"Whenever a scientist has a very serious message to convey, he faces a problem of disbelief. How to be credible?"
- Mary Douglas

TRUTH is Generated HERE: Knowledge Loss and the Production of Nuclear Confidence in the Post-Cold War Era

During the past decade, scientists and engineers in the Department of Energy's nuclear weapon design laboratories have been engaged in an often heated epistemological debate: given current restrictions on the design and testing of nuclear explosives, what is the best way to maintain confidence in the United States' nuclear stockpile?

Throughout the Cold War, Los Alamos and its sister institutions, Lawrence Livermore and Sandia National Laboratories, produced confidence in the nuclear deterrent through an iterative cycle of designing, testing, refining and stockpiling nuclear explosives. That cycle, which structured work practices at the national laboratories for forty-seven years, was abruptly truncated in July of 1992, when Congress approved the Hatfield-Exon-Mitchell Amendment to the Energy and Water Appropriations Act. Within a few months, funding for the Department of Energy's underground nuclear testing program evaporated, and the laboratories' core experimental program was quite literally left hanging, massive experimental assemblies suspended mid-completion over the dry desert floor of the Nevada Test Site (NTS).

In the wake of the test moratorium – which was renewed indefinitely in 1995 – the DOE's nuclear weapons experts have been working feverishly to develop a new certification paradigm that will allow them to maintain confidence in the nuclear deterrent without full-scale testing. Known as Science Based Stockpile Stewardship

Sandia is a multiprogram laboratory operated by Sandia Corporation, a Lockheed Martin Company, for the United States Department of Energy's National Nuclear Security Administration under Contract DE-AC04-94AL85000.
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(SBSS), the program provides the nuclear weapons laboratories several billion dollars per year to pursue a predictive capability grounded in basic physics, using a suite of advanced computational, experimental, and visualization tools.

Generous fiscal support notwithstanding, the transition from Cold War empiricism to the first principles approach embodied in SBSS has been neither smooth nor easy for the weapons laboratories. Each year, they are required to certify the continued reliability and performance of the seven nuclear weapon systems that make up the United States enduring stockpile without conducting any nuclear tests. Compounding the difficulty of this task is the specter of knowledge loss: since 1992, weapon designers and engineers have been worrying that crucial skills and understandings, tacit ways of knowing nuclear weapons, are disappearing as they retire. Without an active program of nuclear weapons work, there are serious concerns that weapons knowledge is quite literally facing an imminent (and, from the perspective of most nuclear weapons experts, untimely) demise.

The knowledge loss issue was pinpointed in a provocative essay by Donald MacKenzie and Graham Spinardi, for whom the nuclear weapons laboratories' dilemma raised the prospect of cognitive decay in the nuclear weapons complex. In 1995, three years after the test moratorium, they suggested that a permanent ban could, in some limited sense, bring about the gradual disappearance of nuclear weapons:

> As designers themselves age, leave and die, the number who have first-had experience of development through the point of full nuclear testing will steadily diminish, yet they will have to decide whether the inevitable changes in the arsenal matter. In such a situation, will explicit knowledge

> be enough? Will tacit knowledge and judgment survive adequately? For how long? (Mackenzie and Spinardi 1995, 92).

For the authors, the precarious state of post-Cold War nuclear expertise provides an opportunity to challenge the "traditional view" of science, in which knowledge is treated as "…independent of context, impersonal, public and cumulative" (1995, 44). An alternative account of scientific knowledge posits that formal method may be less important than *tacit knowledge*: those ways of knowing that resist formal expression and must therefore be transmitted relationally and experientially. If tacit knowledge is indeed necessary for maintaining technological expertise, and the current moratoria on the design and testing of nuclear explosives makes nuclear confidence dependent on explicit forms of weaponeering knowledge, argue the authors, then there is some sense in which nuclear weapons – or at least confidence in those weapons – may be "uninvented" (1995, 47).

Mackenzie and Spinardi are not the first to make this argument. Since the early 1950s, when a test ban was first suggested, its proponents have argued that a CTBT would slow the arms race by limiting its signatories' technical capability to develop new nuclear weapons. In complementary fashion, test ban opponents argued that testing was a necessary condition for maintaining the basis of technical expertise that underlies confidence in the nuclear deterrent (Brown 1986, Birely 1987). Nearly fifteen years after the moratorium, however, however, the United States maintains a substantial cache of nuclear explosives. Considering the impacts of the design and test moratoria, the exodus of experienced personnel, and the tremendous technical challenges presented by a rapidly aging stockpile, the United States' nuclear weapons programs are remarkably healthy. There have been no open challenges to the credibility of the nuclear deterrent, despite the fact that many of the most experienced designers and engineers have left the weapons programs. Moreover, deep cuts in the nuclear stockpile notwithstanding, the United States has not made significant progress towards developing a national security policy that does not rely on nuclear deterrence. Indeed, Congress is currently contemplating the development of a new nuclear system, the Reliable Replacement Warhead (RRW), which the weapons laboratories intend to certify without nuclear testing. The nuclear weapons laboratories, those quintessential relics of the Cold War, may be poised for something of a comeback.

In short, the terrain of weaponeering is simultaneously more stable and more contested than Mackenzie and Spinardi imagined. To explain this apparent paradox, it is necessary to broaden the scope of tacit knowledge, to understand the importance of community, identity and meaning in reinscribing a discursive regime of truth (Foucault 1981) in which nuclear weapons are a beneficial proscriptive technology, the ultimate arbiter in a world of conflict. Despite the fact that the mode of producing nuclear confidence has changed dramatically, perhaps irrevocably, with the end of nuclear testing, Los Alamos and its sister laboratories continue to play a key role in maintaining a regime of truth in which nuclear weapons – and thus the weapons community – remain critical for national security. Nuclear deterrence remains a potent motivator for the efforts of several thousand physicists, technicians, engineers, statisticians, metallurgists, chemists, computer scientists, and other researchers and technical experts who have spent the past decade working to sustain confidence in a stockpile of aging weapons that cannot be tested. In working to establish new "ways of knowing" nuclear weapons, weapons experts are redefining the very nature of nuclear confidence at a time when the role of nuclear weapons in national security is itself undergoing rapid change.

COMMUNICATION, ANTHROPOLOGY, AND WAYS OF KNOWING ABOUT NUCLEAR WEAPONS

Like their counterparts in communication, cultural anthropologists are interested in the study of "social things" (Lemert 2001) as a means of understanding how the actions of individuals reproduce and extend the social structures that they inherit at birth. This is a core research problem for all social scientists, methodological differences and disciplinary boundaries notwithstanding (Lukes 1982). Within the social sciences, anthropology is a quintessentially local discipline that has its roots in the long-term observation of small-scale, non-Western societies. The central organizing principle is firsthand participation and observation in an unfamiliar *culture*: the constellation of collectively held, experientially transmitted traits and behaviors that emerge as people develop ways to make sense of each other and the world around them. Cultural reproduction occurs as newcomers to a community gradually learn the geography, norms, practices, symbols, organization, hierarchy, language, frames, and tools that characterize the group. Over time, newcomers learn how to behave like a full member of the group, and reinscribe and extend what they have learned through their own action-in-the-world. To study culture, the anthropologist situates herself as a novice member of an unfamiliar community and, through engaging in the daily lives of the individuals around her, attempts to acquire an acceptable level of cultural competence. The actions, rituals, practices and behaviors of the natives, as well as their firsthand accounts of what such social "things" mean, are entwined with the anthropologist's trajectory of inclusion to create a "thick description" of the community under study (Geertz 1973). The product of this effort is *ethnography*, literally "the writing of culture."

The emphasis on locality and individuals in anthropology exists in some tension, albeit of the complementary kind, with communication scholarship, which looks to discourse as the primary autopoetic mechanism through which society reproduces itself (cf Taylor et al, this volume). Communication scholars study the discursive elements of human society, insofar as the signs, codes, words and images produced and consumed by people and the institutions they inhabit provide material for scholarly analysis. However, between communication and anthropology there is considerable overlap in subject areas, interests, philosophical frameworks, and even methods. These days, it is not unusual for anthropologists to incorporate the study of discourse in their fieldwork, while communication scholars use ethnographic methods to study the groups that produce discursive products. Moreover, communication and anthropology recognize what Michel Foucault made most explicit: that discursively defining the nature of truth necessarily involves the exercise of power, that knowledge and power are intimately intertwined (Foucault 1980; also Kinsella 1999). Anthropology's critique of truth lies in what Don Donham (1992) calls its 'critical moment,' its ability to illuminate the constructed nature of the taken-for-granted through exploring the exotic. Communication is more direct, dissecting the rhetorical frames and techniques through which power is discursively exercised in the form of truth-telling. In many ways, communication and anthropology bring complementary perspectives to the critique of knowledge, though the potential is perhaps not realized as frequently as it could be.

This is certainly true in regard to nuclear weapons. As détente fell apart in the late 1970s, communication scholars engaged in an intense period of studying, analyzing, and critiquing the contentious, oftentimes paradoxical discourse swirling around nuclear technologies (cf Taylor, Kinsella, Depoe and Metzler, this volume; Taylor 1998). The field has produced a rich body of social critique, particularly when focused on problems of social control, reproduction, power and meaning in complex societies – all themes that dominate nuclear weapons-related discourse (Taylor 1998). In contrast, anthropologists remained largely silent about such themes as nuclear war, deterrence, and realist theories of international relations (see especially Weldes, Laffey, Gusterson and Duvall, 1999). Gusterson (1999) has criticized international relations theorists for being so caught up in the discursive world of security studies that they missed the end of the Cold War. A similar critique could be aimed at anthropologists, for whom the Cold War's end "barely registered" as an ethnographic problem (Weldes, Laffey, Gusterson and Duvall 1999, 6).

Yet in the wake of the Cold War's end, a handful of anthropologists who came of age during the height of the Cold War began to pursue research in, around, and for the nuclear weapons laboratories. This trend is largely attributable to the three-decade repatriation of the discipline that began in the 1970s (see Hymes 1974, Fox 1991), which gradually increased the intellectual credibility of research in the United States vis-à-vis more traditional ethnographic excursions abroad – a problem, I might add, that communication scholars never had to address. As a result, anthropologists discovered some very interesting ethnographic problems in their own backyards (see especially

Ortner 1991). Hugh Gusterson's 1996 book about Lawrence Livermore National Laboratory was a breakthrough ethnography; not because Gusterson was the first social scientist to study a site in the US nuclear weapons complex, but because it was the first anthropological monograph to seriously apply frames and theories usually reserved for the nonwestern Other to a group of Western politico-scientific elites. Since then, a few anthropologists have pursued fieldwork about the nuclear weapons laboratories (for example, McNamara 2001, Gusterson 2004 and Masco 2006). Yet for many reasons – access to secretive institutions and funding being two of the most important – such work remains relatively rare in the anthropological community.

Knowledge as Construction, Communication, Culture

Treating science as a cultural problem is a relatively new phenomena in anthropology, and there are not many truly anthropological studies of Western scientists (with a few notable exceptions; e.g., Dubinskas 1988, Gusterson 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. Outside anthropology, however, there are many ethnographic studies of Western science, most of which emerged from the sociology of scientific knowledge (SSK) movement that developed in France and Great Britain during the 1970s and 1980s. Until the 1970s, most sociology of science in the United States and Europe reflected the institutional emphasis of the Mertonian school. Then, "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. Influenced by the writings of Thomas Kuhn and Michael Polanyi and using ethnomethodological approaches, they began to study the social settings and micro-interactions generative of scientific knowledge (e.g. Latour 1987; Latour and Woolgar 1986). 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): science viewed through the lens of Foucauldian critique (e.g, Latour 1987, 182-183; see also Kinsella 1999).

This body of work speaks to communication scholars and anthropologists alike: the former because it emphasizes science as a bundle of complicated and evolving communicative practices (Kinsella 1999, 174); the latter because it uses ethnographic frames and methods to illuminate science as a social phenomenon. However, where the social studies of science tends to be tightly focused on the nature of scientific knowledge, communication and anthropology ask broader questions about how communities – scientific or otherwise – constitute themselves. As I discuss below, a nuanced understanding of science as a cultural phenomenon requires that we move beyond questions of technical epistemology to focus on issues of community and identity, if we are to understand the remarkable resilience of scientific knowledge as a form of human practice.

An Ethnographic Encounter with Knowledge Loss

As an anthropologist, I spent six years as a participant-observer and researcher at the Los Alamos National Laboratory, conducting *in situ* observations of nuclear weapons scientists, technicians and engineers. I came to Los Alamos as a doctoral student in 1997 to study the emergence of "diversity" discourse in the laboratory workplace. To my surprise, I found that my identity as an anthropologist often produced jokes that played on tensions engendered by aging and inactivity among experts whose work was most deeply impacted by the end of testing. One afternoon, a physicist I lunched with commented, wryly, "I guess I can see why you'd want to study us. We're becoming a bunch of relics." Later, a middle-aged engineer stared at me when my physicist friend described me as an anthropologist studying the laboratory: "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."

Throughout the laboratory I met scientists who 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 point was dramatically illustrated when I visited a senior engineer whose office was decorated with a remarkable collage of nuclear ephemera: small posters, t-shirts, hats, stickers, certificates, and photographs from twenty years of nuclear tests. He explained to me that experienced weapons engineers were talking about retirement and, given the laboratory's foundering mission, he found it difficult to entice new staff with the promise of challenging work. He missed the Nevada Test Site and worried that younger staff members who lacked NTS experience might not be able to execute a weapon test if asked to do so. "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?” That evening, driving back to my little house in nearby Pojoaque, I passed Black Mesa, a volcanic formation that rises above the Rio Grande Valley and has deep historical significance for the Native American people of the nearby Pueblos. Nuclear weapons, cultural survival, engineering, and indigenous knowledge, I thought. What could they possibly have in common?

Over time, I came to realize that the ideational connection between these two is *identity*. Identity is more than the social categories in which we claim membership; it is our way of being in the world, as expressed in the countless actions we take throughout the course of a day, a year, our lives. Identity is simultaneously individual and collective, an emergent property of the many millions of interactions that people have with each other and the world around them. Studying identity requires observing people as they move through the world, as well as listening as they verbalize their understanding of the world. Identity can often be found as the predicate in the sentences through which we locate ourselves in the world *and* ascribe a location to others: I am a guitar player; we have a band; you are not from around here; my parents are Catholic. Statements like these not only verbally instantiate our *selves* vis-à-vis the rest of the world; they comprise texts that can be parsed, read, studied to understand the dynamics of emergent and evolving identity. Texts are the sensible traces of our ways of being and knowing in the world.

In this regard, community of practice theory (Lave and Wenger 1991, Wenger 1998) provides a framework capable of integrating communication’s focus on discourse with anthropology’s emphasis on culture. Communities of practice are variably organized 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). They are the geographical and temporal "places" in which cultural forms emerge and are perpetuated through the actions, beliefs, rituals, behaviors of people. Individuals belong simultaneously to multiple and overlapping communities of practice, which vary in size, formality and level of integration, from the family, to the classroom, to the tribe, to the corporation. 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). As such, community of practice theory broadens the discussion of tacit-versus-explicit-knowledge in science by linking the dynamics of knowing to the ongoing reinscription of shared identity, as expressed a community's discursive products and the practices of its members.

Where the sociology of science is largely silent on the significant emotive issues that lie behind jokes and comments about "dinosaurs," "relics," and "dying cultures," community of practice theory requires that we ask questions about the discursive perpetuation of worldviews, and the relationship between worldview and the loss of tacit knowledge. Understanding these requires not only making as visible as possible the cognitive processes and negotiations that generate knowledge in some fixed form, but also accounting for the social, political, economic, local and global contexts that make cognitive activities meaningful. In looking to see how the global makes its presence felt, however subtly, in the micro-ways that people live their work, anthropology recontextualizes the cognitive processes generative of scientific and engineering knowledge. Only in doing so can we fully appreciate the extent to which scientific pursuits are, like all other ways of knowing, inherently local, time and context dependent activities.

THE SOCIAL PRODUCTION OF NUCLEAR CONFIDENCE

Weapons science is a hybrid of engineering and physics, an applied discipline that focuses on a few microseconds of transition when firing energy from a fuse or trigger enters a stable nuclear explosive system and causes its parts to move, compress, merge, and finally blow apart (Hoddeson, Henriksen, Meade and Westfall 1993). Roger Shattuck has described this enterprise as a form of “forbidden knowledge,” an intellectual endeavor that “takes place on a slippery slope between pure knowledge and its application in the real world” (Shattuck 1996, 182). The ability to manipulate this knowledge, to transform the intimate understanding of arcane physics principles into working prototypes for new nuclear explosives, has been the preoccupation of several generations of the laboratory’s nuclear weapons community. Thousands of physicists, technicians, chemists, engineers, and other technical experts have devoted decades of career time, spent billions of dollars, blown up entire islands, and contaminated hundreds of square miles of Nevada desert in an effort to explore and characterize the nanosecond dynamics of nuclear explosions (Schwartz 1998).

For fifty years, the driving principle behind this vast effort was nuclear deterrence, technologically instantiated in the strategic triad of land, air and sea-based missiles and bombs equipped with “physics packages” – working nuclear explosive systems designed at Los Alamos, Livermore and Sandia, and mass-produced in the Department of Energy’s manufacturing complex. The resulting knowledge about the safety, security, and reliability of nuclear explosives provided the foundation for nuclear confidence, the principle that transformed the recursive, dead-end logic of nuclear deterrence into a workable foundation for defense policy (Rosenthal 1990). As one of the LANL'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 1987a, 2; see also White 1987b).

Or in the words of former LANL 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; also see chapter 2 in Fetter 1988).

Nuclear testing provided an epistemological basis for nuclear confidence, but not in a classical statistical sense. Although tests provided a great deal of data about explosive performance, they were far too expensive and difficult to perform multiple trials for any weapon system, much less isolate and repeatedly measure a single feature of a primary or a secondary. Moreover, the exigencies of Cold War weapon development required the weapons laboratories to focus on the next weapon system, not generate a statistically significant number of samples for established designs. Instead, American military strategists and war planners – the consumers of the laboratory's products – trusted the collective expertise of the laboratory's weapons experts because of their long track record of successfully designing, engineering and detonating a wide variety of working nuclear explosives. In a very real sense, the credibility of the nation's nuclear deterrent was rooted in the expertise of the individuals with the most intimate knowledge of nuclear explosives, so that the laboratory's weapons-related judgments were as much the bedrock of nuclear deterrence as were the weapons themselves.

The Design and Test Cycle and the Social Organization of Nuclear Confidence

Understanding how the production of confidence has changed in the past decade requires understanding the experimental cycles that once structured work at the nuclear weapons laboratories. Throughout the Cold War, interactions between the military and the Department of Energy's weapons facilities were coordinated through an eight phase acquisition cycle that structured the design, development, testing, manufacturing, stockpiling and retirement of nuclear weapon systems. Within this cycle, nuclear weapon designers at Los Alamos[1] played a key role as the voice of confidence. Housed in the evocatively named X Division, primary and secondary designers and their counterparts in LANL's weapons engineering groups worked closely with the military to identify advanced concepts for new nuclear weapons and developed experiments to validate novel designs. As such, this small coterie wielded enormous power in outlining the research agenda for the weapons programs.

However, designers neither built their own devices nor fielded their own tests. Instead, they relied on a vast, multidisciplinary community of scientists and engineers to

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[1] The judgments of weapon designers and engineers at Lawrence Livermore and Sandia National Laboratories were (and remain) equally important. However, my discussion focuses on LANL simply because that was where I conducted the bulk of my fieldwork.

translate design concepts into a working set of functional experimental artifacts.[2] When X Division was ready to field an experiment, it issued a design release that mobilized workers at Los Alamos and at NTS to begin preparations for a nuclear test. This could take several years of effort, as a typical nuclear test required the team to conduct preparatory high-explosive experiments, select a hole in Nevada, develop diagnostics for data collection, create a security plan and review all safety requirements, build the experimental device itself, design and machine a rack to hold the experiment and all associated equipment, lower the rack into the ground, backfill the hole, and finally detonate the device. A very complicated test could easily cost tens of millions of dollars and involve hundreds of staff members: weapon designers, diagnostic physicists, machinists, secretaries and other administrative assistants, physical security experts, mechanical and electrical engineers, radiochemists, construction engineers, drillers and mining experts, engineering technicians, materials scientists, geologists, crane operators, and electricians, among other disciplines.

Given the complexity of the testing program, I can safely say that no single individual ever understood everything there was to know about conducting a nuclear test. Instead, the design and test cycle was a shared *activity system* that integrated social, material, and individual components in the weapons programs, creating a context in which human agents could engage each other meaningfully in a collective problem-solving process (Keller and Keller 1996, 126). Every test catalyzed the emergence of a novel configuration of experts, each of whom understood their location in relation to their

[2] For more detailed descriptions of the United States design and test cycle, see Wolff 1984, Machen 1988, Gusterson 1995; Coolidge 1996; also Federation of American Scientists http://www.fas.org/nuke/guide/usa/nuclear/testing.htm. Shkolnik 2002 provides a thorough overview of cold War test operations at the Soviet Semipalatinsk site.

peers, and took responsibility for their role in bringing the experiment to fruition. Novice weaponeers learned their responsibilities by engaging with a particular sub-community of experts, with the level of responsibility, risk, and exposure increasing in proportion to the learner's level of participation in the process. This experimental program enabled its participants to reproduce and extend the social practices of nuclear confidence, and the collective identity of an integrated community, across generations of experts and weapons.

The design-and-test cycle waxed and waned but never really stopped. There was always another test on the horizon, iteration after iteration, like a series of waves, slowly rising and building towards an end point, then breaking into memory to make space for the next event. The many groundshaking nuclear tests that Los Alamos conducted demonstrated 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; see also Collins and Pinch 1998).

Testing and the Production of Nuclear Truth

During debates over nuclear test restrictions in the late 1980s, one of the laboratory's senior weapons engineers was known for pointing to a map of Nevada and exhorting his fellow weapons experts, emphatically, to remember that "TRUTH is generated HERE." The use of the word 'truth' evokes Michel Foucault's famous characterization of truth as "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" (Foucault 1980, 133).

In the context of the Princeton Plasma Physics Laboratory, Kinsella uses Foucault's proposition in demonstrating that scientific facts are the product of negotiations that occur within an institutional matrix that structures the social and cognitive practice of scientists (1999). Similarly, in a place like Los Alamos, knowledge products are not confined to disciplinary boundaries, laboratory property, or administrative structures. As Hugh Gusterson points out, nuclear weapons are meaningful 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. At LANL, scientific truths were (and remain) so tightly embedded in the political machinations of Cold War conflict as to be inseparable elements of the same historical context. The weapons experts who developed the stockpile were not simply makers of deadly machines; rather, their work actively reproduced and extended a regime of truth in which nuclear explosives exist as a redemptive technology, one whose very deadliness curbs what they believe is a "natural" human tendency towards violence.

All the weapons experts I met, Cold War and post-Cold War alike, expressed a strong sense of individual commitment to maintaining a safe, secure, and reliable nuclear deterrent. My interviewees were proud of 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 taken aback by statements like these, I learned not to automatically interpret 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.

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 for novice weapon engineers. Our group stood around an abandoned test rack as the tour guide, a former NTS electrical engineer, explained the swift process of detonating a nuclear device. 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 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 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." [3] 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.

WEAPONEERING UNDER THE NEW PARADIGM: STOCKPILE STEWARDSHIP

For the nation's nuclear weapons laboratories, the Cold War went out with a bang. On September 23, 1992, at 3:04 in the afternoon, a small group of physicists and engineers from Los Alamos stood in an underground cement bunker at the Nevada Test Site and detonated an experimental nuclear device for a test called Divider. The previous month, Congress had passed the Hatfield amendment, ending the testing program after several dizzying years of change in national security policy. Between 1993 and 1995, Los Alamos went through a series of workforce reduction initiatives that cut the total size of its workforce from a high of roughly 15,600 employees to less than 12,500. Morale in the weapons programs reached a nadir as "the normal flow in and out of the workforce was seriously disrupted…over the four year period 1993-1996, [Los Alamos] hired a total of

[3] Harold Agnew, former director of Los Alamos National Laboratory, is frequently credited as the person who first suggested that all political leaders should witness a nuclear test, so that they would be aware of their responsibilities to the world.

about 115 scientists and engineers while more than 400 departed" (United States Commission on Maintaining United States Nuclear Weapons Expertise 1999, 9). 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 1997, 69).

Ironically, the Clinton administration's opposition to nuclear testing instead brought a reversal of fortune to Los Alamos, since the establishment of Science Based Stockpile Stewardship (SBSS) meant renewed investment in experimental and computing facilities and experts at all three of the DOE's design agencies. By the time I arrived at Los Alamos in 1997, the laboratory had made a firm commitment to stewardship as the reigning mode of knowledge production. This commitment took shape in the "multiple forms of discourse, including oral, textual, and material productions" (Kinsella 1999, 175) through which Los Alamos instantiated itself as a post-Cold War institution capable maintaining its ties to the past as a hedge against uncertainty, while simultaneously embracing Science Based Stockpile Stewardship as the grand challenge of the future. For example, in 1999 testimony to the Senate Armed Services Committee, then-laboratory director John Browne stated,

> Maintaining the safety and reliability of our nuclear weapons without nuclear testing is an unprecedented technical challenge. The Stockpile Stewardship program is working successfully toward this goal, but it is a work in progress… I am confident that a fully supported and sustained program will enable us to

> maintain America's nuclear deterrent without nuclear testing… The Stockpile Stewardship Program has undertaken this unprecedented technical challenge, and to date it is working (1999).

Within Los Alamos, the message that testing was a thing of the past, and SBSS the way of the future, was hard to escape: the new employee orientation session I attended on my first day of work included a half-day session on the recent shift from forty-seven years of nuclear testing to SBSS. On a nearly daily basis, briefings, newsletters, white papers, electronic mail, journal articles, the LANL website, press releases, and the like provided opportunities for different parts of the nuclear weapons programs to describe their progress in making stewardship a success. In three years of fieldwork, my collection of Laboratory generated, stockpile stewardship-related ephemera grew to fill several large file drawers.

Yet institutional commitment notwithstanding, the shift from testing to stewardship was more contested than official discourse might suggest. Even with a revitalized mission and generous funding, weapons experts have spent the past decade struggling to make sense of ambiguous territory. The circular paradox of post-Cold War national security – in which nuclear confidence must be maintained without testing under a CTBT regime that itself depends, at least in part, on the DOE's success in maintaining the nuclear deterrent under Stockpile Stewardship – is tenable only because the weapons laboratories remain trusted stewards of the stockpile.

For weapons experts charged with making this policy work, knowledge loss is a very real problem. I often heard them ask the same questions raised by Mackenzie and Spinardi: will the laboratory be able to conduct a test, should a future President order withdrawal from the CTBT? Without test experience, will future generations of weapons experts understand critical weapons problems? What about their credibility? As Robert, a senior primary designer at Los Alamos, 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?

Reproducing this trust across generations *and* paradigms, without the epistemological bridge of nuclear testing, has created a great deal of stress for weapons experts, who often speak of a race against time to develop, verify and validate the SBSS toolkit and its practitioners, lest a surprise catch the community unprepared and force withdrawal from the CTBT.

The difficulty of keeping a foot in the past, racing into the future, and simultaneously maintaining confidence in an aging stockpile was a theme not entirely absent from official discourse around stewardship. For example, this excerpt from an annual report describing LANL's stockpile surveillance activities characterizes the past as a time of confidence, and the future as a period of uncertainty:

> 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. [But] 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 from fundamental properties… aging mechanisms that cause these potential changes include the in-growth of decay products, damage, and associated void formation (LANL 1997, 1).

Moreover, without the design and testing program as a mechanism for cultural and technological renewal, themes of age and death emerge as worrisome and salient fears not just for the stockpile, but for its stewards. As the Department of Energy's Office of Defense Programs noted in an early description of stockpile stewardship,

> In the past, continuous development and production of new weapons maintained the scientific and technical knowledge and skill base essential for maintaining the safety and reliability of the stockpile. With no new weapons in development or production, budget reductions, and an aging staff with actual experience in designing, testing and producing nuclear weapons, the knowledge and skill base unique to nuclear weapons will atrophy (DOE ODP 1995, 5).

Dealing with the problem of an aging stockpile was a relatively straightforward problem: in the 1990s, the Department of Energy introduced the Stockpile Lifetime Extension Program, or SLEP, which consists of a series of Lifetime Extension Programs, or LEPs one for each system remaining in the nuclear stockpile. Each LEP is designed to refurbish the warhead with updated components and subsystems, thereby extending the design lifetime of each individual system and the stockpile as a whole.

While refurbishing a warhead is one thing, maintaining the collective expertise of the Cold War weapons community is another problem entirely. As I describe below, Los Alamos and its sister design agencies addressed the problem of knowledge loss by developing strategies to capture, preserve, and transfer knowledge as a hedge against an uncertain future. Nevertheless, many senior weaponeers were frustrated with what they perceived as a lack of effort on the part of DOE to adequately address the problem of knowledge loss. Indeed, in 1998, the Congressionally-mandated Commission on Maintaining United States Nuclear Weapons Expertise (also known as the Chiles Commission) released a substantial report criticizing the DOE for its failure to develop a comprehensive knowledge management and expert succession plan for the weapons complex (United States Commission on Maintaining United States Nuclear Weapons Expertise 1998).

Some of this frustration is attributable to the shaky epistemological assumptions that underlie knowledge preservation: as I discuss below, the entire project rests on a misleading metaphor of knowledge as a commodity. However, much of it is directly related to stockpile stewardship itself, which represents a dramatic shift in the social organization of the nuclear weapons programs.

Knowledge Loss, Knowledge Preservation, and Impact Constituencies

Post-Cold War knowledge preservation efforts at the national laboratories tend to fall into three categories: the collection and archiving of artifacts generated during the

Cold War (drawings, memos, reports, input decks for computational models, physical mock-ups of engineered devices, and the like); the elicitation of Cold War expertise from an individual or a small group of individuals whose knowledge is considered unique; and formal training programs designed to ‘transfer’ skills across the generation gap.

In the early 1990s, Los Alamos established the Nuclear Weapons Archiving Program, or NWAP, to coordinate and fund a variety of archiving projects, including 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. Similarly, in 1993, Livermore established the Nuclear Weapons Information Project (NWIP), an effort to capture at-risk knowledge for transfer to future scientists and engineers. A year later, NWIP morphed into NWIG, the Nuclear Weapons Information Group, which included representatives from across the DOE, DOD, and even the UK’s Atomic Weapons Establishment (AWE). The goal of NWIG was to establish a broad information preservation effort across all the sites that had historically collaborated with each other in the design, testing, production and dismantlement of nuclear weapons. At Sandia National Laboratories, the NWIG took the form of a Knowledge Preservation Project, or KPP, that brought together groups of weapons experts to be videotaped as they discussed their involvement in specific programs and projects. The KPP staff also created transcripts and indices so that the videorecordings comprise an electronically searchable database.

Formal training programs also emerged as a strategy to combat knowledge loss. In the late 1990s a group of mid-career and senior weapon designers in X Division formed the Theoretical Institute for Thermonuclear and Nuclear Studies, or TITANS, a two-year, formal, classified post doctoral training program for novice weaponeers, complete with a peer-reviewed thesis and oral defense with senior weapon designers as committee members. Similarly, a small group of test-trained weapons experts at Sandia National Laboratories instituted a formal Weapons Intern Program in which dozen or so junior-level engineers from the Sandia workforce would spend a year immersed in learning about Sandia's role as the engineering link between the DOE and the DoD. Sandia's program was formally accredited in 2001, when it entered into a Memorandum of Understanding with the New Mexico Institute of Mining and Technology in Socorro to provide students 21 graduate credit hours towards a Masters' Degree in energetic materials.

Although these programs indicate substantial momentum within the all the laboratories' workforce around the issue of knowledge loss, none of the nation's three design laboratories ever made a top-down, comprehensive commitment to full institutional knowledge capture, management, preservation, or transfer programs, despite the fact that LANL managers often publicly commented on the value of greybeard expertise and the importance of capturing it for the future. Several factors explain the uneven commitment to knowledge preservation: for one thing, not everyone agrees that maintaining full design and test expertise is necessary for stockpile stewardship to succeed (see especially McKinzie, Cochran and Paine 1998). Moreover, training programs have been fiercely criticized as efforts to subvert the intentions of the CTBT. LANL's sister institution, Sandia, came under fire in 1998, when it was revealed that engineers were planning a flight test of the Bomb Impact Optimization System, or BIOS, a guidance system to improve target acquisition. Despite Sandia management assurances that the engineering design project was intended as a hands-on "exercise to hone the skills of Sandia's weapon design [engineers]," the laboratory was accused of attempting to get around Congressional restrictions on weapon design activities, and the program was abruptly canceled (Fleck 1998). Electronic knowledge management programs were weakened by Los Alamos' much publicized security problems, which caused Congress, the DOE and external critics to focus on computer and electronic security at the laboratories. In 1999 the *Bulletin of the Atomic Scientists* published a piece entitled "Steal This!" alleging the laboratories were developing "…the most attractive nuclear espionage target ever developed… a point-and-click computer network of weapons knowledge so complete that its theft by foreign spies would constitute a loss of virtually every nuclear weapon design secret possessed by the United States." Not surprisingly, the database that the *Bulletin* warned about never came into existence (Stober 1999).

Far and away, however, the biggest reason for the laboratory's uneven commitment to knowledge preservation was its continuing mission vis-à-vis the nuclear stockpile. Los Alamos and its sister laboratories were and remain weapons facilities, not archives or museums, and most of my colleagues around me were far more concerned about maintaining the stockpile in the here-and-now than they were about the eventuality of knowledge loss. Stockpile stewardship both aggravates and mitigates the knowledge loss problem, because the program represents a fundamental shift in the laboratory's traditional mode of producing knowledge: as former LANL Director Browne described it,

> The United States developed its nuclear arsenal using the same methods applied to most other complex systems: a sequence of design-test-produce… Today we

> are employing a new method: a sequence of surveillance-evaluation-response. In this new paradigm, we are using a fundamentally different set of tools to ensure the safety, reliability, and performance of nuclear weapons: the Stockpile Stewardship Program (Browne 1999, 4).

One consequence of this change is the displacement of Cold War experts whose traditional knowledge is no longer directly relevant to the work currently performed. For these individuals, knowledge preservation can be interpreted as a response to a rapidly shifting world. External disruptions bringing uncontrollable change are frightening, particularly when people perceive that traditional ways of knowing, markers of collective identity, are in imminent danger of disappearance. In response, people tend to become more conscious and deliberate about reproducing what is familiar. Although formal knowledge preservation and management programs are the kind of adaptive strategy that one might expect from a large, modern, Western, technoscientific bureaucracy like Los Alamos, anthropological studies of indigenous peoples describe similar patterns of response. For example, in her historical study of the Barolong boo Ratshidi of South Africa, Jean Comaroff describes how European colonization repeatedly overturned the Tshidi social system during the eighteenth and nineteenth centuries. Yet despite constant evictions, illness, proselytizing missionaries, environmental stress, and sporadic warfare, Tshidi chiefdoms worked to reproduce their spatial, agricultural, political, and domestic arrangements wherever they settled (1985, 42).

However, knowledge preservation is not just an adaptive strategy; it is a political one as well. As Bryan Pfaffenberger writes, during periods of social and technological adjustment,

> 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… 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).

If nothing else, the sociotechnical drama that is laboratory's adjustment to the post-Cold War era reveals that nuclear confidence remains the laboratory's entrenched moral imperative. Weapons experts' worries about the disappearance of knowledge reflect far more than the epistemological conundrum created by the end of testing. Instead, they highlight the social fault lines created when SBSS replaced the practices of testing with a new portfolio of tools and techniques.

At Los Alamos, impact constituencies are particularly visible among those disciplines most closely aligned with the Nevada Test Site, such as diagnostic physics. While the laboratory was still testing, diagnostic teams developed sophisticated arrays of sensors for downhole experiments; indeed, their central role in the production of nuclear confidence made the diagnostics groups some of the most politically powerful entities in Los Alamos. However, the realignment to stockpile stewardship, and drastically reduced activities at NTS, meant that downhole diagnostics would play a more limited role in the future. As a result, although diagnostic physicists are still heavily involved in small-scale and subcritical experiments, recruiting has dropped precipitously since 1992. According to laboratory demographics, in 2000, the average age of all technical staff in the diagnostic physics division was forty-nine years of age; indeed, one of the diagnostics groups lost most of its members to retirement between 1996 and 2000.

Perceiving a lack of interest in maintaining their expertise, some senior diagnosticians began in the mid-1990s to pursue strategies to capture and preserve their own problem solving methods, expert judgment, unwritten knowledge, and experimental processes. For example, one NTS expert was championing an archiving project dedicated to the last series of nuclear tests in 1992, to create a state-of-the-art description of underground testing that would map the organizational interfaces in the testing program, provide a schedule for important events, and identify key positions and their responsibilities in relation to executing a nuclear test, in preparation for the day that the United States might return to testing.

Other senior experts assumed responsibility for archiving their own knowledge. One afternoon, I spent a couple of hours watching a retiree carefully annotating data from a test conducted in the 1980s. He explained that many factors could influence data quality, but that someone without test experience might not know to account for them. "This is all in my head," he said. "It could be very hard to evaluate this data when I'm not around."

Champions of these internal knowledge preservation projects were emphatic about the fragility of design-and-test knowledge and its significance for the laboratory's future. Often they had to struggle for funding for their efforts. Few believed that the laboratory could maintain the stockpile indefinitely without testing; consequently, they envisioned a future in which the weapons programs would be unprepared to conduct a nuclear test when – not if – required to do so. They saw their archiving and training work as an important, if incomplete, means of shoring up the laboratory's eroding Cold War knowledge base. For these experts, stewardship pushed aside their ways of knowing, and their concerns about knowledge preservation can be interpreted as an attempt to assert the continued value of their knowing selves to the institution's future. Yet these same people routinely acknowledged that many of the technologies used at NTS were already becoming obsolete, that the laboratory's workforce was changing, that the DOE was committed to stewardship for the foreseeable future, and that the political consequences of returning to testing would be unimaginably high in any case.

But Does it Work?

Even if the national laboratories had fully committed themselves to a broad program of knowledge capture, preservation, storage, retrieval and transfer, the benefits of such an effort are dubious. Knowledge management is a problematic enterprise because it relies on a metaphor of knowledge as a commodity that can be elicited, captured, preserved, archived, stored. However, knowledge is not a commodity; it is an emergent property of human beings "coming to terms with actions and products that go beyond the already known" (Keller and Keller 1996, 127).

Knowledge preservation programs at the national laboratories tended to focus on creating and saving *things*: documents, drawings, even mock-ups of experimental assemblies. In community of practice theory, such items are reifications, or 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). Latour

(1987, 227; 236-237) refers to this property as "immutable mobility," meaning a text that fixes the emergent knowledge in a transportable form: as a drawing, a proposal, a model, a set of equations.

Considering the communicative role of reifications in the production of collective knowledge sheds light on the problems inherent in efforts to 'preserve' knowledge. Reifications are not ends in and of themselves; rather, their importance is as communicative vehicles that catalyze meaning-making. In critiquing knowledge management as a communicative strategy, Heaton and Taylor (2002) differentiate between *artifacts* (documents, drawings, even spoken statements) and *texts* by emphasizing the role of the latter as living media for communication. Artifacts are just things; they become textual only when people actively use them to negotiate meaning (Heaton and Taylor 2002, 222). Shorn from its embedding activity, an artifact ceases to be a text and becomes inert, meaningless, unless it is drawn upon to further catalyze activity.

Rooted in a metaphor of knowledge-as-commodity, knowledge preservation not surprisingly mistakes reifications for the activity of meaning making. In an activity system like the Cold War design and test cycle, artifacts like drawings, memos, and input decks for computer models were embedded in the process of knowing. A document that was generated during preparations for a nuclear test lost its embedding context once the test was completed; with the end of testing, that context is receding into an increasingly distant past. Archiving projects may enable future community members to revisit that document in new context – the writing of institutional history, for example, or to figure out how a particular stream of data was collected – but the consequence of the document for organizational sensemaking has changed (Weick 1995). As such, knowledge preservation falls short insofar as it focuses on developing and distributing stocks of potentially useful artifacts; the real challenge is ensuring their intelligent deployment in new and emerging contexts (Jackson, Poole and Kuhn 2002, 245).

Interestingly, it is in regard to emerging contexts that knowledge transfer programs may be having their greatest impact, insofar as they reinforce a sense of connection between the laboratory's past and its future. As I discuss below, the laboratory's mission remains a potent motivator for cognitive action, and institutional commitment to nuclear deterrence is the most important bulwark against knowledge erosion. Indeed, when this moral commitment is coupled with uncertainty about the aging stockpile, the resulting tension becomes the setting for the emergence of creative new ways of knowing about weapons, as well as novel frames for communicating confidence in the nuclear stockpile.

Plus que ça change, plus c'est pareil?

Anthropologist Bob Simpson writes that "…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). Simpson makes this observation in a discussion of ritual change among the Berava drummers of southern Sri Lanka, but it is quite apropos of LANL's weapons community. As I have discussed above, Cold War knowing was a communal project, in which the activities of many individuals came together in an integrated whole that was far more than the sum of its parts. Participating in this system required individuals to understand their social location in relation to other community members, and to develop a meaningful relationship with their peers.

Seen in this light, knowledge capture and transfer are far less important for nuclear confidence than reconstituting an activity system that enables people to assume responsibility for the laboratory's mission and the stockpile. The challenge for Los Alamos and its sister laboratories in the post-Cold War era is not the cryogenic preservation of the design and test cycle – itself an impossible task – but the development of a new activity system that catalyzes meaningful interaction across individual, organizational, and disciplinary boundaries. 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, Cold War's end notwithstanding.

For one thing, Los Alamos' *raison d'etre* is still national security, although security discourse has changed dramatically since the late 1980s. During the Cold War, the bipower rivalry between the Soviet Union and the United States provided the center of gravity for security discourse. In contrast, the end of the Cold War has multiplied the United States' list of perceived and potential enemies to include proliferant states like Iran and North Korea, an emerging China, a resurgent Russia, and – particularly since 9/11 – non-state actors and terrorist networks. 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). In this world of threats, neither President Clinton nor President Bush have advocated complete disarmament, despite steady reductions in the size of the stockpile.

As a result, Los Alamos and its sister laboratories continue to enjoy generous funding for the nuclear weapons mission, and the past decade has seen Los Alamos' weapons programs recruiting and welcoming more new staff than one might expect, given that the institution is no longer designing and testing nuclear weapons.

From the moment these new employees join the LANL workforce, they are immersed in a tightly rationalist discourse that reproduces this "culture of insecurity" in its members (Weldes et al 1999). As Joseph Masco writes, the laboratory's mission lifts nuclear weapons experts away from LANL's rural enclave and places them squarely in the center of debates about international security, stability and warfare (1999, 210). Security – the bundle of understandings and practices that protect classified information from improper release or compromise – plays an important role in creating and maintaining the sense of realpolitik that permeates Laboratory culture. 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. Along the way, novices go through a series of transformative rituals, including being granted a basic laboratory badge, spending time as uncleared personnel in exile from the classified world, undergoing a federal clearance investigation, and finally being granted a security clearance and full entry into the classified world of nuclear weapons (McNamara 2001). This process of learning is a process of becoming: as Hugh Gusterson has noted, secrecy is the "anvil upon which the identity of new weapons scientists [at Lawrence Livermore] is forged" (1996: 68). As they become more integrated into the core areas of the laboratory, novices learn that their affiliation with classified activities marks them as targets for the hostilities and desires of various enemies, and that they accordingly bear personal responsibility for the security of the American nation-state. In this sense, the formation of a secret, knowledgeable self at the level of the individual is simultaneously a community-building process that draws individual subjectivities together, forming a culture whose worldview is characterized by wariness, suspicion, and a preoccupation with international threat.

Within this institutional milieu, programs like TITANS assume an importance beyond their explicit goal of training young weapon designers in the technical intricacies of Cold War weapon design. Instead, TITANS provides participants with a tantalizing sense of what it meant to be a part of the design-and-test community, and enables them to envision how they might contribute to creating its post-Cold War future. In many of the TITANS lectures, senior weapons experts, some of whom had been working in the weapons programs since the 1960s, peppered technical lessons about primaries and secondaries with tales of heroism and defeat: designers whose efforts to make a particular concept work led to serendipitous discoveries in weapons physics; miserable and embarrassing failures; designers who took an intractable problem and brilliantly transformed it into a working device against the expectations of their peers. Hero tales like these are not just colorful anecdotes; instead, they offer novices a repertoire of concepts, ideas, jokes, symbols, practices and beliefs that allow them to generate "'on the fly' coordinated meanings that allow the [community] to proceed" (Wenger 1998, 84). Moreover, a shared sense of history is critical for learning, insofar as novices must grasp the past if they are to engage meaningfully with present goals and perpetuate the community into the future:

> Interacting with old-timers 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 way to enter these stories through one's own experience (1998, 156-157).

Likewise, among novices, active interest in past practices – including dedicated participation in a program like TITANS – 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. Although a sense of connection with institutional mission is not sufficient for maintaining the weapons community's cognitive authority over things nuclear, it is a necessary condition for novices to take the laboratory's mission into the future and, in the process, to re-define what it means to be a weapon designer in a world without design work, without testing, and without a Soviet Union. Hence, while several of Gusterson's interviewees at Livermore described stockpile stewardship as boring – one even likened stockpile maintenance to "polishing turds" – such ennui is notably missing among most of the primary and secondary designers that I met and observed, novices as well as test-hardened experts.

At the time that I observed TITANS classes in 1999, one of the program's participants was a young secondary designer who 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 told me. "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 motivated X Division's senior designers to create TITANS: a weapons community split into an elder generation concerned with the stockpile and a younger generation that considered weapons research passé.

UNCERTAINTY, CONFIDENCE AND THE STOCKPILE OF THE FUTURE

While newcomers' dedication to the maintaining a nuclear deterrent may be a necessary condition for successful cognitive engagement with the problems posed by a post-Cold War stockpile, it is not sufficient. Without testing, certification of nuclear primaries and secondaries is a tricky project: in contrast to the nonnuclear systems in a warhead, nearly all of which can be decomposed into testable components and subsystems,[4] the nuclear primary and secondary function as integrated wholes. Stockpile Stewardship has to do more than fund a collection of expensive stovepiped research and development programs; it must enable the community to integrate knowledge about artifacts that must function as integrated wholes, while demonstrating its continued cognitive authority around the stockpile.

[4] Sandia National Laboratories in Albuquerque, NM is the agency responsible for most of the engineered, non-nuclear subsystems in a warhead. Even under the test moratorium, SNL engineers have been able to test many of the components for which they are responsible. However, fiscal constraints are limiting Sandia's ability to perform extensive tests on its subsystems, with the result that Sandia is itself looking to a non-test based methodology for subsystem certification.

Ironically, uncertainty *itself* is emerging as the discursive basis of post-Cold War nuclear confidence; or more specifically, the development of formal methods for representing, analyzing, and minimizing uncertainty around the safety, security, reliability and performance of a nuclear device. Since around 2000, weapons experts at both Los Alamos and Lawrence Livermore – and increasingly Sandia – have been working to define, bound, and make confident statements about their knowledge: what they know about the aging stockpile, what they do not know, and to assess how "known unknowns" and "unknown unknowns" might impact weapon performance, safety, security, and reliability. New approaches to uncertainty, and the mathematical and statistical language and operations that enable weapon designers to represent and assess uncertainty, are commonly grouped as "Quantifying Margins of Uncertainty," or QMU. A recent Livermore publication compared QMU to testing and referred to the former as a "better way of certifying the nuclear stockpile" because it drives the weapons community to identify and resolve gaps in their understanding of nuclear performance (LLNL 2004). Within Los Alamos, developing new ways to think about uncertainty represents a major research area: for example, an Uncertainty Quantification Working Group brings together physicists, engineers, computer scientists, mathematicians and statisticians to debate and discuss the philosophy and mathematics of uncertainty on a regular basis. Not surprisingly, LANL's statistical sciences group has risen in political prominence vis-à-vis other research groups as interest in QMU grows. Indeed, one of the secondary designers who was in the TITANS classes that I observed recently completed an internal sabbatical with LANL's statisticians, so that he would better understand probabilistic approaches to modeling uncertainty in nuclear weapons problems.

QMU is also gaining discursive ground outside the national laboratories as a formal methodology for nuclear weapon certification. In 2002, when the Department of Energy renegotiated the University of California's contract to manage Los Alamos, the fiscal year 2004 performance objectives required both LANL and LLNL to refine and demonstrate an integrated QMU certification methodology (NNSA Livermore Site Office 2004; US DOE, 2). The National Nuclear Security Administration has called out development of QMU under one of the Stockpile Stewardship Science Campaigns, and has asked the laboratories to establish a working QMU methodology by 2010 (DOE Office of Defense Programs).

The ultimate test for QMU could come in the form of the proposed Reliable Replacement Warhead (RRW), to which Congress in 2006 appropriated NNSA $25 million for further study. The idea of RRW is to replace warheads in the Cold War stockpile with a device that builds in very large margins against failure – in that sense, RRW as an artifact will reify QMU's concepts of margins and uncertainties in its very physical design. The NNSA has characterized this as a shift from "certify what we have built" – which is what stockpile stewardship currently seeks to do – to "build what we can certify" (NNSA 2006). Indeed, some claim that maintaining the Cold War stockpile into an indefinite future will require an eventual return to testing; RRW, say its proponents, can be certified without testing, and therefore obviates any possibility that the United States would invoke Safeguard F (see especially Medalia 2006).

So, even without a return to testing, it is the weapons laboratories seem to be quite successfully reproducing their specialized ranks, 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. This is because survival of the weapons community in whatever form is as much a matter of sociopolitical context as it is scientific practice. 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 continue to 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 (1988), 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 a remarkably resilient regime of nuclear truth.

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