments use nuclear material, but do not form a self-sustaining or critical chain reaction – provides the laboratory with data about the aging processes that take place in plutonium. In the context of the subcritical and hydrodynamic programs, ESA-WE continues to recruit and train neophyte engineers in the art and science of assembling high explosive devices.

Organizationally speaking, ESA-WE's has grouped its assembly engineers into a single team, the Above Ground Experiments (AGEX) team, which includes roughly six to eight engineers as well as several support staff who perform office functions for the team. In the post-Cold War era, AGEX is an appropriate designation for the assembly engineering team because most of the experiments the laboratory currently conducts are above ground tests, as opposed to the elaborate underground nuclear tests of the Cold War.

The AGEX team's offices are located with the rest of ESA-WE's offices, in a rather remote location, TA-16, about three miles southwest of the town of Los Alamos (see map). Many people refer to this area by its wartime name, S-Site, perhaps because its function has not changed since the 1940s, when it was established as a high-explosives research, development, and fabrication area. Today, S-Site is a large, forested area surrounded by chain link fencing and barbed wire, sparsely and sporadically populated with office buildings, warehouses, machine shops, sheds, and high-explosive bunkers. Not surprisingly, this is a limited access area requiring a Q-clearance for entry, and because it is closed to public access, S-Site has become a haven for large herds of deer and elk that wander down from the nearby Jemez mountains to graze in its meadows.

Assembly engineers work in two areas: their offices, which are located in a rather nondescript building near the front entrance to S-Site, and the high-explosive assembly bay, Building 410 (referred to as "410"), a cavernous, windowless warehouse located roughly a mile west of ESA-WE's main offices. Although assembly engineers do a great deal of work in 410, the assembly bay actually belongs to ESA-WMM, the group that employs the assembly technicians. Technicians, or "tecs" as they are called at Los Alamos, are the blue collar, skilled craft workers of the laboratory – machinists, metalworkers, laboratory assistants – who do hands-on scientific and engineering labor:

in chemistry or biology labs and in machine shops (Meyer 1985: 12-14). ESA-WE's engineers supervise the assembly work that the technicians perform in 410, but they do so as professional guests under the auspices of ESA-WMM.

Because so many groups at S-Site work with high explosives, a good part of the area is fenced off as an "exclusion area" requiring an escort for visitors. Building 410 is located deep within this exclusion area, a long way off from the more populated office buildings and cafeteria. Pedestrians and private vehicles are not allowed in the exclusion area, so AGEX team members drive government vehicles back and forth between their offices and the assembly bay, which is about a mile's drive. Doing my observation meant that I had to arrange appointments with the assembly engineers, who graciously picked me up near the security gate and drove me to 410 with them.

The Assembly Process

In the context of the hydrodynamic and subcritical programs, AGEX assembly engineers are responsible for putting together experimental explosive packages for tests at Los Alamos (hydro shots) and at the Nevada Test Site (subcritical experiments). More specifically, ESA's assembly engineers coordinate the manufacture of individual components, ordering and receiving special tools for performing an assembly, inspecting

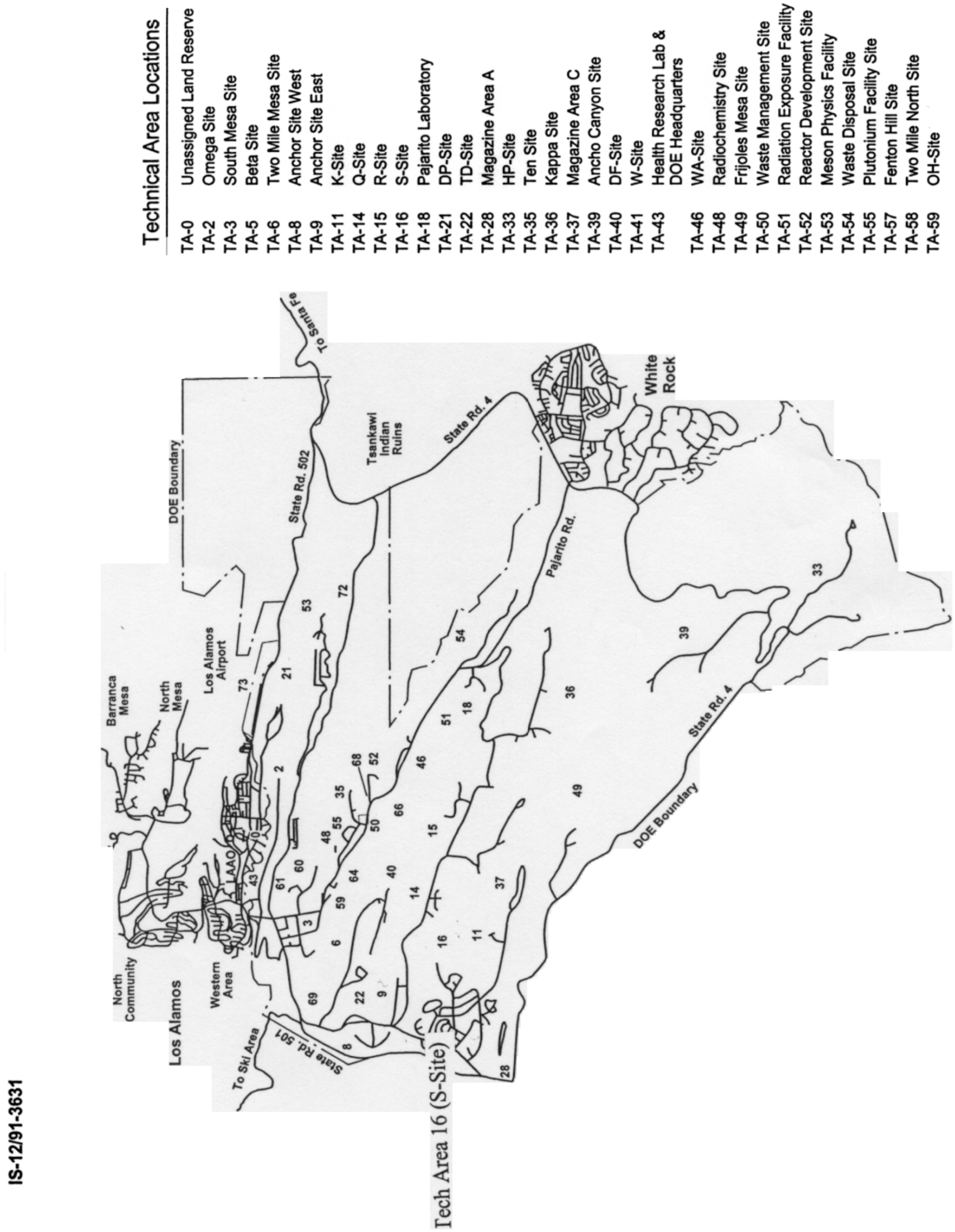

Figure 4-2. Map Showing Location of TA-16.

parts and tools for defects, developing a set of instructions for the assembly process, (including checkpoints for measurements to ensure that the device is being put together properly), and overseeing technicians as they do the precision work of assembly.

The assembly engineer's highest priority throughout the assembly process is to ensure that the artifact she builds is a precise material representation of experimental intent. Every assembly engineer whom I interviewed emphasized the importance of understanding at an intuitive level what the designer is trying to accomplish with a particular device, and being able to translate that intention into the finished assembly. Throughout the assembly process, the engineer maintains close contact with the experiment's planners – primarily X Division's designers, and the diagnostic experimenters who design the data collection equipment – to ensure that the device she is building meets proper specifications.

AGEX engineers enter the experimental cycle when X Division issues a design release for a hydrodynamic or a subcritical test. The design release provides ESA staff with information about the experiment, the components and contours of the high-explosive device, along with the diagnostics being fielded on the experiment. If the experiment involves an entirely new kind of device, something that the engineers have not previously built, the design release can be quite extensive. On the other hand, the laboratory often detonates several of the same type of device, perhaps slightly modified, in a series of experiments. In this case, the design release may only consist of few pages of drawings and notations indicating any modifications to the design and describing the diagnostic layout. Historically, the design release has also provided assembly engineers with a deadline in the form of a firing date for the test.

Turning a design release into a working experimental device can involve a great deal of discussion among all parties involved in the experiment. The process begins when the engineer(s) meet with the designer (or designers) from X Division, and with diagnosticians who design data collection equipment for the experiment. Each group comes to ESA with its own set of ideas and priorities for the shot. As one AGEX engineer explained to me,

> The designer might tell us, “Put all these diagnostics in.” But we’ll go back and tell them, “From our perspective, we can’t get this component,” or “It will take a long time to get this in, so I can’t turn this assembly around as fast as you need it.” Or let’s say X Division tells us that they want to see a particular type of information. The diagnostics people say, “Okay, I can give you this kind of diagnostics.” But then the assembly engineer will say, “I can’t do it that way. So how else can we do it?” So there is a lot of negotiation, particularly up front, to set on the final design. (Laughs) There is never, ever a time when you get a design release and all the things that are requested in there end up on the assembly. It never happens that way.

In addition to designers and diagnosticians, the assembly engineers also act as liaisons with the technicians in ESA and in other divisions. Technicians and machinists are the people who fabricate the individual components for a device, and as such, they play a significant role in determining whether or not a particular design is feasible: as one engineer told me in conversation, “If the machine shops can’t make something, it doesn’t get made.” This should not be taken to imply recalcitrance on the part of the laboratory’s machinists; rather, it highlights the fact that designers and engineers are not always aware of the limitations of certain materials or fabrication processes until the machinists and technicians point them out.

During the Cold War, most of the inert and high-explosive components were fabricated at Los Alamos proper, while the fissile parts – e.g., plutonium pits for nuclear primaries – were fabricated at the DOE's production facilities, including Rocky Flats in Colorado and Y-12 in Tennessee. This meant that assembly engineers frequently traveled off-site to consult with nuclear engineers, machinists and technicians in other facilities. These days, since ESA is primarily focused on high-explosive experiments, assembly engineers negotiate the manufacture of most experimental components within Los Alamos proper.

AGEX assembly engineers also work with other ESA engineers to develop drawings and procedures for putting the device together. The design release provides ESA with a set of numbers that serve as a mathematical description of the device and its contours. However, engineers work with drawings, not numbers; and before any components can be fabricated or assembled, design engineers in ESA must transform the numbers in the design release into a set of engineering drawings. These assembly drawings provide information to the people who will work in the "hands-on" development of the device: e.g., the machinists who fabricate the components, as well as the engineers and technicians who need a visual map for putting the package together.

As the device plans begin to emerge from the ongoing discussions among engineers, designers, machinists, and diagnosticians, the assembly engineer makes preparations for the actual assembly. She writes a set of densely detailed instructions that will provide her team with step-by-step guidelines for putting the package together. The assembly instructions are far more than a laundry list of tasks describing which part should be attached where. They "call out" special tools and materials for the assembly and set out points at which the technicians should take measurements to ensure that the assembly is going together properly. Writing these instructions requires that the assembly engineer consider the question of experimental intent in each and every step of the assembly, so that she can guarantee the finished package is a faithful material translation of the designers' and diagnosticians' concepts.

In addition to writing the assembly procedures, the engineer has to consider what tools (or "tooling") the process calls for. Assembly work is highly specialized, requiring delicate measuring instruments, stands, epoxies, hoists and other equipment. Because ESA-WE has done so many high explosive and nuclear experiments, the engineer can draw much of her tooling from the hundreds of pieces that ESA already owns. However, because every experiment is slightly different from the others, she may have to order a new set of tools for a project, perhaps from an industrial supplier or another DOE laboratory. If the tool is unavailable, the engineer can also consult with one of the laboratory's machinists to have a piece custom-made for a project. Unless temporarily loaned from another organization, all tools are stored in 410 after the project is completed, perhaps to be used for a future project.

Every assembly is assigned a four digit number (e.g., an upcoming hydro might be referred to as "2006") and as tooling and parts arrive at ESA, they are grouped by number on large, flat, wheeled pallets in the assembly area. The assembly engineer (and sometimes the technicians) inspect parts and tools as they arrive to ensure that they meet the design specifications. This might include load-testing stands and hoists, or other load-bearing pieces of equipment, to ensure that they will not break during the assembly. Not infrequently, assembly operations are stalled by delays in the supply chain: tooling that does not arrive on time, or parts that arrive promptly but are later discovered to be defective.

Every high explosive project that comes through ESA is assigned an assembly team composed of a lead engineer, a lead assembly technician, and several supporting technicians, including one who specializes in materials preparation and another who focuses on quality assurance. The materials technician supports the assembly team in the preparation and precision application of adhesive materials to the device. The quality assurance (QA) technician checks every step of the process during the assembly to make sure that the team does not deviate from the assembly plans, or that deviations are noted and explained clearly.

Most of the ESA-WMM assembly technicians that I encountered were Hispanic or Native American men; and although none of them held college degrees, a few had taken college courses, and one held an associate degree. All had received their core training on the job and were highly skilled in working with high explosive materials. Several of the tecs I talked to had worked in other areas of the laboratory before coming to ESA and had extensive – and useful – experience as metalworkers or machinists.

The teams I observed also included an assistant engineer. Assistant AGEX engineers are usually novices with a master's degree in mechanical engineering who are learning the craft of device assembly by watching and helping the experienced engineer and technicians. Although it is not unusual for an assembly to be assigned two engineers, this situation most typically occurs when a novice engineer is being trained to do assembly work. The AGEX engineers that I met, both novices and established assembly engineers, were both Anglo and Hispanic, both men and women, varying in age from their mid-twenties to their late forties.

As the assembly date draws near, the engineer calls a design review meeting with the members of her assembly team. She opens the meeting by describing the experiment; then she and the technicians go through the entire set of assembly instructions, visualizing the process as they discuss particular steps. It is not unusual for a senior ESA technician with Cold War experience to have participated in dozens of assemblies, a base of experience upon which the technician draws to critique the assembly steps: The instructions on page eight are not clear. Why has the engineer called for this tool, and not another? This glue usually sets for ten hours, not the eight specified in the instructions. Are you sure you want us to take a measurement at this point and not earlier? "The technicians are really important," one engineer told me. "They're the ones that help you gather what you need in order to do the assembly." The design review can be a very valuable source of information for the assembly engineer: even if she has good reasons for laying out the instructions as she has, the technicians' comments can help her double check her reasoning.

After all the planning is completed, the design review complete, and the parts and tooling have arrived, the assembly team can begin putting the device together, a process which takes roughly three weeks to complete. At this point, the engineers and technicians will work almost exclusively in ESA-WMM's assembly bay.

Just as security is a key component of laboratory culture, physical safety is highly valued in ESA's organizational culture. This is particularly true in high-explosives work. Before I could even drive through the high explosive exclusion area with the engineers, I had to read a safety brochure and sign a release saying that I understood and would follow all required safety precautions. Anyone entering or exiting the high explosive exclusion area on TA-16 must swipe their badge through in a badge reader. This is a security practice that prevents unauthorized persons from entering the area, while serving the additional safety purpose of tracking who might be stuck in the area in the event of an emergency. In addition, all the assembly engineers I worked with carried special pagers for evacuation notification.

Site-specific regulations govern behavior within the assembly bay itself. High explosive work is safest in a cool environment, so 410 is always chilly. Even in the summer, I was glad for long pants and a sweater, particularly since I spent most of my time sitting still, watching and writing. In addition, there is a strict dress code for working in the high explosives area: long pants, steel-toed boots with non-sparking soles, and eye protection, usually thick Plexiglas chemistry-type goggles that people take from a small cabinet on the wall near the front door. I discovered too that clothing marks roles: most assembly technicians working on the assembly floor wear dark or light blue one-piece, heavy cotton working suits, while the quality assurance technician and the engineers usually wear street clothes – khakis or jeans, long sleeved shirts, maybe a sweater or a sweatshirt. A few of the senior technicians, such as the team leader who oversaw all operations at 410, also wore street clothes, but I never saw him doing hands-on assembly work.

ESA may employ a relatively high number of women engineers, but the assembly bay is still a masculine world. All the technicians that I observed doing assemblies were men, although I did meet a few women technicians permanently stationed at 410. The staff coffee room off the main assembly bay struck me as particularly masculine. No food or drink is allowed on the assembly floor, so the assembly teams take breaks whenever their work allows them to do so. I spent several coffee-and-donut breaks quietly listening to the technicians and engineers chatting about guns, hunting, fishing, motorcycles, and their families. A dozen or so hunting and fishing magazines sat on the table, and the bulletin boards on the walls were decorated with a menagerie of pictures: magazine pictures of elk; photos of wives, daughters and girlfriends; and the occasional poster of a missile, a bomber, or a mushroom cloud from the days of above-ground atomic testing.

Assembly operations take place on high, black, fixed tables with thick supports and a broad, dense, non-sparking surface to prevent unintentional ignition of the high explosive – e,g., from a tool accidentally hitting the table and sparking. The floor beneath is also a non-sparking surface and feels slightly springy under one's feet. At any one time, there might be several assembly operations taking place in 410, with two or three teams quietly clustered around different assembly tables in the bay. Each team keeps its tools and parts at hand, storing them on large pallets labeled with the shot number, or on "cadaver tables" – wheeled surgical tables whose surfaces have been covered in dense black rubber. These tables usually have wide racks attached above them, with clips spaced evenly along the top rung. This is where the assembly teams display the poster-sized engineering drawings that guide them in the assembly process.

Within the assembly bay there is a strict division of labor: 410 is technician territory, and although the assembly engineers supervise the process to be sure that the finished product meets the designers' specifications, the technicians do nearly all of the hands-on work. As I watched the teams putting devices together, I realized that I was observing the workings of a highly refined knowledge system, in which engineers and technicians each bring a specific set of understandings, responsibilities, and skills to the assembly table. Assembly engineers, for instance, are responsible for the overall experiment: for the contents of the device, for the materials and tools that are used to attach the device components, and for ensuring the proper alignment of all parts in the system. One of my assembly engineer interviewees told me, "Because I know all of the dimensions, I know how things are supposed to go together. I am responsible for the overall quality of the experiment."

However, successfully putting together an assembly requires joining a series of parts with nearly faultless precision, and this precision work is the purview of the technicians. Despite her supervisory role in the assembly process, the assembly engineer leaves the hands-on work to ESA-WMM's technicians, who are specially trained in high explosives safety:

> The technicians are materials experts. They do a course in handling high explosives. Engineers, we're not trained in handling HE. It drives the technicians crazy when an engineer just reaches in and touches something. Sometimes I have to touch a piece to get a feel for it, but I always ask permission before I pick it up.

Similarly, although the engineer's instructions might call out certain tools and fixtures for an assembly, the technicians are the ones who are trained in using the special tooling, adhesives, and fixtures. Hence, technicians perform nearly all of the hands-on work: picking up parts, moving them, gluing them together, pressurizing joints, all under the watchful eye of the assembly engineer, who observes the process but rarely handles any of the pieces: “That way, if something goes wrong,” one engineer told me, dryly, “It’s not my fault because I wasn’t touching it.”

The question of tolerances and measurements provides a good example of knowledge domains in assembly engineering. Design engineers in ESA work with AGEX assembly engineers to estimate a range of measurements for every part of a system, within which the system can reasonably be expected to perform as designed. As such, these ranges – which are more commonly known as tolerances – play an important role in ensuring that the finished product will function as specified in the design release. These ranges are astoundingly small, usually a few “mils,” or thousandths of an inch. It is the assembly engineer’s responsibility to ensure that all parts and sections of her assembly, and ultimately the entire system, fall within acceptable tolerances.

Since the engineer does not put the device together, she must rely on the technician’s skills to ensure that the system is properly assembled. However, along the way, the engineer checks the technician’s work by measuring the parts as they are put together. Indeed, nearly every step of an assembly operation ends with measurements to verify that parts fall within the specified tolerances. The assembly engineer tells the technicians what curve, joint, or surface she wants to measure, and they choose the appropriate instrument for doing so.

> The technicians know the little intricacies of the whole thing. I can go in and say, “I want to take this measurement, an equator to a pole,” but I don’t know what kind of tools the technicians are going to use, what’s the best method to take the measurement, that particular technique. I don’t specify exactly how they’re going to do it. They tell me how they’re going to do it, so that they can give me the most reliable information.

While watching assembly operations, I was always fascinated by the tools that the technicians used for taking measurements. For instance, when the steps called for precisely measuring spaces between parts, one of the technicians would pull out a velvet-lined wooden case. Similar to a case in which one might store sterling silver, it was filled with small porcelain measuring blocks in different widths, from about two inches to fractions of an inch. The surfaces of the blocks were so smoothly machined that they sealed on contact to ensure precise measurement. When measuring a gap between two surfaces, the technician would insert blocks into the space, starting with a large one against the far surface and adding progressively smaller blocks until he had combined them into a perfect fit. Then he would read the measurement notations on the surfaces of the blocks, adding them together to get a value for the space.

Figure 4-3: ESA-WMM quality assurance technician, left, and assembly technician checking wiring on assembly. Photo courtesy Los Alamos National Laboratory.

The technicians will often take three separate measurements of the same part to make sure that the values they provide the engineer are accurate. As the they do so, the QA technician checks the technicians' measurement and compares it to the tolerances specified by the assembly engineer. If the QA technician approves of the measurement, and determines that it falls within acceptable ranges, the assembly engineer records the measurement in the assembly instructions and both the QA technician and the assembly engineer initial the notations as being correct. Because everyone wants a successful experiment, the team is very careful about documenting every measurement, every step of the assembly procedure, including the minutest deviations from the assembly instructions.

ESA-WMM's technicians know how to take the measurements accurately, and the QA technician helps ensure that the measurements are accurate and fall within tolerances; but the AGEX assembly engineer is responsible for knowing why the measurements are important, and must interpret them to be sure that the system she is building is on track. Experiments can fail for a number of reasons: a basic design flaw on the part of X Division, a problem with the diagnostic equipment – or a problem with the device engineering. AGEX assembly engineers learn early on in their careers that even microscopic faults in an assembly can ruin an experiment. As one engineer told me while I was watching the technicians measure a part, "One tiny fault, just the size of a human hair, that you can barely see, can cause a perturbation that will blow your entire assembly to hell in a split second." The assembly engineer is constantly aware that if the experiment does not perform to expectations, she may be asked to review exactly what went into the device and how it was assembled, to demonstrate that her engineering understandings, decisions, and practices created a device that accurately captured what the designers requested.

Frequently, the lead AGEX assembly engineer is called upon to judge the potential effects of a particular feature on the performance of the entire system. Although unexpected, problematic features can arise in the course of an assembly: an air bubble in the glue, a minutely bowed part, a tiny fault in a joint, a measurement that falls just slightly out of tolerance.

Early one morning, as we were driving to the assembly bay, the lead engineer told me that the team had spent hours the previous day trying to fit and glue together two small parts of a larger assembly. As we walked into the bay, one of the technicians called the lead engineer over to look at the parts, which had been drying overnight. Apparently, the technician had noticed tiny air bubbles in the glue joining the two pieces. This caused a fuss, and pretty soon, the entire assembly team was scrutinizing the bubbles. For the next half hour or so, several engineers and technicians were engaged in a debate over the pieces: should they start over with the gluing operation? It had take nearly a day to complete. Was the problem more basic, like a piece that had not been machined properly? Could they afford to ignore the bubbles and move onto the next step?

As they debated, an engineer and a technician from another assembly team came over to examine the pieces and review the steps the assembly team had taken the day before. Suddenly, the whole table broke into laughter. Apparently the lead engineer had tried to coax the consulting engineer into formally approving the parts by initialing his approval on the assembly steps. "No way, no way!" he said, backing off. "This one's all yours." As he and the technician left, he joked that the technician had approved the part by carving his initials into it. Later, I asked the assembly engineer about the exchange. "Well, it's not like we're building model airplanes," she replied, and explained that technicians are never responsible for making these kinds of judgment calls, despite the fact that many of them are highly experienced in the assembly process. Neither could she ask another engineer to sign off on a problematic part. "If anything goes wrong, if there's an engineering problem, well, that's my fault," she finished.

Ultimately, of course, the engineer cannot know if she has built the device correctly until it is tested. However, several weeks can pass between the time the assembly engineers and technicians finish the device and the actual test. This is because other teams in the weapons community – primarily the diagnosticians, as well the team that fires the device – must finish setting up the experiment, and can only do so once they have the device in hand. Occasionally, the design might call for a piece of diagnostic equipment to be built into the device. In this case, the assembly engineer receives the finished diagnostic piece from the diagnostician and includes it in the assembly, sometimes under the observation of the diagnostician. However, there are always additional diagnostic parts to be added to the device, and after the assembly team has completed its work, the assembly engineer passes the device onto the diagnosticians, who work with the firing site personnel to finish setting up and executing the experiment.

The firing day can be quite nerve-wracking for the assembly engineer, who will not know if the device works until it is actually detonated. Although engineers frequently attend their tests, several of the AGEX engineers that I interviewed told me they avoid going to the firing site when one of their hydrodynamic devices is being tested, either because attending the test is too stressful, or because they are afraid they might jinx the results. One of my interviewees had a bad surprise when her first test failed: "Basically your response when they say, 'Ready, set, fire!' and nothing happens, you're like, 'Okay, what did I do? What did I bend, crimp or break that I don't know about?'" The experimenters later determined that problems with the firing site area, and not the device itself, had caused the failure. "But we were up until one o'clock in the morning trying to fix it. It took them three days to get it fired. I vowed after that that I wouldn't ever go back. So I've only been to the firing site once."

The firing site is not the only point at which the engineer's work can be called into question. When I asked my assembly engineer interviewees what kind of feedback they typically received from designers, they usually told me that "no news is good news," meaning that silence from X Division or the diagnosticians indicates general satisfaction with the device. Occasionally, however, the results of the experiment may differ significantly, even inexplicably, from the expectations of the designer or the diagnosticians:

> The most satisfying thing from an engineer's perspective: you send something off, it blows up, and it doesn't come back. You're finished. But if the designer calls you back with lots of questions, you know there are significant questions that have come out of the data. You can tell how an experiment went if you get a phone call a couple of weeks afterwards.

This situation can result in an extensive, and potentially very stressful, review of the experiment, including the assembly process. It is at this point that the QA technician becomes important: if something goes wrong with the experiment, and the engineering process is called into question, the engineers can use the QA's notations to demonstrate that the assembly process went "by the book." Nevertheless, if a design review calls an engineer's abilities into question, it can have a serious impact on his or her career.

The actual test, then, does far more than provide feedback data to the designers. As Trevor Pinch has insightfully observed, any situation that tests a technology also tests the people who created it: in other words, we should understand any technological artifact as a material representation, a reification, of active human knowing (Pinch 1993: 26-28). If an assembly engineer becomes nervous before a hydro shot, it is because the test will demonstrate her skill in interpreting the designer's experimental concept, and in directing her team in the material realization of that interpretation. From her perspective, the experimental artifact she has built is a material reification of her own skills and understandings. In a very real sense, the test demonstrates the engineer's proficiency in device engineering: not just to her fellow engineers, but to the entire group of people who have contributed to the experiment, and by extension to the weapons community at large.

BECOMING AN ASSEMBLY ENGINEER

Assembly engineering – like any other field of weapons work – is a highly specialized discipline. If they are to fulfill their responsibilities to their peers, AGEX assembly engineers must master a set of locally significant skills and sensibilities in order to contribute effectively to the experimental process. The assembly engineer bears a particular cognitive burden: she must not only grasp the purpose of an experiment, but must be able to translate her understanding into a working material artifact. In doing so, she must recognize her role as an interface and develop the ability to communicate understandings among several different groups of people at the laboratory: designers, diagnostic experimenters, machinists, design engineers, technicians.

Arguably, any engineer working in a multidisciplinary and/or experimental setting would face similar cognitive challenges. However, experimental weapons engineering as practiced at Los Alamos is a unique discipline, and not simply because Los Alamos is one of world's few nuclear weapons research facilities. Rather, in fifty-five years of working with nuclear weapons, the laboratory has developed its own particular research culture, so that assembly engineering as practiced at Los Alamos is different from assembly engineering as practiced at other laboratories: e.g., Livermore or Sandia National Laboratory. As strange as this may seem to outsiders, weapons research is not standardized across the nation's weapons facilities laboratories. Each of the DOE's laboratories has its own local research practices: hierarchical structures that establish roles and responsibilities among experts, types of tools that are used in one laboratory but not another, even particular styles of designing a nuclear weapon that stamp a device as "uniquely Los Alamos" or "uniquely Livermore." In regards to assembly engineering, then, becoming an expert requires that novices master the particulars of mechanical weapons assembly engineering as practiced in the context of Los Alamos.

In the following pages, I briefly describe the developmental cycle of the assembly engineering community, exploring the activities through which novices are gradually drawn into the core activities of device. In doing so, I discuss the importance of apprenticeship relationships in the weapons community as a whole, and briefly compare the developmental cycle of assembly engineers to the developmental cycle of weapon designers. Assembly engineers engage in several stages of apprenticeship before they are allowed to supervise their own assembly; and during this time, their seniors evaluate their practices and behaviors to determine if they will be granted full status in the assembly community. I describe some of the practices and understandings that novices must acquire if they are to participate fluently in the local assembly community and, by extension, with the weapons community at large. Lastly, I spend some time reflecting on the nature of learning and expertise. I argue that learning is best understood as a process of identity development and social reproduction; while expertise is emergent, displayed in the ongoing engagement between knowing selves and artifacts in the experimental world of nuclear weapons research.

Formal Education and Entrance into the Assembly World

Assembly engineers are drawn to high explosive work for different reasons, but nearly all of them describe a strong desire to work with their hands. Maria, for instance, had spent the early years of her career performing numerical analysis for different engineering projects. She moved to the assembly team because she wanted to "really put something together":

> I'd spent several years modeling things on the computer, but then I thought, "Let's see how it actually goes together." [You can] put nominally modeled components together in a simulation, but the models are different than the real thing. [Simulations] are going to be different than if you put together components as they actually have been fabricated…. I felt a need to understand the assembly process, to understand was actually required to put a system together. So this has been a good thing for me.

Then, of course, there is the excitement of working with high explosives. As Elizabeth, a senior engineer, told me,

> I wanted a job where I could do the hands on engineering. With a Masters degree, a lot of times you're not given a hands-on job. [My interviewer] told me I could blow up things. And my Masters' degree is in damage mechanics... [laughs] I guess my eyes lit up when he said "blow up things."

Several of the novice assembly engineers that I met had come to the laboratory as recent graduates with a master's degree in mechanical engineering. Most were natives or long-time residents of New Mexico, who had either gone to school in state and then moved to work at Los Alamos, or who had attended school out-of-state but later returned home. ESA-WE recruits many of its engineers from New Mexico schools, like the University of New Mexico (UNM) in Albuquerque or the New Mexico Institute of Mining and Technology (NMT) in Socorro. In addition, two of the engineers that I interviewed described extensive professional experience in disciplines and institutions with ties to the Los Alamos assembly engineering community. One engineer had worked extensively with the Department of Defense before coming to the laboratory, while another had transferred to ESA-WE from a design engineering group in ESA. Prior to her arrival at Los Alamos, she had worked in nuclear engineering at another Department of Energy weapons facility.

Coming to Los Alamos with a degree in mechanical engineering, many novices have some experience working with high explosives; and if they have transferred to the LANL assembly team from another DOE laboratory, or even from within Los Alamos, they might even have some experience working with nuclear materials. However, while educational attainment and professional experience provide novices with a basic vocabulary of engineering practices, all neophyte assembly engineers face the same challenge: to extend their formal training, and any related experience, into the highly specialized, ever-shifting realm of experimental weapon engineering as practiced at Los Alamos.

The Stages of Apprenticeship

The trajectory of formal membership in the assembly engineering community begins when newcomers are hired into ESA-WE, an event that grants them tentative status as potential members of the AGEX team. Being hired into the group provides an opportunity for learning what it means to be an assembly engineer at Los Alamos, as senior members of the team invite newcomers to engage, in limited fashion, with the practices of assembly engineering; and by extension, with the larger goals of the weapons community.

Outsiders to the weapons community are often surprised to learn that, at Los Alamos, apprenticeship relationships have traditionally played a significant role in helping newcomers get a feel for the highly specialized processes, methods, and understandings that the Los Alamos weapons community relies on to do its work. The whole concept of "apprenticeship" evokes images of feudal craft-guilds and small-scale operations of production, and therefore seems particularly medieval when described in the context of nuclear weapons design and development.

However, apprenticeship training is extremely important at Los Alamos: the highly secretive nature of weapons work means that it is not taught in any college or university in the United States. Even if there were such a program, novices would have to do considerable training at the laboratory regardless, since Los Alamos has its own particular ways of building explosive devices. Neophyte weapon designers, for instance, can easily spend the first ten years of their careers under the tutelage of an experienced designer:

> In [X Division's] apprenticeship-journeyman-master system, the hierarchy is based on years of successful testing. After eight years, I'd call myself a senior journeyman, or maybe a junior master (Jas Mercer-Smith, quoted in Bailey 1995: 80-81).

In comparison to other groups in the weapons community, designers have traditionally had a particularly long learning curve before graduating to the status of master. For one thing, Cold War designers were the integrators of knowledge from many areas of the weapons community, and therefore they had the widest range of responsibility in relation to a nuclear test. In addition, the process of designing and fielding a nuclear experiment could take three to five years. Novices in X Division opened their careers by assisting senior designers on tests already in progress, but would not be considered fully-fledged designers until they had fielded their own shot. Moreover, during the Cold War, designers were expected to field at least two successful shots before being granted master status. Not surprisingly, it could take several years for neophytes to accumulate the experience necessary to be considered fully-fledged members of the X Division design community.

Similarly, during the Cold War, assembly engineers were expected to work under the tutelage of an experienced engineer on several assemblies – both local shots and full-scale nuclear devices – before assuming responsibility for their own projects. However, my interviewees indicated that, historically, the learning curve in ESA-WE has been considerably shorter than X Division's: for one thing, ESA-WE engineers, while heavily involved in the design and engineering of nuclear weapon systems, do not have the broad scope of developmental responsibility that designers have, so their task field is more circumscribed. Secondly, designers are expected to follow a project from cradle to grave, so to speak, while assembly engineers come into a project at a specific phase and are released to work on other projects after the device assembly is completed. In other words, assembly engineers are able to accumulate the experience required to work independently on projects more quickly than can the designers, because their period of involvement and scope of responsibility in the design and test cycle were far more limited.

Despite the fact that the scale of work these days is much more limited than it was during the days of nuclear testing, apprenticeship in ESA-WE has not changed much. Novices are still expected to participate in several iterations of the local shot cycle before they are considered skilled enough to supervise their own test. Learning stages are fairly formalized in ESA-WE, lasting through three to five iterations of the assembly process, with the novice being granted increasingly higher levels of responsibility until she has demonstrated that she is adequately proficient in assembly engineering.

Almost immediately after she joins the group, the novice engineer enters into a period of formal apprenticeship with an experienced member of the AGEX team. In doing so, she assumes the mantle of "assembly engineer," but is not yet expected to assume full responsibility for the experiment. At this point, the novice's role is entirely that of the assistant. This betwixt-and-between status is important for learning, because it allows her to try out assembly engineering, to practice her new role in low-risk settings, under the supervision of a more experienced engineer. Throughout this first stage of apprenticeship, neophytes might observe and participate peripherally in several iterations of the assembly process. During this "lay of the land" stage, their mentors encourage them to ask questions and to assume responsibility for small, relatively low-risk tasks, such as arranging meetings, ordering parts and tooling, and recording measurements in the assembly bay.

If her mentor judges that she has made sufficient progress by the end of her first assembly, the two switch roles, so that the newcomer acts as the lead engineer, while the established engineer acts as her assistant. Although relatively sheltered, this arrangement exposes the newcomer to a greater level of risk, while allowing her to engage more thoroughly with different aspects of the assembly process. She begins to establish deeper working relationships with groups outside ESA: diagnostic physicists, designers, firing site personnel, other engineers.

Eventually, after participating in several iterations of the experimental process under the supervision of her mentor, the neophyte will be released from the mentoring relationship and will take the lead position in an assembly project. At this point, the novice is considered a young assembly engineer, and is sufficiently engaged in the process to carry out her own assembly from start to finish. In doing so, she can still rely on the advice of her mentor and of other assembly engineers on the AGEX team, but is considered an independent actor prepared to take full responsibility for the quality of the experiment. As such, she has reached a sufficient level of engagement with the assembly community to be considered a peer: her fellow engineers may begin calling her for advice, or a designer might request that she be assigned to an upcoming experiment. After completing several of her own assemblies, she will probably be asked to mentor a newcomer.

Apprenticeship relationships are critical in training novices, structuring their work activities so they can develop a sense for working with groups outside ESA-WE, as well as with other engineers and assembly technicians in their own group. Because the weapons community has its own ways of doing things, the knowledge acquisition curve can be quite steep during the early stages of learning:

> When I first come in, the first job they gave me was to follow around an experienced assembly engineer, to learn

> what I guess you'd call the tricks of the trade. To become an assembly engineer. I followed him around on his NTS test and his local experiments. And the whole thing was mystifying, the whole process.

The first learning experiences are cognitively complex because learning is occurring on so many levels. In addition to stretching their engineering skills to new areas of practice, novices must figure out what they do not know, and establish connections with resources that will help them fill in those areas of ignorance:

> I'd go to the meetings and jot down all my questions, then I'd go back to my team leader and I'd say, okay, I have questions. And he'd patiently explain it all to me. It was mostly terms, weapon related terms. An assembly, as a whole, is a fairly complicated system, and getting familiar with all the intricacies of that takes a little time.

Many of the engineers indicated that before they could do extensive hands-on work with an assembly, they had to locate their role in relation to other figures involved in the experiment: diagnosticians, designers, technicians. As one retired engineer told me, "Theoretical weapons design is a whole other region, and a good rapport with the designers is absolutely essential to make a test device or a weapon system." Another mid-career engineer told me,

> I didn't know exactly what happened. I came in and I knew the engineering role was to put things together, but I didn't understand the interfaces between X Division, [the groups at NTS], the different entities in weapons, or even how the lab was set up….The way the groups fit together was pretty nebulous to me. It took a long time to understand the group interactions.

Even experienced Los Alamos engineers can have trouble when they first encounter the local practices of the assembly team. One of my interviewees had worked for several years in another engineering group at Los Alamos before transferring to the AGEX team, yet still had problems understanding the language of assembly work:

> I spent a lot of time when I first joined WX, attending meetings and just trying to soak in information. It was a whole new language, even though I was dealing with the same general subject in Los Alamos that I had encountered in other places. I mean, the specifics were different, and so the language was different, so I spent a lot of time trying to learn this new language.

Understanding the language and practices of assembly engineering; defining one's role and responsibilities in the context of the experimental process; developing smooth working relationships with other experts in the weapons community: all of these are necessary if the novice is to act fluently in the experimental environment of the laboratory, and can only be learned as she participates in the assembly process.

It is important to recognize that the apprenticeship period is a two-way street. Just as it enables novices to learn what it means to be an assembly engineer, so does it enable seniors in the assembly engineering community to observe and test their juniors. In addition to developing the skills, sensibilities, and relationships involved in the assembly process, novices are also expected to display the appropriate characteristics, behaviors and attitudes that make a good engineer. Safe handling of high explosives, patience, and meticulous attention to detail are critical in assembly engineering, which is a deliberate process that involves putting together both explosive and inert components into a precisely delineated experimental configuration.

Despite the initial attraction of hands-on work with high explosives, not everyone is suited to the slow, meticulous process of putting together explosive devices. One senior engineer emphasized the importance of patience:

> You have to have the right personality to be an assembly engineer. (Pauses.) You have to be able to watch the grass grow. If you can't watch the grass grow, you can't be an assembly engineer. Assembly engineering has been [compared to] brain surgery... things don't move very fast. A lot of people don't deal very well with that –if they can't have something that's quickly turned around, then they're not happy. Assembly engineers have to learn to watch [parts] dry, or to take the same measurement fifteen times until they're happy. It's a really precise process, and a lot of people – well, I've had several engineers in the team that I've been on, that found out it wasn't their thing. You need patience, you need endurance.

In addition to exhibiting qualities like patience and attention to detail, newcomers must also prove that they can work effectively and productively with ESA-WMM technicians. Although assembly engineers deal with representatives from many groups in the weapons community, they work in very close physical and cognitive proximity with the technicians. Several engineers emphasized the importance of showing respect for the technicians, despite the fact that they do not share the same degree of educational attainment that the engineers do. "They'll test you," one engineer said.

> The technicians initially – they watch you and they're standoffish and they don't say a lot. They work with you to help you with whatever you need to get done. But every once in a while… they'll ask you for a left handed screwdriver or a metric crescent wrench to see if you're on the ball.

Moreover, newcomers who "cop an attitude" with the technicians may find it very difficult to work productively. As one engineer put it, "I think that if a new engineer was really trying to throw his weight around, the technicians could make him pay, and they'd do it in very subtle ways." It is fair to say that a new engineer who fails to build a productive relationship with ESA's technicians will not have a long career in the assembly bay.

It is also critically important that neophyte engineers display an appropriate level of respect for working with dangerous materials. Unlike their counterparts in the design groups, or in the diagnostic groups, assembly engineers and technicians interact routinely with potentially deadly explosives. A powerful folklore of risk and responsibility surrounds high explosives work; and although the laboratory has had few serious high-explosive accidents, newcomers learn about the importance of safety through hearing about the few accidents which have occurred, the most serious of which are etched deeply into the collective memory of the engineering community. Both engineers and technicians will quickly reprimand and even dismiss newcomers who do not work safely. The ESA-WE group leader, who is a senior assembly engineer, emphasized this point:

> Within two years of joining this group they didn't hesitate to let me work on my own assembly. I was only 25 but I've always been really careful. That's a big thing here, being careful. You have to show good judgment, that you respect that you're working with really dangerous substances, stuff that can kill you... Members of your team watch how you work. Has he had a lot of accidents? Is he careless? The more safety problems you're associated with, the less likely you'll ever be allowed to do this work on your own. Eventually really careless people are pulled out of the team because they don't contribute to a culture of safety.

This means respecting the division of labor in the assembly bay, a rule that can be difficult for those neophyte engineers who are drawn to mechanical engineering in general because they enjoy the hands-on work of putting systems together. However, they must learn not to touch any of the high explosive materials during the assembly process. During one of my periods of participant observation at the assembly building, I witnessed a novice engineer reprimanded by the lead assembly engineer and the technicians after she picked up a small piece of high explosive material to visually inspect it. The two technicians stared at her as she picked up the piece, and within a second she realized her mistake and carefully replaced it on the assembly table. As soon as the piece was resting safely on the table, the lead engineer playfully but meaningfully slapped her wrist. “Don’t touch ANYTHING,” she chided her protégé, who immediately apologized to the technicians.

Learning to become an assembly engineer entails far more than merely acquiring and mastering a discrete set of skills. Rather, as one engineer told me, “It’s not like they offer a course in nuclear weapons in graduate school. When you come here, you’ve pretty much got to start learning an entirely new ball game.” As the “ball game” metaphor implies, learning implies activity on several levels: one must grasp technical understandings, such as why a particular tolerance is critical, for example, or when and where to measure a device under assembly. In addition, newcomers to the assembly world must understand where they fit into the larger experimental process, so that they can fulfill their relationships and responsibilities to other weapons experts. They must learn to work creatively and productively with other engineers and technicians; with weapon designers, diagnostic physicists, and field test personnel. When working with their counterpart engineers and technicians, novices are expected to grasp and enact locally significant values, such as attention to detail and a cautious respect for high explosive work.

Learning and Becoming in the Assembly Bay

Any model of learning which focuses solely on skill acquisition misses the important point that learning does not happen in the tidily bounded spaces of individual minds, but is rather a social process through which learners begin to lay claim to a particular mode of engaging with the world. As Lave and Wenger have written,

> The individual learner is not gaining a discrete body of abstract knowledge which (s)he will then transport and reapply in later contexts. Instead, (s)he acquires the skill to perform by actually engaging in the process, under the attenuated conditions of legitimate peripheral participation. This central concept denotes the particular mode of engagement of a learner who participates in the actual practice of the expert, but only to a limited degree and with limited responsibility for the ultimate product as a whole (1991: 14).

In other words, learning implies a trajectory of participation, the movement from the periphery to the core of a community of practice, as novices engage with the processes, practices, goals and activities that mark the community's local ways of knowing.

Secondly, participation is not just one particular mode among many others for acquiring knowledge. Rather, participation is a necessary condition for learning, because

> [l]earning is a matter of engagement; it depends on opportunities to contribute actively to the practices of communities that we value and that value us, to integrate their enterprises into our understanding of the world, and to make creative use for their respective repertoires (Wegner 1998: 227).

Novice engineers learn what it means to be an assembly engineer by engaging with increasing fullness in the activities of a particular community of experts, be it assembly engineers, weapon designers, or technicians. Indeed, absent the opportunity to engage in the practices of assembly engineering, there is no way that novice members of ESA-WE can build the relationships, skills or understandings necessary to be an assembly engineer. This is because the process of assembling an experimental device constitutes an *activity system* that integrates social, material, and individual components, creating a context in which human agents engage in action (Keller and Keller 1996: 126). Participation in the assembly process is a requirement for learning how to act competently in this system, with the level of responsibility, risk, and exposure increasing in proportion to the learner's level of engagement with the process. This definition of learning-as-participation holds as true for a classroom, incidentally, as it does for the assembly bay: students are learners because they are engaged with, and defined in relation to, a socially sanctioned process for acquiring knowledge. This engagement also extends to teachers, other students, counselors, coaches, administrative staff, et cetera, all in the context of an institutional community organized around the furtherance of education (Lave and Wenger 1991: 40).

Understanding the assembly process as an activity system implies several things about learning and knowing within that system. First of all, each element of the assembly process exists meaningfully in relation to the system as a whole. The device, the tools, the assembly bay, the AGEX team, the ESA-WMM technicians, novice assembly engineers, senior assembly engineers are *mutually constituted* elements of the same entity: each is implicated in the existence of the other, insofar as no single one of these elements exists meaningfully outside the context of practice that is device assembly. Indeed, the social category "assembly engineer" only makes sense when understood in the context of the Los Alamos weapons community, the assembly bay, to a specific way of knowing nuclear devices, to a particular area of practice at Los Alamos. Similarly, the information, skills, sensibilities and understandings that define the knowledge domain of the assembly engineer do not exist meaningfully outside the context of the assembly process: rather, they are situated in the nexus of relationships that link each element of the activity system together, and emerge as assembly engineers act in concert with different elements of that system.

Also embedded in this view of learning as participation in an activity system is a process of identity formation. At Los Alamos, claiming expertise in any area of weaponeering is a matter of coming to understand oneself as knowledgeable in relation to a specific aspect of the design-and-test cycle. As novices move through stages of apprenticeship, and become increasingly familiar with an area of practice, they are constructing their identities as a particular kind of expert – as a designer, as a diagnostic physicist, as an NTS test coordinator. In ESA-WE, novice engineers gradually build a nexus of relationships in which they understand themselves as "assembly engineers" vis-à-vis the immediate elements involved in the assembly process: the design release, the assembly steps, the technicians, the assembly bay. Similarly, a technician is never just a technician – he is a particular kind of technician, an ESA-WMM technician who works in 410, who engages with a specific set of artifacts during a particular stage of the design-and-test cycle. Learning implies a trajectory of membership; it is a matter of becoming a particular *kind* of person, of re-inscribing in one's subjective experience a certain, locally meaningful way of knowing, as newcomers become members of a community that lays claim to a particular mode of engaging with the world-at-large (see especially Wenger

1998). Practice and identity are two sides of the same coin, insofar as who we are is quite literally what we do.

Learning is also a matter of social reproduction. Learning that takes place within one local area of expertise – like device engineering – opens a channel for individuals to engage with the weapons community as a whole. It is the process through which the laboratory's culture of weapons work is reproduced and extended: as novice engineers participate with increasing fullness in the activities of device assembly, they begin to contribute to the shared goals of the weapons community, since furthering the practices of device engineering contributes to the ongoing development of knowledge about nuclear weapons. As they become increasingly familiar with their local roles, then, novices are actively contributing to the wider reproduction of the Los Alamos weapons community as a unique community of practice. Moreover, as they are drawn more deeply into the activities of the weapons community, they can begin to play with established ways of doing things, to engage more creatively with the problems of weaponeering, extending and transforming these practices through time and space.

CONCLUDING DISCUSSION: THE NATURE OF EXPERTISE

Becoming a member of the weapons community, moving toward full participation in its goals and mission, involves gradually being able to lay claim to a particular area of expertise, some kind of locally valuable knowledge or skill. However, most people in the weapons community will tell you that "expertise" never implies full mastery of a specific knowledge area. To my surprise, even people with years of experience in their particular field of weapons work are reluctant to call themselves experts. One gentleman who had worked in weapons testing since the 1950s bristled when, in requesting an interview, I mentioned that his colleagues considered him an expert. “Young lady,” he chided me, “you've got to realize that there are no experts in this field. No one will ever know everything there is to know about this job.” Similarly, Elizabeth, an assembly engineer with ten years of experience at the Laboratory, had a difficult time articulating her own level of expertise.

> I don't like the term expert, and I don't like master, either, but I am not a journeyman. I am at that (emphasis verbal) level where I've built enough assemblies to where I know how they go together, I know the operations, and I feel that if they gave me just about anything I could cope, I guess you'd put me in that category. But I don't like those words....I always have this vision of experts as... (she pauses for a long while). I don't know... Highly experienced would be a good adjective for me... I think of an expert as - somebody who... actually, I don't really consider anybody an expert in this thing.

I heard statements like this all the time from people throughout the weapons community, on many occasions from individuals with two, three, sometimes even four decades' worth of experience at Los Alamos. At first I attributed them to professional modesty. Gradually, however, I realized something significant: these claims to limited knowledge always extended beyond the self in question, to indicate communal limitations, to describe anyone and everyone's ability to KNOW about weapons work. Modesty suddenly seemed an inadequate explanation. But what these statements meant, collectively, remained puzzling for a long time. “After all,” I kept thinking, “If people at Los Alamos aren't weapons experts, then who would be?”

The longer I spent watching people at work, the more I realized that reluctance to claim expertise is significant not because it indicates the knowledge (or lack thereof) of a particular person or a even group of people. Rather, it speaks volumes about the nature of experimental weapons work itself, an enterprise that is located at the fuzzy fringes of what is concretely understood about weapon behavior. This kind of work is very different from that performed in other areas of the DOE weapons complex. Take, for instance, the Pantex plant in Amarillo, Texas: there, engineers define the parameters for assembling warheads, and technicians repeatedly perform the same precise set of steps in putting together one system after another. In contrast, the Los Alamos weapons community developed every device as a unique experiment designed to answer specific research questions. Hence, no two iterations of the design-and-test cycle were ever exactly the same.

The assembly technicians I observed were particularly fond of comparing themselves to their counterparts at Pantex. One afternoon, I was watching an assembly engineer and a group of technicians working on a small assembly for a local high explosive experiment. The assembly under construction was tricky, and none of the tools at hand seemed appropriate for attaching two particular sections to each other. One of the technicians pulled out some paper and started doodling, telling the assembly engineer that he had an idea for a tool that would facilitate the entire process. The assembly engineer seemed pleased with his idea. Later, she told me that this assembly was one of a pair, that the team would be doing a second package similar to the first, and that the technician's tooling would come in very handy. I asked where he'd get the tool made at such short notice. “Oh,” she told me, “they've got a machine shop, and if they need a tool, they just make it. They're pretty talented.”

Later that week, during a different stage in the assembly, the engineer and one of the technicians realized that they would need to modify another small tool to make it fit a part. Everyone stepped back from the assembly to take an afternoon coffee break while the technician and the tool disappeared towards the back of the high explosive bay. While he was gone, another of the technicians walked over to the high metal stool where I was sitting with my notebook. "See that room back there? We've got some machining equipment back there. We can make our own tools," he told me, proudly. "We're not like Pantex technicians." When I asked him what he meant in comparing himself to "Pantex technicians," he explained that the nature of work at a research facility like Los Alamos is very different from the production-line work at Pantex, where people see the same configuration repeatedly. At Los Alamos, he explained, every assembly is a bit different, so the work is constantly changing. Engineers and technicians here have to be problem solvers; to do the job well, he told me, "you have to think on the fly."

In talking to technicians about their work, in watching them put different experimental assemblies together, I was frequently told that individualism and creativity are not valued in operations that depend on the repeated execution of precise steps, such as production operations in the Pantex factory floor. This is not to say that Pantex assembly operations rely entirely on explicit knowledge. Putting together a system as complex as a nuclear warhead necessarily relies on a great deal of tacit knowledge, intuitive understandings, and embodied skill acquired through years of hands-on experience in an assembly environment.

However, operations at Pantex and at Los Alamos are qualitatively different. Sociologists of science might explain this by pointing out a simple fact about mass-produced technologies: by the time that the weapons get to Pantex, they have become what Latour and Woolgar (1987) would call “black boxes,” reifications of design principles that are so accepted they can be put into mass production. But if “black boxes” represent technological reifications of verified theory, then LANL’s devices are reifications of questions, of uncertainty, of curiosity. In an experimental setting like this one, creativity, intuition, quick thinking, and problem-solving skills are valued both at the Laboratory and at the Nevada Test Site, where technicians, machinists, physicists, crane operators, engineers, geologists, and other experts are constantly reconfiguring their skills and understandings to meet the unique requirements of every experiment.

This, in turn, gave me a significant insight into the nature of knowledge and expertise at the Laboratory: in the creative setting of weapon research, highly individualistic projects continually place new demands on the skill and creativity of different groups of experts. Knowing in this setting implies much more than mastery of a specific body of information or a particular set of skills. Rather, knowledge exists simultaneously with human activity; it is emergent, “constantly coming to terms with actions and products that go beyond the already known” (Keller and Keller 1996: 127). In his ethnography of design engineers, Larry Bucciarelli emphasizes the importance of ambiguity and uncertainty in “allowing design participants the freedom to maneuver independently within their object worlds, save for ties to others to ensure some degree of consensus” (1988: 120). In the Cold War weapons community, every iteration of the design-and test cycle, every single test event, was an opportunity for participants to respond creatively and energetically to ambiguity. Uncertainty not only gave “life to a project" (ibid.), it gave life to the community, allowing participants to creatively reapply their skills to new problems and – occasionally – to display their genius in doing so.

In this sense, it is critical to recognize that the movement from being a novice to an expert in the weapons community is not about mastering information, one's length of tenure, or possessing a core set of skills that can be re-applied in every setting. Rather, the design and development of nuclear devices, knowing how these devices work, is an experimental process, one that by definition deals constantly in the realm of the not-quite-known. As such, expertise in this realm involves requires the ability to draw creatively from one's base experience to participate more fully in debates and discussions about areas of knowledge whose contours are perhaps sketched rather than perfectly mapped.

"I do not believe we can maintain a technology base or the necessary cadre of first-rate scientists and engineers… for more than a few years – if testing ceases."
- Former Laboratory Director Harold M. Agnew to Congressman Jack F. Kemp, April 19, 1977 (in Los Alamos Science, Winter-Spring 1983: 70)

CHAPTER FIVE: WEAPONEERING UNDER THE NEW PARADIGM

The Hatfield-Exon-Mitchell Amendment that Congress passed in the summer of 1992 was intended to slow the Cold War arms race by limiting the development and testing of nuclear explosives in preparation for a permanent test ban. The amendment's authors did not expect that enactment of their legislation, in and of itself, would mark the end of the United States' nuclear testing program. However, this is precisely what happened, as the moratorium was first enacted in 1992, extended in 1993, then re-extended indefinitely when the President signed the Comprehensive Test Ban Treaty (CTBT) in 1996. Despite the fact that the Senate failed to ratify the CTBT in October of 1999, international treaty protocols require that the United States observe the signed CTBT while the Senate attempts to ratify it. Currently, there is no indication that the United States will return to testing in the near future, and for all intents and purposes, the testing program is defunct.

However, in 1995 the Department of Energy proposed Stockpile Stewardship and Management (SSMP) as an alternative method for validating the safety, security and reliability of the nation's nuclear weapons. Under SSMP, weapons experts at Los Alamos will keep their expert judgment honed through Science Based Stockpile Stewardship (SBSS), a multidisciplinary approach that relies on experiments, archived data and computer simulations conduced under the Accelerated Strategic Computing Initiative (ASCI) to produce knowledge about nuclear weapons.

In providing the weapons programs with a new, challenging research focus, the adoption of SBSS arguably saved the laboratory from closure, a future that seemed all-too-possible as the Cold War ended. However, the transition from testing to SBSS has not been entirely unproblematic for the weapons community. In particular, the test moratorium has raised significant questions about the nature of weapons-related expertise in the post-Cold War era. Members of the weapons community are quick to point out that testing and expertise together provided an epistemological foundation for confidence in the stockpile. Now that the testing program is over, scientists and engineers at Los Alamos, Livermore and Sandia have become increasingly concerned about the specter of knowledge loss in their weapons programs. As the Cold War workforce ages, they say, retirees will carry critical skills and understandings out of the laboratory. Without the testing program to replenish these skills in newcomers, a great deal of the knowledge base could disappear as early as 2005, when the last of the Cold War era weaponeers reach retirement age.

Weapons experts at Los Alamos are not the only ones voicing these concerns. In 1997, Congress established the "Commission on Maintaining United States Nuclear Weapons Expertise," better known as the Chiles Commission for its senior member, retired Admiral Henry G. Chiles. After twelve months of investigation, the Commission found "disturbing matters" that "make it difficult to conclude that the Department will succeed in maintaining future nuclear weapons expertise in the complex" (1999: 7), and offered a series of recommendations to the DOE for preserving and replenishing its base of expertise. By 1999, Los Alamos was actively working to implement some of the Chiles Commission's recommendations, although a tight science and engineering labor market is making the laboratory's job challenging.

In addition to politicians and defense experts, a handful of scholars have become interested in the knowledge loss problem, either because it provides a valuable case study in paradigm shifts in Western science (e.g., Mackenzie and Spinardi 1995), or because it marks such a dramatic decline in the political fortunes of a once-powerful cabal (Masco 1999, Gusterson forthcoming). Masco, for instance, has observed, "Like any culture that has experienced the loss of cosmology, elder nuclear bomb designers in the 1990s were worried about how to preserve their cultural knowledge in the face of a rapidly changing world," (Masco 1999: 210), while Gusterson has pointed out that, "the total number of designers at Livermore and Los Alamos fell by about 50% in the decade after the end of the Cold War, a trend that made the designers feel theirs was a dying art" (Gusterson forthcoming: 10). Most provocatively, Mackenzie and Spinardi have suggested that, in the wake of a test ban, tacit knowledge might erode so greatly that certain highly sophisticated weapon designs might be accidentally "uninvented" as the knowledge to design, test, diagnose and build those devices disappears. Perhaps most seriously, they argue, the lack of a testing program might lead to an erosion of public and political confidence in the designers themselves, who may find their once-unquestioned "cognitive authority" over the stockpile challenged (Mackenzie and Spinardi 1995: 89-93).

In this chapter, I am not attempting to weigh the scientific or technical merits of SBSS. Neither do I wish to evaluate the laboratory's ability to evaluate the stockpile without testing, nor to question the "cognitive authority" of the weapons community.

Rather, I seek understand how the shift from testing to SBSS has impacted the weapons community's ability to reproduce itself as an integrated body of knowing selves. In doing so, I begin by describing what I have come to think of as the "official" elements of the knowledge loss problem. These include the disappearance of the design and test cycle as an experiential training site, an aging workforce, and an enduring stockpile that can no longer be renewed with new weapon designs. However, I interpret these "official" elements through an anthropological lens, arguing that knowledge loss is at least partly attributable to dramatic shifts in the temporal cycles that facilitated the reproduction of expertise and the integration of knowledge in the weapons community. The cycles that structured knowledge production during the Cold War are defunct, and these days it is not unusual to hear members of the weapons community describing their work as "race against time" to establish knowing ties with the stockpile under SBSS, to capture unwritten communal knowledge, and to train novices in the art and science of weapons work, all before weapons, knowledge, and experts become overly vulnerable to age. In addition, I explore how the adoption of Science Based Stockpile Stewardship has displaced some ways of knowing at the laboratory and changed the meaning of others. As a result, some experts are battling to reassert value of their knowledge under the new paradigm, others are struggling to adapt to new roles, while some members of the weapons programs are enthusiastically pursuing new knowledge production methods under SBSS. Because the weapons community's landscape of practice is still shifting, I argue, it is premature to make predictions about the inevitable demise of tacit weapons related knowledge, as the weapons community continues to recruit and train novices in old and new ways of knowing the stockpile.

THE END OF TESTING AND THE LOSS OF KNOWLEDGE

If Los Alamos is currently facing a knowledge crisis, it is perhaps because the laboratory was not prepared for a test moratorium, despite the fact that laboratory leaders had often testified that the cessation of the testing program would lead to an erosion of expertise in the weapons programs. During Congressional debates about test restrictions in the 1980s, for example, one senior designer testified that, "…the importance of long term stability in the weapons programs cannot be overemphasized… in providing a training ground for new scientists in complex subjects, some so difficult that only after a decade of work beyond the PhD is one prepared to undertake original research" (Robinson 1983). Similarly, former laboratory director Sigfried Hecker told Congress,

> Nuclear competence … has carefully evolved at the DOE nuclear weapons laboratories over the past forty years and will be required as long as there are any nuclear weapons at all…. Nuclear testing is imperative to maintain the competence and judgment of our nuclear designers and engineers. Every test is important in building and validating their nuclear competence (Hecker 1987: 10-11).

Even Lawrence Livermore's iconoclast physicist Ray Kidder, a retired weapon designer who has been lobbying in favor of test restrictions since the 1980s, admitted that maintaining weapons related expertise would be difficult without an active program of designing and testing nuclear explosives. "[Under a CTBT] weapon designers would become 'rusty' with time through lack of practical hands-on experience," he wrote in 1987. From Kidder's perspective, this was not an entirely undesirable outcome, since presumably "Soviet weapon designers would also become rusty through lack of exercise" (1987: 20), making it difficult for the two nations to continue engaging in the arms race.

Given the high level of political interest in test restrictions at the end of the 1980s, I was surprised to realize that the 1992 test moratorium caught many staff members off guard. When I asked one physicist to explain why the test moratorium was such a surprise, he replied, "Denial." Another retired designer, a former director in X Division, told me,

> The folklore in the late 1980s was, "We don't talk about test bans." We don't let anybody know that we may be getting ready for a test ban. Because if you do, that will almost certainly result in a test ban. In retrospect, that was exactly the wrong attitude.

Yet this kind of attitude does explain why the test moratorium came as a demoralizing shock to many weapons experts, many of whom were just beginning to grapple with the implications of the Cold War's end for the laboratory's mission.

Denial, however, is only part of the story. Several of the weapons experts I met readily acknowledged that they were aware of an imminent end to testing, and were concerned about maintaining the laboratory's knowledge base under a test ban, yet few weapons experts made significant efforts to "capture" weapons related knowledge for "preservation" in the event of a test moratorium.

In explaining why, I think it is critical to remember that the process of preparing and conducting a full-scale nuclear experiment was extremely demanding, creating a work environment that was not particularly conducive to reflection or conjecture. Although the testing program slowed considerably in the late 1980s, the weapons programs were still quite active: the last full-scale U.S. nuclear test, "Divider," was a Los Alamos experiment conducted at three o'clock in the afternoon on September 23, 1992, eight days before the Hatfield amendment went into effect. At that point, the laboratory was busily preparing for another test, Icecap, to be conducted in the Spring of 1993. Since completing a test required the full attention of its participants, who more often than were preparing for one or more other projects at the same time, experimenters were perhaps so caught in the momentum and rhythm of their research that they missed the proverbial forest for the trees.

Many of the moratorium stories I collected from weapons experts suggested just that. During one of my trips to Nevada, I chatted with a geologist about the end of the testing program. Although he did not remember passage of the Hatfield amendment, he told me a story that, for him, marked both the end of testing as well as his career as an NTS test geologist: in 1992, was performing geological studies in preparation for several DOE tests, one of which was a Livermore shot scheduled for execution that summer. The morning of the test, as the staff sat in the Control Point waiting to detonate the device, the Livermore test director stood and made a short, emotional speech thanking the staff for years hard work at the NTS. The geologist described the scene as "weird," telling me emphatically, "that kind of thing had never happened before." He went home wondering if he had witnessed his last full-scale nuclear test – as indeed he had. Looking back, he felt the test director had paid better attention to political events than many of his colleagues had.

Another of my interviewees was a senior assembly engineer named Jacob who had participated in several underground tests throughout the 1980s. He pinpointed the moratorium with the cancellation of Icecap. Jacob knew that Congress had signed a test moratorium into effect, "but it still didn't feel as though it had really happened," he told me, "even though we knew that politically it was a done deal." The reality of the moratorium struck a few weeks later, when the Department of Energy abruptly cancelled the upcoming Icecap test event. "That's when I realized that things weren't business as usual, I realized how politicized my job was," Jacob said. "My eyes were opened to the fact that careers and livelihoods could be cut at the whim of a politician."

The cancellation of Icecap was particularly difficult for its lead assembly engineer, Elizabeth, who at that time was the only female engineer in an all-male group. She explained that although the assembly engineering group had hired women before, none of them lasted very long. "After about two years with the group, I had demonstrated that I could do the work and that I wasn't going to be a short-timer," she told me. Elizabeth had finally attained a level of expertise at which she could be trusted to complete her own assembly and was excited about doing Icecap. "This was the first NTS test that a female engineer at Los Alamos was going to field," she explained. "It would have been a monumental event, to be the first woman engineer from Los Alamos to do one." However, just as she was beginning preparations for the final assembly at NTS, she got word that Congress had placed a moratorium on the testing program and that Icecap would be cancelled. "And I had gone through the entire development portion of it, presented the safety study, had all the parts fabricated, and I was ready to step on the plane when the moratorium hit. I felt totally frustrated, just shafted," Elizabeth said.

Knowledge Loss, Officially Speaking

From the perspective of most weapons experts, the knowledge loss problem began when the testing program ended. The Hatfield-Exon-Mitchell amendment coincided with President Bush's mass cancellation of orders for new nuclear weapons systems in 1992, and together these events effectively halted the design and test cycle. As a result, the laboratory quite abruptly lost its primary mechanism for integrating knowledge, maintaining skills in its experts, and training novices.

However, several other end-of-Cold-War factors exacerbated the laboratory's knowledge loss problem. Perhaps most significantly, there is the question of stockpile renewal: At the height of the Cold War, the DOE employed over sixty thousand people at fourteen sites across the country. However, the end of the Cold War and a much smaller stockpile made it difficult to justify maintaining the DOE's massive production complex, and by 1998, the DOE's weapons workforce had been reduced to twenty-four thousand employees at eight sites: the three weapons laboratories, the Nevada Test Site, and four production facilities (see U.S. Department of Energy 1998; United States Commission on Maintaining United States Nuclear Weapons Expertise 1999: iii). These days, the DOE has neither the facilities nor the skilled workforce necessary to remanufacture the weapons systems in the stockpile, while the weapons laboratories are no longer designing new devices to renew the stockpile. As a result, for the first time in the history of the weapons programs, the stockpile is steadily aging.

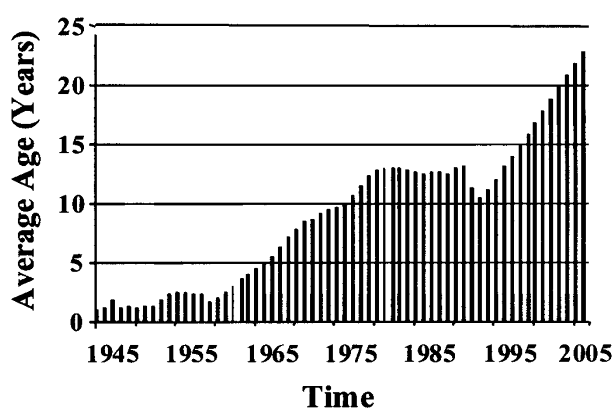

Figure 5-1. Average Age of U.S. Nuclear Stockpile Through the Cold War (United States Commission on Maintaining United States Nuclear Weapons Expertise Report 1999: 4)

With constant renewal between 1975 and 1995, the average age of all weapons in the stockpile remained a steady ten to fifteen years; by 2005, however, the stockpile will reach twenty-three years of age, as shown in Figure 5-1.

Under SBSS, weapons experts at Los Alamos, Livermore and Sandia are acting as "stewards" of the "enduring" stockpile. In comparison to the complexity of the design and test cycle, stewardship may seem relatively uncomplicated until one realizes that the laboratory has no experience maintaining weapons beyond their design lifetime. It is not clear how aging materials or components will affect the performance of devices in the stockpile, which "will change… through radioactive decay and other processes of aging, as well as through the maintenance and replacement of aged components" (Mackenzie and Spinardi 1995: 92). The Department of Energy makes a similar point when it describes the challenges of the Stockpile Stewardship program and contrasts the stockpile of the past to that of the future:

> In the past, a large, often renewed, and diverse stockpile provided insurance against… failure and defects compromising the safety and reliability… Nuclear testing could be done to provide unambiguous verification of the effects of design features, material changes, or safety issues… continuous development and production of new systems provided the U.S. Stockpile with the most modern and effective weapons, but also maintained the technical competence of the laboratory in the science and engineering of new weapons… today, none of these conditions exist (United States Department of Energy, Office of Defense Programs 1995: 3–5.)
>
> In the past our mission was accomplished on a large-scale with growth. Stockpile systems were periodically replaced with newer and better versions, a robust design and production capacity supported both stockpile modernization and the rapid implementation of stockpile

> repairs, and confidence was assured with the certainty of an underground nuclear test (LANL 1997: 1).

The past was a time of confidence, in which weapons knowledge was generated with relative ease through designing and testing new devices – note how the past is described with words like *unambiguous, renewed, verification, modern, effective, competence, accomplished, growth, newer, better, robust, capacity, modernization, rapid, confidence, assured, certainty*. In contrast, the laboratory describes the enduring stockpile in language that highlights the potential vulnerability of weapons to age:

> Current plans require systems to remain in the stockpile indefinitely, and therefore confidence in the readiness of the stockpile now includes an uncertainty driven principally by aging. Changes resulting from aging are expected in fundamental properties… aging mechanisms that cause these potential changes include the in growth of decay products, damage, and associated void formation… (LANL 1997: 1-2)

The new lexicon of corrosion includes words like *aging* and *change*, potentially causing *life-limiting developments*: *damage, swelling, instability, embrittlement, vacancies, voids, defects*. Given the absence of testing as a means of validating the stockpile, it is no surprise that the weapons community's historically confident relationship with the stockpile now includes an element of "uncertainty."

Of course, demonstrating the explosive power of a nuclear device was only element in producing nuclear confidence: throughout the history of the weapons programs, the laboratory has relied heavily on the expert judgment of the design community to certify the stockpile. That expert judgment resides in the laboratory's senior weapons experts, who are currently drawing on their long experience in designing and testing these devices to diagnose the effects of age on the stockpile and to suggest potential remedies. However, these experts represent a last generation of Cold War trained personnel, and they are aging in tandem with the stockpile. In its 1999 report on knowledge loss, the Chiles Commission noted that the end of the Cold War saw Los Alamos, Livermore and Sandia cutting workforce size through retirement incentives and separation programs. As a result, throughout the early 1990s, "the normal flow in and out of the workforce was seriously disrupted. For example, over the four year period 1993-1996, [Los Alamos] hired a total of about 115 scientists and engineers while more than 400 departed" (United States Commission on Maintaining United States Nuclear Weapons Expertise 1999: 9).

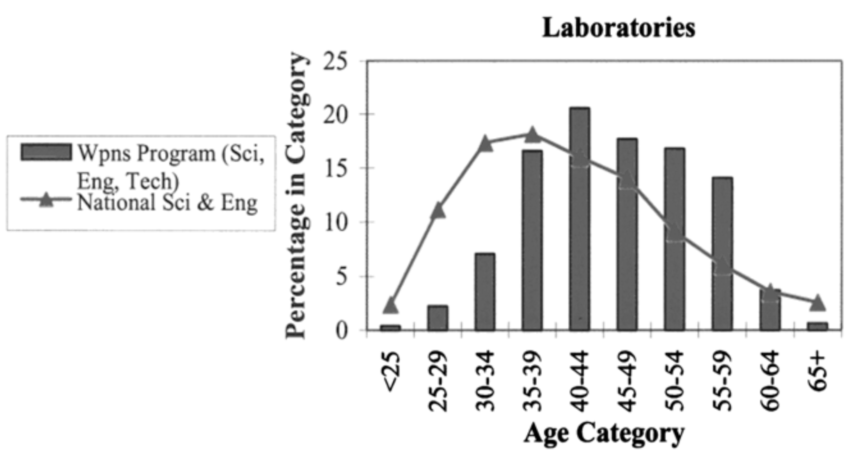

Figure 5-2. Age Distribution of Workforce at DOE Weapons Laboratories (United States Commission on Maintaining United States Nuclear Weapons Expertise Report 1999: 9)

As senior staff left, a second tier of mature, qualified staff – the people who learned the craft of weaponeering in the late seventies and early 1980s – assumed their positions in the weapons programs. However, as Figure 5-2 illustrates, this second tier is rapidly approaching retirement: the bulk of the workforce at Los Alamos, Livermore and Sandia falls between forty and fifty-five years, the latter age marking retirement eligibility for

Los Alamos and Livermore. Perhaps most notably, Figure 5-2 illustrates that the absence of a "third tier" of experts between the ages of twenty-five and thirty-nine.

Officially, these factors are the most critical elements of the laboratory's knowledge loss problem: as the authors of the Chiles Report put it,

> Notwithstanding the widespread perception that the principal new change is the ban on underground nuclear testing, there are other factors of considerable significance for the purpose of sustaining expertise, [including] a move away from a large complex and the challenge of an aging stockpile…. on balance, these fundamental changes define a new nuclear weapons workplace….Funding appears to have stabilized, the SSMP is providing a coherent planning focus, the current workforce is dedicated and talented, and training and hiring are resuming… [Yet] large numbers of workers are reaching retirement and a new generation of workers must be hired and trained in order to preserve essential skills (United States Commission on Maintaining United States Nuclear Weapons Expertise 1999).

Or, as one of the laboratory's staffing experts said to me one day, "We're looking at a train wreck."

However, while such official characterizations accurately several of the events and trends that have contributed to the knowledge loss problem at Los Alamos, they are incomplete because they fail to take into account temporal changes in the laboratory's ecology of knowledge production.

Time and the Social Reproduction of Expertise

During the Cold War, the laboratory's knowledge production activities were structured by a set of temporal cycles: the arms race, the cycling of weapon systems in and out of the stockpile, the weapons acquisition cycle, the design and test cycle. It is not far-fetched to compare the temporal rhythms of the Cold War to the agricultural and ecological cycles so prominent in agrarian societies. As Alfred Gell has written, cycles of agrarian production play a critical role in shaping the rhythm and pace of social life: "…the passage of time and the carrying out of a regular sequence of productive tasks and social activities cannot be dissociated from one another. Time is concrete, immanent and process-linked" (Gell 1996: 17). At Los Alamos, the cycles of weapons work offered an ongoing process of renewing the stockpile, for training new experts, for pushing the boundaries of communal knowledge and for reintegrating a diverse community of experts. However, the end of the Cold War quite literally brought an end to these temporal cycles, and in doing so, drove a wedge into the active "ways of knowing" that continuously reinscribed the relationship between weapons community and the nuclear stockpile.

For one thing, every iteration of the design and test cycle provided young weapons experts with an opportunity to gather what Sharon Traweek calls "replicable time," or periods of research activity that gradually accumulate into a base of experience for a career. Although the testing program did not end until 1992, by the late 1980s it was becoming increasingly difficult for novices to gain the replicable time necessary to be considered an expert. The massive antinuclear protests that took place in the 1980s created a political climate that was very hostile to continued testing. At the same time, the shots themselves were becoming increasingly complex: diagnostic technologies had improved significantly in the 1980s, making it possible to piggyback bundles of very sophisticated experiments on otherwise simple weapon physics tests. While diagnostic improvements improved the quantity and quality of data, they also made every shot more complicated and more expensive. Throughout the 1980s, tests were not only costlier, but required substantially more time to field.

William was a Nevada Test Site event engineer for Los Alamos. He was responsible for overseeing many of the set-up activities at the test site, and found that his responsibilities took more time as the tests grew increasingly complex:

> In the early days, we were doing - well, there were three [NTS event] engineers and we'd do two, three, sometimes four tests per year, and that was each one of us (emphasis added). So at that time the events were very simple. The diagnostics hadn't been developed to a point where they could do all the fancier things they were doing at the end. Generally speaking, when I got there in 1976, tests were simpler, quicker to field, and [equipment and people] spent less time in the field. In the beginning, if the rack was a pretty simple rack, it might be out there a month. But when the tests got more complicated, it took longer. Like a really complex rack, it might be out there six or eight months [while everyone worked on it].

By 1990, both Los Alamos and Lawrence Livermore were doing well if they managed to field between five and seven shots in a single fiscal year, a far cry from the twenty-five to thirty shots that the laboratories were able to field in the 1970s.

This trend had implications for how the field test experts defined experience, seniority, and expertise, all of which were linked to the number of shots that an individual had fielded in her/his career. While experienced staff members in the 1970s might have fielded over one hundred shots during their careers, staff who joined the program later had considerably fewer chances to hone their skills. This point was made particularly clear during one of my visits to the Department of Energy's Las Vegas offices in the spring of 1999. While pouring a cup of coffee in the office kitchen, I struck up a conversation with a semi-retired meteorologist who had worked for over forty years at the test site, forecasting weather patterns for fallout prediction. He mentioned to me, quite unselfconsciously, that he had worked with both Los Alamos and Livermore on over *seven hundred* nuclear tests in the both the Pacific and at the Nevada Test Site. "Yup, seven hundred," he repeated, sipping his coffee, smiling at my look of disbelief.

However, as testing program slowed, field test personnel had experience on fewer and fewer shots. As William explained, "During my ten years in Nevada, I think I ended up doing about twenty-five events. There were people that did more, but as the numbers went down, twenty-five was quite a few. So I'm considered really experienced now."

These days, without the design and test cycle, novice weaponeers can no longer gain experience by engaging iteratively in the process of fielding a full-scale nuclear test. Instead, some areas in the laboratory, such as the Engineering Sciences and Applications (ESA) Division, are relying on the local hydrodynamic shots and the subcritical program to give novices experience in conducting experiments. Indeed, I came away from my observation activities in ESA with the sense that the engineering community is counting on present-day activity to act as a bridge between a rapidly receding past and an uncertain future. In other words, by offering novices an opportunity to engage in the weapons engineering as practiced today, senior engineers are trying to teach their novices about the way weapons engineering was practiced during the Cold War, so that they will be able to extend that knowledge to full-scale nuclear testing, should the day arrive when they are called to do so.

Nevertheless, ESA's efforts to maintain its skill base through experiential learning have been hampered by fact that the DOE closed many of its production facilities when the Cold War ended. Because the United States is no longer manufacturing nuclear devices or their components, it has become increasingly difficult to get parts and materials for ESA's experiments, a situation that slows ESA's experimental cycles. As Elizabeth explained,

> With the moratorium in underground testing, they shut down or significantly cut down the production facilities, like Y-12, Allied Signal, Rocky Flats…. When we were in underground testing, you could get two or three parts at a time. We always had an infinite supply, and we had an infinite amount of money to do things. Now, in today's environment, we don't have an infinite supply of parts, there isn't an infinite amount of money. We have to use what is currently available and what had been fabricated [before the end of the Cold War.]

Difficulties in getting parts for hydrodynamic and subcritical tests, in turn, has had a snowball effect on the experimental process. As Elizabeth explained, "The number of experiments that we've fielded has dropped. We can only fire [maybe] four hydro tests a year right now." In contrast, one of the senior ESA-WMM technicians in the assembly bay told me, "We used to get twenty, twenty five of these hydros out in a year. It was a really different environment."

Complicating this situation is the fact that, as the rate of experiments has slowed, diagnostic complexity has increased: as Elizabeth explained,

> X Division, knowing that they have a limited amount of experiments, wants to get as much information as they can from each one of those experiments. So they turn around and put all the diagnostics they can to get all the information out. That complicates our experiments.

This further crowds the queue in ESA, slowing the pace of work and making it difficult for novice engineers to rapidly accumulate periods of experience. Elizabeth told me, "Oh, the experienced assembly engineers would do, maybe, two NTS tests a year. And usually with an NTS test, there were two or three hydro tests for each test." In contrast, most of the novice engineers that I met in ESA-WE had worked on one, perhaps two assemblies in the previous year.

Testing, the Demonstration of Expertise, and the Integration of Knowledge

During the Cold War, every iteration of the design and test cycle culminated in a nuclear test that, in addition to providing data about the explosive under study, served as an initiation ritual in which novice weaponeers demonstrated mastery of key skills and work practices. In addition, the culminating event of a test provided repeated points of focus for the integration of knowledge across the weapons programs.

In some areas of the weapons community, hydrodynamic and subcritical tests are fulfilling these functions: ESA-WE, for instance, has fallen back on local high-explosive shots to provide a *rite de passage* for novices. However, hydrodynamic and subcritical tests are not adequate for this purpose in all areas of the weapons community. This is particularly true in X Division: as the authors of the Chiles report pointed out,

> Weapons engineers are not as directly affected by the absence of nuclear test [as are designers]… the most drastic change affecting… the weapons design groups has been the disappearance of what used to be the key test which would show that a person was trained successfully, a nuclear test (United States Commission on Maintaining United States Nuclear Weapons Expertise 1999: 21).

Among designers, the ability to make confident predictions about the performance of the stockpile required an intimate familiarity with the complexities of nuclear devices. The laboratory relied on nuclear tests to demonstrate designers' expertise and to generate nuclear confidence. Without testing, many senior designers wonder how future generations will validate their claims to cognitive authority over the stockpile. As Robert, a senior primary designer, lamented,

> For reasons I don't entirely understand, weapons design never had the hallmarks of a true profession. Lawyers have their bar exam, doctors have medical boards, but we don't have anything like that. Why? Because it was understood that the important people were tested by nuclear test experience…. Now who certifies the experts, and in the future, who will certify their replacements? We might have a cadre of experts now, but we won't have them forever.

As I discuss below, X Division is actively developing a new set of certification standards for its novices, looking to academic models of peer reviewed independent research as a possibility for assessing the skills and creativity of new designers.

Certifying expertise without testing is a difficult question; equally troublesome is the problem of knowledge integration and, by extension, the reproduction of a unified community. During the Cold War, the pressure of meeting test dates created a crucible for the integration of knowledge as different groups coordinated their research activities in an effort to stay "on track." To a certain degree, subcritical and hydrodynamic tests are fulfilling this integrative function. The tests draw upon designers, materials experts, diagnostic physicists, engineers, technicians and others to design the experiments and create an integrated set of experimental artifacts, although the scheduling tracks are constructed differently than they were during the Cold War. As Elizabeth explained,

> We used to do a backend approach: "This is the delivery date, you will do everything in your power to make this date." Whereas now we're doing a front in approach that says, "Ok, we're starting right now on this subcrit, we're going to resource load everything, when can you deliver it?" So the stress level and pressure is different than trying to pick a date and say, "You will meet this date no matter if it's realistic or not." That's helped lower the stress level a lot.

Because the subcritical and hydrodynamic tests are the laboratory's best available substitute for nuclear tests, several skill areas – for example, diagnostics, rack design, weapon engineering – are counting on these experiments to maintain an integrated field test community that will be prepared to conduct a full-scale nuclear test if required to do so. In this regard, subcritical tests are particularly important: because they involve small quantities of plutonium, Livermore and Los Alamos must conduct the tests at the Nevada Test Site. They do so in a specially-designed underground facility known as "U1A" that is located near the Control Point. Currently, subcriticals are the only experimental program that actively exercises the laboratory's field test capabilities at the NTS.

At the same time, subcritical tests draw on a more limited range of personnel than the Cold War testing program did. In addition, the skills exercised are qualitatively different because the experiments themselves are not full-scale nuclear tests, although they do allow different groups to extend their practices into new research areas. And like hydrodynamic tests, it can be difficult for the divisions involved to marshal resources and to gather parts for the tests, a situation that slows the rate of occurrence. Currently, the laboratory is fielding roughly one subcritical test per year.

SCIENTIFIC CHANGE, IMPACT CONSTITUENCIES AND TECHNOLOGICAL DRAMAS

Complicating the end of the testing program, the adoption of Science Based Stockpile Stewardship displaced certain areas of expertise while reasserting the value of others. Simply put, the end of the testing program has meant that many experts no longer have the opportunity to do what they once did, either because their practices are not directly relevant to SBSS or because the adoption of SBSS has changed the nature of their engagement with weapons problems. Since 1992, many groups have seen their level of activity drop sharply as the age of their members steadily rises, a situation that has raised worries about "knowledge loss" among many senior weapons experts.

Diagnostic physics is an excellent example: when the laboratory was still testing, diagnostic experts played a key role in developing sophisticated data collection technologies for nuclear tests. While diagnostics are still critical in gathering data from hydrodynamic and subcritical experiments, the diagnostic community is no longer fielding the elaborate downhole arrays of sensors and cables that it once did. Although some experts are working on subcritical and hydrodynamic tests, many senior diagnostic physicists are archiving, cleaning, and annotating test data for future generations of weapon designers who will be certifying the stockpile. They are on a tight timeline: according to laboratory demographics for 2000, the average age of all technical staff in P Division was forty-nine years of age. Most of the diagnostic physicists I met and/or interviewed were in their mid fifties or early sixties. Not surprisingly, retirement is a common topic of discussion in P Division; one of the diagnostics groups lost sixteen members to retirement between 1996 and 2000.

Archiving and the Preservation of Knowledge

This kind of archiving activity has become the laboratory's primary tool for preserving the "knowledge" of experts who are no longer actively engaged in the design and testing of nuclear devices. Archiving is a relatively new concept at Los Alamos: although the director's office has maintained a small museum since the 1960s, the Archives and History Programs office where I worked was not established until the early

1980s. Even after the Archives were established, most weapons experts still put little effort into recording their research practices, decisions, trajectories, or methods. Several of my interviewees – designers, diagnosticians, engineers – told me that archiving for the future was not a priority because they were so engaged in research as a present-tense activity. As one former X Division designer explained,

> Postshot reports never got written as frequently as they should have. They were just another piece of baggage. Someone would go out and fire a test, and then the group leader might come by a few weeks later and say, "How's that postshot report coming?" and the response was, "Well, do you want me to work on that, or do you want me to work on this test that's coming up in six months?"

Now that the testing program is over, however, archiving has emerged as a cause celebre across Los Alamos, Livermore and Sandia. Los Alamos has established the Nuclear Weapons Archiving Program, or NWAP, which is coordinating and funding a variety of archiving projects, including so-called Cooperative Research and Development Agreements (CRADAs) with major industrial partners like IBM and Xerox to develop electronic archives, sophisticated scanning and retrieval procedures, and computer-based knowledge management programs.

For the most part, however, NWAP projects are aimed at documenting and organizing data to establish baseline specifications for remanufacturing weapons components in the future. In contrast, archiving efforts like the one in P Division and the Archiving Operation Julin project tended to be "expert-initiated" archives that were developed and executed by aging weapons personnel who were taking steps to capture and preserve their *own* problem solving methods, expert judgment, unwritten knowledge, experimental processes, and the like. The Julin project, for example, was championed by

Marvin, a Los Alamos diagnostic physicist whose father had once directed the United States nuclear testing program in the Pacific during the 1950s. When the atmospheric program ended in 1963, Marvin's father had authored a classified history of aboveground testing that documented many of the technologies, requirements, and procedures required to perform an above-ground nuclear test. In developing the Julin project, Marvin wanted to create a "state of the art" description of underground testing, one that would also map the organizational interfaces in the testing program, a schedule for important events, identify key positions and their responsibilities in relation to executing a nuclear test.

Efforts like the Julin project were far more interesting to me than many of the other NWAP projects, if only because champions like Marvin were so emphatic about the fragility of their knowledge and the importance of maintaining it for the future. For example, I spent a couple of hours one afternoon watching a senior diagnostic physicist in P Division carefully annotating data from a test conducted in the 1980s. He explained that many factors could influence the quality of the data, but that someone who had never worked on a test might not know to take account of these factors. "They're all in my head," he said. "It could be very hard to evaluate this data when I'm not around." Not infrequently, the senior weapons experts I interviewed also expressed a desire to leave a legacy of their contributions to the laboratory, so that their work could be appreciated by future generations of weapons experts.

Dinosaurs, Impact Constituencies and the Value of Knowledge

Perhaps most significantly, my identity as an "anthropologist" often produced jokes that played on tensions engendered by aging and inactivity in experts whose work was most deeply impacted by the end of testing. For example, when I was introduced at a meeting of the subcritical program in early 1999, one of the senior NTS engineers elicited chuckles when he said, “If an archaeologist is studying us, does that mean we’re history?” Throughout my research I was interested in making sense of these claims, which are real and widespread *despite* the fact that the SBSS program stresses the importance of knowledge preservation and the fact that the laboratory is funding a wide range of archiving projects involving senior weapons experts.

In the course of my fieldwork, I realized that the shift from testing to Science Based Stockpile Stewardship has transformed many of the laboratory’s expert subcommunities into what Bryan Pfaffenberger refers to as impact constituencies: “In technological adjustment,” he writes, “impact constituencies – the people who lose when a new production process or artifact is introduced – engage in strategies to compensate for the loss of self esteem, social prestige and social power caused by the technology.” In the ensuing “technological drama,”

> people make use of contradictions, ambiguities and inconsistencies with the hegemonic frame of meaning as they try to validate their actions… a technological drama’s statements and counterstatements draw upon a culture’s root paradigms, its axioms about social life; in consequence, technological activities bring entrenched moral imperatives into prominence (1992: 506).

Similarly, in her thoughtful discussion of identity formation and social conflict in American churches, sociologist Penny Edgell Becker points out that churchgoers tend to coalesce around particular modes of engaging with their church: defining the congregation’s goals, choosing the best ways of fulfilling those goals, expressing what it means to be a member of that congregation and the larger church. Several such modes of engagement may exist relatively quietly within a single congregation until some event brings one mode of engagement into explicit competition with another. The resulting conflict – which can destroy the viability of the congregation – is often expressed symbolically, as people point out and play upon differences between the way "*we* do things" and "*they* do things" (Becker 1999).

These days, several of these dramas are playing themselves out around Los Alamos, as various groups of Cold War experts assert the value of their "ways of knowing" to the future of the stockpile. If nothing else, these dramas make it quite apparent that nuclear confidence remains the weapons community's primary moral imperative. As one prominent member of X Division said recently, "We have to take care of the stockpile so that the nation doesn't get put into an untenable or a less secure situation, because confidence in weapons is gone" (United States Department of Energy 1998).

What is contested, however, are competing "best ways" to get there, some of which have enormous political and fiscal backing. The Accelerated Strategic Computing Initiative (ASCI), for example, had a great deal of direct support from President Clinton, who repeatedly characterized it as the key to nuclear confidence without testing. Within the laboratory, ASCI also has extensive and well-documented scientific support: many physicists and engineers at Los Alamos see ASCI computer simulations as the key to Science Based Stockpile Stewardship, since they will allow scientists to integrate enormous amounts of data from multiple sources with unprecedented speed and resolution. In many ways, supercomputing has emerged as the new scientific frontier in the weapons programs because, unlike many other areas of weaponeering, numerical and computational modeling promises rapid and striking advances in hardware, software, and visualization technologies. In addition, it is providing a new area of competition between Los Alamos Sandia and Livermore, as teams of computer experts in each laboratory race to make the next improvement in the computing speed of ASCI machines, which can currently process well over one trillion operations per second.

However, from the perspective of many experimental scientists, the laboratory's emphasis on ASCI has signaled a decisive shift away from experimental projects towards theoretical efforts. Translated practically, this has meant that a great deal of the laboratory's fiscal and resource priorities go to computational and numerical projects in the weapons programs, while experimental scientists outside the weapons programs – in biology, chemistry and physics, for example – have expressed frustration at high overhead costs, aging facilities and equipment, and difficulties keeping "good" experimentalists in the workforce. They argue that the laboratory's broad base of expertise in *both* experimental and computational sciences were the hallmark features of Los Alamos, which is one of the few institutions in the world with the budget and expertise required to support large-scale multidisciplinary projects. As one experimental physicist wrote in an open web-based forum on experimental sciences at the laboratory, "With all due respect to my theoretical and computational colleagues, the only reason to have a national laboratory is to provide multidisciplinary experimental capabilities to address areas of national importance" (LANL 2000).

Even within the weapons programs, many experimental scientists are irritated at what they perceive as an emphasis on simulation at the expense of "good experimental science." In doing so, they often question the relationship between computation and confidence. I interviewed one experimental physicist, a widely respected senior researcher, who rather scathingly likened ASCI simulations to a well-known Microsoft screen saver:

> You know the one with the fish on it? So you're watching it, and you see that the bubbles from the fish are going up to the surface of the water. And it all looks just fine, the fish and the bubbles – until you look at the bubbles. The small ones are rising faster than the big ones. Is that right? Of course it's not right. In reality, the large bubbles always rise faster. But if you had never conducted an experiment to verify the picture you were seeing, you'd never know that what the screen saver was telling you is all wrong. Well, ASCI is just like that, it's one big expensive screen saver.

ASCI's supporters respond to these claims by pointing out that they are painfully aware of the difficulties in validating simulated phenomena, and that they are validating their codes against past tests, cross-checking codes against other codes, and developing experiments to address this problem. "It is very, very dangerous to start believing your computer calculations," said one senior designer, "unless you are very carefully, very thoroughly tied back to data. It's the problems that you don't know about that surprise you… we have to be able to deal with whatever may come up" (United States Department of Energy 1998). They also point to formal mentoring and training programs in X Division as mechanisms to teach novice designers caution in their assessments and to make careful judgments when analyzing simulations.

New Roles and New Ways of Knowing

The shift away from testing has arguably had the deepest impact on the laboratory's experimentalists. However, even divisions that are still actively engaged in the weapons programs have changed in character. For example, primary and secondary designers are playing a key role in transforming SBSS into a working methodology for certifying the stockpile. However, when combined with the current moratorium on weapon design and weapon testing, the shift to SBSS has in a very real sense changed what it means to be a "designer." As Robert, a senior primary designer, told me,

> During the Cold War, I was a designer. Nowadays, we have analysts. There's always some question about a bomb in the stockpile, we want to determine the effect of a change on the performance of a warhead. So we analyze, we make judgments about whether the defect is important enough to matter.

The switch from designing to analyzing under SBSS was accompanied, perhaps predictably, with the reorganization of X Division. Until 1996, X-2 was known as the "secondary group" while X-4 was known as the "primary group." These design moieties were two of the most politically powerful groups in the laboratory, and their members were extremely attached to the numerical designators as markers of their location in the laboratory, in X Division and in the nuclear weapon design process. However, in 1995 X-2 became XTA, for "Thermonuclear Applications," while X-4 became XNH, for "Nuclear and Hydrodynamic Applications." The name change created a furor in X Division, where morale was already low. People complained that the letters were confusing and pointed out that, reorganization notwithstanding, most divisions in the laboratory used alphanumeric acronyms, not initials, to designate their groups. Several senior designers refused to acknowledge the new initials and continued to use the old system; one secondary designer even made a point of marking every one of her viewgraphs with "X-2" whenever she gave lectures to the laboratory.

Most of the senior designers that I met seemed resigned to their new role as assessors, although it is safe to say that they are not thrilled about the situation: as one designer told me, "Well, we're analysts now, and believe me, there are a lot of folks around here that don't like that very much." For one thing, many designers strongly believe that active engagement in design problems is the best way to shape the expert judgment of the novices who will be certifying the stockpile in the future. At a deeper level, the end of the testing program means that senior designers can no longer put their knowledge into practice as they once did. As Wegner points out (1998: 248), learning a field of practice involves a transformation of identity that is "profound and cannot be easily undone." Although the design community's expert judgment still provides a critical foundation for nuclear confidence, externally imposed restrictions on how designers put their knowledge into action creates a frustrating situation for experts who were used to revalidating their intuitive grasp of weapon physics in the visceral demonstration of a nuclear test.

At the same time, many people in the weapons programs are excited about their involvement in creating "new ways of knowing" the stockpile without testing. This is particularly true for theoretical and numerical researchers, for whom ASCI machines represent an unprecedented improvement in computational power. "ASCI," said one researcher, "totally blows your mind."

But computational physicists are not the only ones excited about SBSS: even some Cold War-era weapon designers are pleased with the opportunities that SBSS offers. Morris, for example, was not particularly nostalgic for the "good old days" of the design and test cycle. A junior designer in X-2 when the Cold War ended, he felt the end of testing had opened up the community's research options. When the laboratory was still designing and testing devices for the stockpile, Morris explained, designers had to meet a host of stringent requirements imposed by the military: weight, yield, fit to the delivery system, performance under different environmental conditions. Between the military specifications and the tight timelines for developing and certifying new systems, it could be quite difficult to explore new concepts.

In contrast, Morris told me, SBSS "allows us to extend our knowledge in new ways, to answer questions about significant findings. We've never had to extend our work in these directions before." Morris was excited about the possibility of isolating specific physics questions and using the new experimental facilities to answer them:

> You have to create experiments that maybe aren't directly related to the weapon that you're curious about, but that are more basic physics questions, then you extend that data to benchmark the code to certify the weapon. In some ways, that requires more creativity than other projects have. You have to make it relevant so that you're connecting two different physics experiments. That is a challenge.

In the course of our conversation, Morris mentioned that some of the novice designers in his group, physicists who had only joined the laboratory after the Cold War was well over, had listened to senior designers talking about the testing program and wondered what it might be like to design a device and test it themselves. Morris attributed their curiosity to the fact that they had never actually experienced the Cold War. "As a junior secondary designer, I worked on a lot of new concepts, but none of them ever made it to the stockpile," Morris told me. "This is much more exciting."

THE CONTINUITY OF KNOWLEDGE

In the past ten years, the laboratory has seen enormous changes in its research program: shifts in its temporal rhythms, the loss of the NTS as a place for integrating expertise and demonstrating competence, the formalization of a new paradigm for producing confidence in the stockpile. Without testing, is clear that certain forms of Cold War knowledge – for instance, context-dependent skills and unwritten problem-solving processes – could indeed disappear, as Mackenzie and Spinardi have argued, leading to an "accidental uninvention" of nuclear weapons. Weapons experts are quite aware of this problem and often speak of a "race against time" to make SBSS into toolkit of working methods, one that can re-establish knowing ties with aging stockpile, at the same time that senior experts are transferring their skills and understandings to novice weaponeers.

However, the more time I spent at Los Alamos, the more I questioned the idea, implicit in Mackenzie and Spinardi's argument, that the disappearance of tacit knowledge is inevitable. Ironically, I think, this argument places so much emphasis on the testing program that it winds up decontextualizing tacit knowledge from the remainder of the weapons community, treating it as an epistemological isolate and sketching scenarios for its disappearance. Their approach also stresses discontinuity in the weapons programs, despite the fact that the laboratory's core mission – maintaining confidence in the nuclear stockpile – has emerged substantially intact since the Cold War ended. In other words, rather than focusing on how and what forms of knowledge will be lost in the future, it may make more sense to ask in what form the weapons community is continuing to reproduce itself, loss of the testing program notwithstanding, and how the emergence of new contexts facilitates the longevity of established "ways of knowing."

For example, several of Los Alamos' weapons divisions have been developing their own customized training programs to facilitate the "transfer" of knowledge from experienced experts to novice weaponeers. Some of these are carry-over arrangements from the Cold War: in ESA Division, for instance, assembly engineers rely still on traditional mentoring relationships to train novices. This arrangement works well in ESA because many of ESA's practices are still in place: as one engineer explained to me,

> Whether it's [subcritical or hydrodynamic tests], the techniques and the processes that you have to go through to field those experiments are very similar to what one has do in an underground testing program. And ESA Division has maintained the rigor that it would take to go into and underground test to field those programs. The way we do our assembly procedures, our tooling analysis, the techniques we use, all of those would be implemented on an underground test. We still are high explosive assembly engineers.

In ESA, this hands-on approach has enabled the assembly engineering community to reconstitute some of the core capabilities that it nearly lost when the weapons programs ended.

In contrast, other areas of the laboratory are far more limited in their ability to carry the practices of the past into the present. X Division is a case in point, since weapon designers are no longer funded to develop new nuclear devices. Instead, X Division has developed a formal training program known as TITANS, the Theoretical Institute for Thermonuclear and Nuclear Studies.

TITANS was the brainchild of a mid-career secondary designer, Louis, who had never fielded his own test, though he was in line to do so when the moratorium went into effect. As Louis explained to me during an interview, he and several colleagues had developed TITANS as a response to the test ban. By 1993, as the test moratorium took effect, any people in X Division felt that they lost their mission, a feeling that “didn’t recede until 1995 or 1996.” In the meantime, junior staff in X Division had started moving away from weapons work towards applied physics problems in other research areas. This was a matter of great concern for Louis: “In the long run, I could envision dual trajectories: elder members would leave X Division and their weapons knowledge would go with them into retirement, while the new generation would diversify into other areas and leave the weapons stuff behind them,” he said. “And then who would be around to maintain the stockpile?”

Within a year or so, Louis and several of his colleagues had developed TITANS as a two-year, post-graduate program for novice weaponeers, complete with lectures, professors, seminars, homework assignments, and discussion sessions. Most critically, they replaced the validation provided by nuclear testing with a peer-reviewed thesis to be conducted at the end of the students’ second year. “This is very much like another dissertation,” Louis explained. “The senior designers are your committee.” TITANS had already graduated one class of ten or so participants, who started the program in October of 1996.

With Louis’ permission, I attended the first semester in the second iteration of TITANS, which began in the fall of 1999. The sessions were held from 1:10 to 2:00 PM thrice weekly in seminar room that held about one hundred or so people. It bore a great deal of resemblance to a university lecture hall, with the speaker at a podium in the front of the room and the students sitting in desks facing the lecturer. Initially, I assumed that most of the hundred or so people around me were staff members from X Division. However, I quickly realized that I was not the only guest in the program: only ten or were "career track" designers from X Division who were taking the course for credit. Like me, many of the people in the course were staff members from other areas of the laboratory who needed to know more about the weapons programs in order to do their work more effectively. Some of these were mid-career designers who had specialized in one area of weapon design and who wanted to learn more about other weapons issues. Other attendees were experimental physicists, engineers from ESA, even an historian who was writing a series of technical discussions about past tests.

Most of the TITANS courses that I attended consisted of overview discussions on a wide variety of weapons-related topics: Principles of Thermonuclear Operation, Fundamentals of Primaries, Special Purpose Weapons, History of Nuclear Weapons Delivery Systems, Nuclear Weapons Outputs and Effects. Louis started most sessions with brief announcements about homework discussion sessions or upcoming assignments, then introduced the day's speaker. Often, the lecturers were retired laboratory employees, many from X Division, who were either working part time at the laboratory or who had volunteered to give a TITANS lecture. In addition, the program drew on the expertise of other areas of the weapons community, such as weapon engineers and diagnostic physicists.

In addition to familiarizing novice weaponeers with the core technical issues in weapons work, TITANS very effectively communicated a strong sense of mission, community, and history to its audience. This sense of mission was directed in particular at the ten or so core participants in the program, the novice primary and secondary designers who would soon be taking over for retiring experts. For example, one lecturer opened her talk with an admonition: "We have been responsible for these weapons since the beginning, and you will be responsible for them in the future. This is a serious task." Other senior lecturers – some of whom had been working in the weapons programs since the 1960s – offered tales of heroism and defeat: designers whose efforts to make a particular concept work led to serendipitous discoveries in weapons physics; others who took an intractable problem and brilliantly transformed it into a working device against the expectations of their peers.

Initially, I thought of these stories as expressions of nostalgia that, while fascinating, were ultimately peripheral to TITANS' goal: transferring technical weapons-related knowledge to novices. However, as Etienne Wegner points out, history is critical for learning, insofar as novices must grasp the past of a community if they are to engage meaningfully with its goals and perpetuate the community into the future. For one thing, history is a resource that offers participants a repertoire of concepts, ideas, jokes, symbols, practices and beliefs that allow novices to begin to generate "'on the fly' coordinated meanings that allow the [community] to proceed" (1998: 84). Moreover, understanding the history of the community, its development and trajectory through time, allows new members to understand themselves in relation to the community. "Interacting with old-timers," writes Wegner,

> offers living examples of possible trajectories… in a community of practice, old-timers deliver the past and offer the future, in the forms of narratives and participation… the possibility of mutual engagement offers a away to enter these stories through one's own experience (1998: 156-157).

For novices who arrived at Los Alamos after the end of testing, TITANS has provided a window on X Division's history that is critical for the formation of their knowing selves as members of that community. It has provided impetus for senior designers to more carefully document and pass on their physics knowledge to existing members (LANL 1997b: 6). In doing so, TITANS has regenerated interest in weapons work in the next generation of weapons stewards: as one laboratory article described the program, "Frequently after class, there are a half dozen students who remain in the classroom to ask questions of the professor. The X Division hallways have been abuzz with discussion as students have been working and discussing homework problems" (LANL 1996: 2).

During my fieldwork, a colleague and I conducted a series of five interviews with young secondary designers in X Division, only one of whom had any direct experience as a designer in the testing program. Most had arrived in X Division after 1992 and were working on developing and refining codes and comparing the results to archived test data. Each was paired in a mentoring relationship with one or more senior designers, and several of these novices had participated in or were currently enrolled in the TITANS program. At the time we were conducting the interviews, we were trying to understand differences in the way novice and experienced secondary designers approached weapon-related problems.

In retrospect, however, one of the most interesting things about these interviews was the strong sense of continuity between generations. As a very basic example, both groups stressed the danger of putting too much faith in codes and simulations: as one young (male) secondary designer rather colorfully told me, "Codes seduce you. You wake up the next morning and realize that they've been lying the whole time." In other words, juniors seem to be learning, as their seniors did before them, that models and simulations are of limited utility without the judicious, deliberate analysis of an experienced interpreter. Novices have internalized this concern and are actively working with their seniors to develop that experience without testing. Expressed in the context of running codes, neophyte caution is itself a form of tacit knowledge, a way of approaching a problem, of understanding how one can best engage in the activity of knowing in the context of a design community that can no longer test its devices.

One of TITANS' participants is a young secondary designer, Frank, who is widely considered one of the laboratory's most talented new recruits. Frank spent a summer at Los Alamos while finishing his doctoral research in high-energy physics at an Ivy League university. During that visit, his mentor at the laboratory had given him some simple physics calculations to work "on the back of an envelope," he said. "You can get pretty far with pencil and paper." The experience was exciting. "Working on a nuclear explosion is like reading data from a star," he said. After he finished his PhD, he came to Los Alamos, joined X Division and started modeling explosion dynamics for secondaries. In his job he works closely with two of the laboratory's most respected senior designers, both of whom tell stories about the design process, the days of testing, the importance of intuition in code development. "There's a lore to doing things," he said, "Ways that things are done and ways they are not done. I'm learning the lore." This is a far cry from the scenario that Louis described in X Division between 1992 and 1995, when he saw X Division splitting into two tracks, an elder generation concerned with the stockpile and a younger generation that considered weapons research passé.

While many accounts of knowledge loss emphasize discontinuity in the weapons programs, I was often surprised at the great deal of continuity, and not just in X Division. Throughout my research, the weapons experts I met expressed a strong belief in nuclear deterrence, a sense of personal responsibility and mission, and a great appreciation, even a kind of nostalgia for the days of testing, all of which cross generational boundaries quite fluently. Knowledge of the past is a critical kind of tacit knowledge, one that enables young designers, engineers, technicians, materials experts, and others to engage effectively with the concerns for their seniors, despite the fact that the landscape of practice has changed dramatically in the past decade. Among novices, active interest in past practices signals the creation of a relationship that connects their knowing selves to a weapons community in transformation: its history, its present condition, and the possibilities for its future.

"Truth is linked in a circular relation with systems of power that produce and sustain it, and to the effects of power ***which it induces and which it extends."***

- Michel Foucault

CODA: KNOWLEDGE, IDENTITY AND PRACTICE

In the preceding chapters, I have explored how production of weapons knowledge during the Cold War was inextricably linked with the formation of identity at Los Alamos, as well as the maintenance and extension of the weapons community. In doing so, I have sought to understand how engagement in the scientific and technical practices of weaponeering reproduces larger structures of meaning that shape the lives and work of the laboratory's weaponeers. This is not merely an academic exercise: as Gusterson (1996) and Masco (1999) have both pointed out, the laboratory's weapons mission places it in close contact with military and political figures who define national and foreign policy. Similarly, Schwartz (1996) has traced how members of this expert community often pursue career paths out of the laboratories into key government positions in defense, science policy, and affairs of state. He argues that experts from Los Alamos and Lawrence Livermore

> …carry with them not only their technical expertise but also their narrowly constructed sense of values… they have played a vital role in shaping the government's options and priorities… for the direction of national policy on all aspects of nuclear weapons development and arms control (Schwartz 1996: 154).

However, explicit political linkages are only part of the picture. Michel Foucault quite deliberately chose atomic scientists as his paradigmatic "specific intellectuals," experts who have a "direct and localized relation to scientific knowledge and institutions," yet create and reinscribe a discursive regime of truth that is "universal… because the nuclear threat affects the whole human race and the fate of the world" (1984: 69). Throughout my fieldwork, I explored how the scientific practices of weaponeering at Los Alamos have established and reinscribed a discursive regime of truth in which the threat of nuclear destruction is a rational prophylaxis against the possibility of armed conflict. I have attempted to locate this discourse in the lived experience of the laboratory's weapons experts, whose research activities have offered them a means of meaningful engagement with issues of (literally) mortal significance.

Los Alamos' weapons community, I have argued, is best understood as a community of practice: a historically situated set of relations among persons, activity and the world that provides a stable location for the production of knowledge about weapons. Employment in this community provides an opportunity for individuals to engage with its goals, although at differential levels. An administrative assistant in X Division, for instance, will never connect with the practices of weapons research in the same way that her physicist colleagues in X Division do. At the same time, membership in the weapons programs has shaped her identity, so that she understands her work, her skills and quite possibly even her life in relation to the laboratory's mission.

Every trajectory of membership in the weapons programs is unique to the individual pursuing it, yet there are some common denominators, security being the most powerful one. Secrecy, and security practices that maintain it, provide a shared fabric of meaning and experience for all members of the laboratory. At a basic level, access to knowledge is one way in which laboratory employees locate themselves within the institution: as a Q-cleared, L-cleared, uncleared, or liminal as they await an upgrade in their clearance status. More dramatically, however, the rhetoric of security training teaches laboratory employees about Los Alamos' role in maintaining confidence in the nuclear deterrent, and by extension the stability of the American nation-state in a threatening world. During security training, laboratory employees learn that throughout the Cold War, weapons experts at Los Alamos and Lawrence Livermore created nuclear weapons that not only curbed the expansionist tendencies of the Soviet state, but were instrumental in bringing about its collapse.

Now that the Cold War is over, however, the United States faces a more diffuse set of threats that are capable of emerging unexpectedly from any quarter: from transnational terrorist groups, rogue nations, a re-emergent Russian state, even anti-government militants within the United States. In this new world order (or disorder), the role of nuclear weapons has changed. From an overt deterrent, they now exist as a kind of gold standard underwriting the continued hegemony of the United States as the world's sole superpower (see especially Mandelbaum 1997; also Huntington 1999). In the post Cold War world, Los Alamos is a repository for knowledge about the stockpile and is therefore a target for a multitude of threats – for instance, rogue nations pursuing nuclear weapons designs. Security practices are designed to monitor and control the behaviors, language and movement of laboratory staff to ensure that they do not give away state secrets, either purposefully or inadvertently. In this sense, security represents the power of the state in its repressive form, prohibiting laboratory employees from speaking openly about the secret aspects of their work and interdicting the transfer of information from Los Alamos to the world beyond.

However, to paraphrase Michel Foucault, repressive power is fragile power; far stronger is power in its productive form, as it "traverses and produces things… induces pleasure, forms knowledge, produces discourse" (1984: 61). Just as security at Los Alamos restricts certain behaviors, it simultaneously *produces* a community of knowing selves who understand themselves as stewards of the enduring nuclear stockpile and, by extension, as targets for the desire of other nations. In this regard, security and secrecy provide a meaningful context for the continued pursuit of knowledge about the nuclear stockpile. Expressed locally, within the confines of the laboratory, the language of security creates a discursive regime that articulates truths about human nature, the nation-state, and nuclear weapons whose role as technologies of stability and peace transcends the particular historical moment of the Cold War. Rhetorically speaking, the laboratory's mission lifts Los Alamos beyond the narrow geographic confines of northern New Mexico and places the laboratory and all its employees squarely within the elite realm of international security and power politics. In doing so, secrecy and security shape the ways in which people at Los Alamos understand their knowledge, their work, and their lives vis-à-vis the American nation-state and the world beyond.

Although the laboratory's role maintaining the nuclear deterrent is most vividly evoked in security discourse, it is also reinscribed in the scientific and engineering practices of the weapons community. In making this argument, I want to challenge the idea that scientific knowledge is somehow ontologically distinct from the context in which it is created. To a certain extent, sociological studies of scientific knowledge make the same point, insofar as they emphasize that science consists of local, context-dependent, tacit understandings and skills that are not easily replicated across time and space. Here I am more interested here in characterizing the relationship between the science of nuclear weaponeering and the laboratory's "sweeping worldview," (Pickering 1992). In doing so, I have argued that this worldview makes the pursuit of nuclear weapons knowledge a meaningful activity. More subtly, however, specific statements about weapons-related technologies were meaningful during the Cold War because they exist as statements of the weapons community's nuclear competence; and as any of the laboratory's senior weapons experts will attest, nuclear competence underwrites nuclear confidence.

In this regard, it is important to understand that throughout the Cold War, demonstrating competence was not solely the responsibility of weapon designers; rather, the entire weapons community participated in this project, from engineers to electricians, geologists to metallurgists, designers to machinists. The process of conducting a nuclear experiment required that expert subcommunities grasp the intent of other experts involved in the project, while simultaneously communicating their own understandings across disciplinary boundaries. However, the design and test cycle was not just a locally meaningful process that reinforced and reinscribed linkages among different members of the weapons program, recreating an integrated community of experts. Throughout the Cold War, weapons science helped to define the identity and position of the American nation-state as a particular kind of superpower, one that relied on scientific expertise and technological excellence as a basis for its military hegemony. The laboratory's regime of knowledge production played a significant role in defining America's military posture, so that even the most arcane and narrowly technical statements about any aspect of the laboratory's experimental process – radiographs illustrating implosion dynamics, the explosive properties of rare fissile materials – were important as statements of the laboratory's unique claims to knowledge, and by extension, the technological competence of the United States.

By virtue of its long engagement with nuclear weapons research and development, the laboratory is an institution in which the global and the local are frequently indistinguishable. Secrecy notwithstanding, Los Alamos is a kind of public space, a federally funded institution in which powerful discursive forces – for instance, nuclear deterrence, international relations, militarism, science and the American nation-state – are vivid elements in the lives and work of people within its boundaries. In this sense, Los Alamos is somewhat like the "mediating institutions" that Lamphere et al describe (1992), or the American religious congregations studied by Becker (2000). Within its confines, "macro-level forces are brought to bear on micro-level relationships," so that the laboratory's local culture has been directly shaped by some very powerful constraints and forces (Lamphere 1992: 4). At the same time, through its weapons mission, Los Alamos has quite actively *reinscribed* these forces, which have never been entirely external to the laboratory. Ethnography in places like Los Alamos drives home the important point that slippery, abstract discursive concepts like militarism, deterrence, scientific and political positivism (Gusterson 1996) and national security take root and thrive within local cultures, so that they are reinscribed and expressed in the lived experience of people who work in powerful institutions like the laboratory.

In other words, the weapons community allows members to routinely engage directly and meaningfully with institutions and forces that are well beyond the reach of most American citizens. This sense of personal immediacy and engagement is all the more powerful because is so closely linked to the acquisition and mastery of locally significant technical skills and the development of knowing ties that bind a wide range of expert subcultures into a purposeful community. Members of this community work in a context that stresses the importance of their work vis-à-vis the enduring nuclear stockpile. In this environment, technical skills become inextricably interwoven with a powerful sense of political responsibility, and weapons experts come to understand their knowing selves as meaningful contributors to the stability of the American nation state in a volatile and unpredictable world. This, I think, is one reason why the weapons experts I met, both Cold War and post Cold War generations, are so deeply attached to and convinced of the value of their work, despite well over fifty years of extremely cogent, often cutting critique from multiple quarters: social scientists, psychologists, philosophers, heads of state, physicians, religious authorities, a populist anti-nuclear movement, even weapons experts *within* the laboratories – Livermore's Ray Kidder, for example, or Ted Taylor from Los Alamos.

In this sense, I think, it is important to realize that the end of the Cold War displaced *people* whose ways of knowing were critical in the design and test process, but which are less active under SBSS: the ability to design and deploy a full-scale diagnostic array, for example; geologically containing a nuclear explosion, or being able to design a test rack that houses a set of interrelated experiments. Under SBSS, weapons experts who once were directly engaged with their peers, with nuclear weapons, with maintaining confidence in the deterrent and the American nation-state, are now struggling to make sense of their own diminished relevance and the possible mortality of their expertise.

Their situation is ironic, even poignant: the Clinton administration's reaffirmation of nuclear weapons and the DOE's subsequent adoption of Science Based Stockpile Stewardship saved their mission while simultaneously displacing their knowledge. Nuclear strategy made a strong comeback, but the rules of engagement for weapons experts changed dramatically.

As for the future of the weapons community, it is difficult to predict what it will look like in ten, twenty, thirty years. I have not yet met a weapons expert who envisioned a non-nuclear future, and given that the CTBT still awaits ratification, it is impossible to discount a return to testing. Even without a return to testing, it is quite possible that certain areas of the weapons programs – X Division, for example, and ESA – could benefit greatly from pursuit of SBSS, although the nature of "expertise" in these areas may change dramatically as a next generation engages with past knowledge and new techniques to establish novel understandings about a changing nuclear stockpile. One can also surmise that those areas of activity that do not translate themselves effectively into the SBSS paradigm – NTS event engineering, for example, or diagnostic physics – might atrophy rather quickly if practitioners retire without the opportunity to train newcomers.

Regardless, the survival of the weapons community in whatever form is largely a matter of sociopolitical context. It is difficult to envision a time when "the unleashing of the nuclear genie is so unlikely that threats of [nuclear] retaliation become unnecessary," (Turner 1997: 106) and therefore weapons experts, defense strategists, and politicians consider Los Alamos necessary for national security. However, it is important to remember this context is one that *the laboratory itself makes possible and meaningful* through its research, despite the fact that, like Traweek's high energy physicists, weapons experts are extremely reluctant to acknowledge their own agency in shaping the world. As Wolfgang Panofsky has observed, "…ultimately, we can keep nuclear weapons from multiplying only if we can persuade nations that their national security is better served without those weapons" (Panofsky 1994, cited in Mackenzie and Spinardi 1995: 88; see also Bundy, Crowe and Drell 1993). Until political and military leaders take decisive steps to change the context that makes weapons knowledge so valuable, experts at Los Alamos will continue pursue new ways of knowing nuclear weapons that, in turn, reinscribe the regime of nuclear truth.

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