

3 A Pedagogy of Diminishing Returns: Scientific Involution across Three Generations of Nuclear Weapons Science

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In science studies we hear a lot about the construction of facts, the building of networks, and the growth of disciplines, but much less about the withering, decay, or arteriosclerosis of fields of knowledge (brought on either through natural exhaustion of a particular approach or through external constraints). We have a disciplinary bias toward scientific evolution and revolution rather than what I am here calling involution.¹ A generation of historians, sociologists, and anthropologists of science has learned from actor-network theory and the sociology of scientific knowledge (SSK) to focus on the building of scientific institutions and facts, and from Thomas Kuhn to expect a certain historical rhythm in the evolution of scientific fields of knowledge: first, a dynamic burst of creativity (the “revolution”) as the foundational ideas of the new field are laid down; second, a period of “normal science” in which gaps are filled in as the new knowledge is institutionalized; and, finally, as puzzles emerge that cannot be fully explained by the established paradigm, a new burst of creativity as another generation redefines the fundamental precepts of the field.² In this essay, looking at three generations of nuclear weapons designers, I follow and then depart from the Kuhnian script. Although the first two generations of nuclear weapons scientists conformed perfectly to the Kuhnian storyline, the final story is not about the punctuated equilibrium of scientific revolution, but about a process of scientific involution as nuclear weapons science has simultaneously matured and withered in a way that is beautifully evoked in a blues ballad once sung for me by a group of weapons designers from the Lawrence Livermore National Laboratory:

Went down to Amarillo
Lookin' for my sweet '53³
It was laying on a long white table
Looked cold and hard to me

Let it go, let it go, retire it
No city scrapers do we need

Take a 61⁴ and modify it.
Call it the mod 11-E

Now you can search this whole world over
From Frisco to Albuquerque
You can mentor anyone that you want to
But you'll never find designers like me

Now when I'm gone, just put me way down
In a hole off the old Orange Road.
'ttach a cable to my device can
So I can run those legacy codes (fading)
So I can run those legacy codes
So I can run those legacy codes.⁵

The great breakthroughs in nuclear weapons design were made in the early, heroic phase of American nuclear weapons design—roughly coinciding with the period of atmospheric nuclear testing—when the nuclear weapons laboratories at Los Alamos and Lawrence Livermore were smaller than today, the designers were younger, and the organization of weapons design much less formal. It was in this period that American nuclear weapons scientists learned how to control nuclear fission in the engineering of the first atomic bomb⁶; devised two-stage devices in which the energy unleashed by a fission bomb was used to create a larger, thermonuclear explosion⁷; shrunk these hydrogen bombs so they would fit atop missiles⁸; learned to use tritium to “boost” the yield of atomic weapons, thus creating fission-fusion hybrids; and designed narrow cylindrical weapons that would fit in artillery shells despite the need for a spherical implosion at the moment of detonation.⁹ The names of the men behind many of these achievements include some of the great physicists of the twentieth century and are well known outside the parochial world of nuclear weapons science: J. Robert Oppenheimer, Edward Teller, Stan Ulam, John von Neumann, Hans Bethe, Richard Garwin, Herb York, and Ted Taylor.¹⁰

The 1970s and the 1980s, when nuclear testing moved underground, were a period of routinization: the institutional apparatus for nuclear weapons design and testing grew, its scientific achievements shrank, and the arteries of the weapons design bureaucracy hardened. Attempts to perfect a third-generation nuclear weapon—the x-ray laser—failed and were abandoned in an atmosphere of scandal and disgrace.¹¹ The art of weapons design progressed, but by increments rather than great leaps: weapons designers learned to squeeze greater yields out of smaller quantities of plutonium so that nuclear weapons could be made lighter and smaller, weapons were made safer through the addition of Permissive Action Links (PALS) and the substitution of

Insensitive High Explosive (IHE) for conventional explosives,¹² and the supercomputer codes used to model the behavior of nuclear weapons were gradually refined. The names of the men (and now women) behind these achievements are largely unknown outside the nuclear weapons bureaucracy, and in some cases their achievements are only partially known within the weapons laboratories, thanks to the compartmentalizing effects of official secrecy in the weapons complex.¹³

Nuclear tests were forbidden after the end of the Cold War, and the practice and pedagogy of nuclear weapons science shifted again. Forced to largely abandon their nuclear test site in Nevada—a place where the desert sands encroach on the old bowling alley and cinema, now disused, as tourist buses disgorge camera-laden voyeurs to gawk at the nuclear craters—many of the old-timers elected to retire. Those that stayed have regrouped their forces in the virtual world of simulated testing, where they are attempting to train a new generation of scientists to maintain devices they cannot test. In some ways the scientific challenges of nuclear weapons design have shrunk to microscopic proportions: new designs are not built or deployed, and even the decision to substitute a new epoxy in an aging weapon can send a tremor of fear through design teams unsure if their weapons will still work. In other ways, the scientific challenges are suddenly magnified: how to design implosion, shock wave, and laser fusion experiments that will shed light on the performance of aging nuclear weapons in the absence of nuclear testing? How to use the physics knowledge of today to understand test data, long buried in dusty filing cabinets, from the 1950s and the 1960s? And how to convert old two-dimensional codes designed for Cray supercomputers into three-dimensional codes that can run on massively parallel systems now being designed?

With a nod to Max Weber, I call two of the three periods I have described here the “charismatic” and the “routine”; I call the third (nothing to do with Weber) the “virtual.”¹⁴ Obviously the periodization I have sketched out here is schematic, not to say contrived, but it is a useful optical device through which to investigate the practice of knowledge creation and pedagogy in the nuclear weapons laboratories of Los Alamos and Livermore. My argument, summarized in the crudest terms, would be that, as nuclear weapons science has progressed, as the weapons laboratories have grown and matured as institutions, and as the laboratories lost their central experimental practice, nuclear testing, the training of new weapons designers has become more lengthy and formalized, the knowledge they acquire more carefully codified, the contributions of individuals more anonymous, and the incremental leaps in knowledge less substantial. We see, in other words, a scientific bureaucracy engaged in an increasingly involuted pedagogy of diminishing returns.

My claims here are based on more than 15 years of research into the organizational culture of nuclear weapons scientists. I have been investigating the strange closeted worlds of nuclear weapons designers since 1987, when I first arrived in Livermore as an anthropologist engaged in my own apprenticeship program as a graduate student, embarking on field research among nuclear weapons designers.¹⁵ Since 1992 I have also been conducting fieldwork at Los Alamos and, to a lesser degree, the engineering support laboratory at Sandia (which has branches in both Albuquerque and Livermore), trying to understand how the end of nuclear testing has altered the practice of nuclear weapons design and the means by which a new generation of designers is trained. In my earlier work I focused on what, adapting a well-known phrase from Foucault, we might call the “pedagogy of the self”: the processes through which new weapons scientists learned to internalize the appropriate emotional and ideological orientation toward their work from the everyday discourses and practices in which they were immersed. In this essay I look instead at a more orthodox kind of pedagogy: the means by which neophyte weapons designers learn and elaborate their life’s craft.

The Charismatic: The Atmospheric Testing Era

In reading accounts of the Manhattan Project and the early years of the Cold War arms race at the Los Alamos and Livermore laboratories, one is struck by the extraordinary youth of the scientists and the relative informality (especially by contemporary standards) of the laboratories as organizations. Wartime Los Alamos was a place where, dislocated from the hierarchies of university life to a remote desert mesa in the West, a community of scientists crystallized in which divisions between faculty and students or European émigrés and native-born Americans were subordinated to the collective goal of mapping out a new kind of weapons science and engineering in which none had prior expertise in the strict sense of the term.¹⁶ At the outset of wartime work at Los Alamos, J. Robert Oppenheimer won a struggle with General Leslie Groves, the military overseer of the atomic bomb project, about the internal organization of the laboratory. Where Groves, concerned about the possibility of espionage at the laboratory, wanted to compartmentalize the laboratory and inhibit conversation among its scientists, Oppenheimer insisted that good scientific work could be done only in an environment in which open discussion and the sharing of ideas among all the leading scientists was possible.¹⁷ New scientists arriving at Los Alamos were given a 24-page mimeographed document called *The Los Alamos Primer* (Edward Condon’s notes on Robert Serber’s introductory lectures explaining the basics of what was known about

neutron cross-sections, critical mass, and so on).¹⁸ In the meantime, the team of scientists Oppenheimer assembled to undertake this collaborative research project was extraordinarily young, the average age being 25. Oppenheimer, the director of the laboratory, was himself only in his late 30s, while his two leading theorists, Hans Bethe and Edward Teller, were 36 and 34 respectively. "At thirty-four," recalled Stan Ulam, "I was already one of the older people." Other scientists working on the project were, like Oppenheimer's students Philip Morrison and Robert Wilson, much younger. Many were on leave from graduate programs in physics while they worked at Los Alamos. Frederic de Hoffman, who would go on to help found General Atomic in the 1950s, was 19 when he arrived at Los Alamos.¹⁹

This pattern was repeated when the second nuclear weapons laboratory was established in the California town of Livermore in 1952. The scientists who ran the Livermore Laboratory were even younger than those in charge of the Manhattan Project, most being "newly minted PhDs from Berkeley" with no prior nuclear weapons design experience.²⁰ One of these new PhDs from Berkeley, Harold Brown (later Jimmy Carter's Secretary of Defense), was put in charge of the thermonuclear weapons design division, A Division, at the age of 24. John Foster, in charge of the design division for fission bombs, B Division, was 29. Herbert York, the man put in charge of the new laboratory by E. O. Lawrence and one of the few with previous experience in nuclear weapons science, was 31. Discussing the group of men running the Livermore Laboratory, York recalled in his memoirs that "Teller aside, the average age of its members was just thirty and, except for certain modest projects set up deeply in and supported solidly by the larger laboratory structure, none had ever directed or managed any very substantial free-standing enterprise."²¹

Remarking on "the relaxed and unstructured atmosphere" at the new Livermore Laboratory, Sybil Francis observed that "the relatively small Livermore staff contributed to informality and overlapping organizational identities."²² Herbert York recalled that "Lawrence firmly believed that if a group of bright young men were simply sent off in the right direction with a reasonable level of support, they would end up in the right place. He did not believe that the goals needed to be spelled out in detail or that the leadership had to consist of persons already well known."²³ Although York was known to be the person Lawrence had put in charge of the new laboratory, he did not call himself "Director" for two years. "Whenever I wrote a letter to officials in Washington to propose some new element of the program, to arrange for the construction of a new facility, to ask for more money, or the like and whenever I wrote to officials at Los Alamos or Sandia to arrange for cooperation on some new project, I simply signed my name followed only by my address: UCRL, Livermore."²⁴

Los Alamos and Livermore in the early years, then, were institutions where managerial titles and hierarchies were relatively unimportant. Despite the high wall of secrecy placed around the laboratories,²⁵ a group of young scientists was given great freedom to explore new ideas in an atmosphere of openness and informality. Since these young scientists were inventing (rather than reproducing and extending) a field of knowledge and practice and the most experienced were still relatively inexperienced, there was little in the way of formal pedagogy and those without formal qualifications could be recognized for their contributions in a way that would have been improbable in larger, more hierarchical institutions. Thus, for example, one of Livermore's senior scientists from this era, Carl Haussmann, was said not to have even finished his bachelor's degree.²⁶ Later Livermore would appoint John Nuckolls as its director and Roy Woodruff as its Associate Director for Weapons Development—both men lacking PhDs but respected for their contributions to weapons science.

Ted Taylor, perhaps Los Alamos' most prolific warhead designer, recalls the informality and lack of bureaucracy at Los Alamos in the 1950s thus:

In my seven years at the laboratory I never had to participate in the writing of a single grant proposal. . . . In Los Alamos in the '50s someone would get an idea and go down the hall and get Preston Hammer to put it on the computer and six weeks later you get printouts and find out whether the guess was right. If the results came out interesting you go up and talk to Carson Mark [the head of division] and he often would find some flaw. Or he'd say something like, 'Well, I'll be damned,' and then you'd cut across the overpass to the middle of the laboratory, the head of the explosives division, and he'd say, 'That sounds great! We'll put it on the fission committee agenda.' A week, two weeks later we'd have a fission-weapon committee meeting, and sometimes flaws turned up there. When they didn't, OK, we'll put it on the list. List for what? List for testing, either in Nevada or Eniwetok, quite often in less than a year from the initial concept to the successful test.²⁷

It was in this early period—roughly, the era ending in 1958 with the commencement of the first moratorium on nuclear testing—that the most important breakthroughs in American nuclear weapons science were made. (Thus, in 1959, Darol Froman, scientific advisor to the director of Los Alamos, arguing that the end of nuclear testing might not greatly matter, was able to write to a member of the Atomic Energy Commission that "we have milked the nuclear weapons business pretty dry . . . [while] they [the Soviets] have a few quarts yet to go."²⁸) In these years American nuclear weapons scientists had developed reliable processes for the enrichment of uranium and the production of a new element: plutonium. They had devised two different ways of making effective fission devices: the gun-assembly weapon used on Hiroshima and the plutonium implosion bomb dropped on Nagasaki.²⁹ (This, in turn, required important advances in metallurgy, high-speed photography, the physics of shock waves, the engineering of

high explosives, and so on). They had then learned how to make two-stage devices—hydrogen bombs—that used x-ray radiation from fission devices (“primaries”) to heat and compress tritium and deuterium (stored as lithium deuteride) in “secondaries” so that nuclear fusion was achieved. They had also learned to use tritium and oralloy (enriched uranium) to boost the yield of fission devices, mastered the design of non-spherical implosion devices that had narrower diameters than spherical implosion bombs (and were therefore well suited to missile warheads and nuclear artillery shells), and had shrunk hydrogen bombs while enhancing yield-to-weight ratios enough that they could fit a one-megaton bomb into a missile warhead. Finally, they had learned how to make a bomb that maximized radiation output relative to blast (the neutron bomb), had developed sophisticated methodologies for measuring weapons effects beyond the mere explosive yield of the bombs, and had demonstrated the possibility of testing nuclear weapons underground with full radiation containment and accurate diagnostics. They also made bold attempts, finally cut off by the ban on atmospheric nuclear testing in 1963, to develop a nuclear-bomb-powered spaceship (the Orion Project) and to develop nuclear weapons optimized for the construction of harbors and canals, and even for mining and oil drilling.³⁰ Although Livermore managers argued as the testing moratorium went into effect in 1958 that, if only testing were to resume, further exciting advances were possible (atomic hand grenades, “clean bombs,” and ultra-small nuclear bombs for recoilless rifles, for example³¹)—nuclear weapons design from the 1960s onwards increasingly became, in the memorable phrase of one university physicist I interviewed, like “polishing turds.”³²

The period up to 1958 also saw the emergence and consolidation of a striking divergence in the organizational and design cultures of the two nuclear weapons laboratories. Los Alamos was founded by J. Robert Oppenheimer, possibly the premier theoretical physicist of his generation in the United States, and it bore both his imprint and that of the other great Los Alamos theorist, Hans Bethe, in that Los Alamos scientists relied heavily on theoretical calculations in their preparation of new designs. The heavy reliance on calculations also minimized the need for expensive nuclear tests and therefore conserved nuclear material which was scarce at the time Los Alamos first institutionalized this approach to nuclear weapons design during and immediately after World War II. The Livermore Laboratory, by contrast, was established by Ernest Lawrence, arguably the leading experimental physicist of his generation, and its scientists (the first of whom were largely Lawrence’s students) relied heavily on experimental trial and error and, increasingly, on computer codes calibrated against test experience. Sybil Francis described Livermore thinking in her fine historical study of the Livermore Laboratory: “Weapons development, within limits, could be pursued

without deepening fundamental scientific knowledge. This could be done by testing a variety of designs and selecting those that worked without complete knowledge of the reasons for their success."³³ Francis contrasted the Livermore style with Los Alamos' more theory-oriented approach:

Livermore scientists were comfortable pursuing designs for which it was "hard to make advanced calculations of expected results," a consequence of their experimentally-oriented background. As a UCRL budget document explained, experiment provided Livermore scientists the means of studying designs "that might go unused because of difficulties in making calculations." Another useful technique was computer modeling, in which Livermore invested substantial effort. Designs difficult to calculate meant Livermore developed empirically based models, resulting in a more incremental approach to weapons design: computer modeling, tests, then more modeling in preparation for the next test. Livermore thus made its earliest contributions to the weapons program in areas especially difficult to calculate theoretically.³⁴

Francis also argued that a sibling rivalry dynamic emerged in the relations between Livermore and Los Alamos, and that the Livermore Laboratory, always insecure about its right to exist as the second laboratory whose founding had been opposed by Los Alamos, consistently cast itself in the role of the "new ideas" laboratory in order to legitimate itself. (Dan Stober and Ian Hoffman report that, when the Livermore Laboratory was established, "the orders from its young lab director, Herbert York, were clear: 'Whatever you do, don't do it like Los Alamos.' For the researchers in the lab's Small Weapons program, that meant make 'a fission bomb that was anything but spherical.'")³⁵ Also, since two-thirds of the weapons entering the stockpile through the late 1950s were Los Alamos weapons, Livermore could afford to do speculative and exploratory design work, while Los Alamos was forced to devote its resources to validation of actual designs. In its preference for more ambitious and exploratory design work the Livermore Laboratory was not only responding to the structural exigencies of its relationship with Los Alamos; it was also expressing the persona of its other distinguished founding scientist, Edward Teller. Thus, while Los Alamos developed a conservative approach to weapons design, "the new laboratory worked on 'bolder' designs, less certain of success than those of Los Alamos."³⁶ Francis argued that Los Alamos, "more experienced, and with established ties to the military . . . gained responsibility for the highest priority and most urgent military requirements. Livermore found opportunities in nuclear systems that were more speculative, that did not yet have formal JCS [Joint Chiefs of Staff] authorization, or were low priority."³⁷

Thus, after a rocky start in which the "new ideas" laboratory's first three nuclear tests were "fizzles" (the weapons designers' term for low-yield duds), Livermore consistently pushed the envelope of nuclear weapons design in a way that Los Alamos did not. It was Livermore that shrank the diameter of fission weapons enough to make the

low-diameter Davy Crockett artillery shell for the army in the 1950s.³⁸ It was Livermore that pushed yield-to-weight ratios and shrunk a hydrogen bomb enough to produce the first hydrogen bomb warhead for a strategic missile—the Polaris system—after Los Alamos scientists had scoffed at the Livermore proposal.³⁹ It was Livermore that designed the first MIRV warhead. It was Livermore that introduced the first two-dimensional computer code in the mid 1950s. And it was Livermore that conducted the first underground nuclear test in 1957, in the face of opposition from Los Alamos.⁴⁰

But there was a dark side to Livermore's organizational culture of risk and creativity. Livermore's institutionalized propensity to cut corners and push forward with poorly understood design approaches led to periodic failures, even scandals, of a kind unknown at Los Alamos. Livermore's first two tests, which failed to even vaporize the towers from which the bombs were suspended, made the new laboratory the butt of jokes in Los Alamos, whose scientists eagerly photographed the still-standing towers.⁴¹ Meanwhile, during the test moratorium, Livermore rushed Polaris warheads into the stockpile despite a poorly tested mechanical safing system that, it was subsequently discovered, turned many of the warheads into duds.⁴² This risk-taking culture, essential to the younger laboratory's organizational persona, was to endure: in the 1980s Livermore scientists' optimistic and mistaken assurances that the x-ray laser—a critical component of President Ronald Reagan's Strategic Defense Initiative—was ready for the engineering phase would create a public relations disaster for the Laboratory when the weapon was finally cancelled amidst revelations that it never really worked and that the Associate Director for Weapons Design had secretly resigned in protest against his colleagues' representations of the weapon to the White House.⁴³ And in the 1990s Livermore's Associate Director for Lasers would resign after public revelations of cost overruns and wildly over-optimistic performance projections by Livermore's management regarding its huge new laser project: the National Ignition Facility (NIF).⁴⁴

At the end of the Cold War, a Livermore weapons designer said to me, only half joking, "The Soviets are the competition, but Los Alamos is the enemy." The fierce rivalry between the two weapons laboratories encouraged each, in the complex competition for contracts from the Atomic Energy Commission and the three armed services, to develop partly opposed institutional personae: Los Alamos as a conservative organization of good pedigree making carefully understood incremental design changes as it developed higher and higher yield hydrogen bombs for air force bombers and improved tactical weapons for the army, Livermore as a bolder organization using riskier design ideas Los Alamos had shelved in order to miniaturize nuclear weapons for

the navy and the army. The divergence of design cultures at the two laboratories was also a legacy of the approach to physics as craft of the charismatic scientists who established the research orientations of the two laboratories at the outset: Oppenheimer and Bethe, both theorists, at Los Alamos; Lawrence, an experimentalist, and Teller, a dreamer, at Livermore. In the words of the physicist Brian Dunne, "You've got the Livermore gang and you've got the Los Alamos gang, two different cultures. . . . Acolytes of Teller, disciples of Bethe. They are different tribes, warring tribes."⁴⁵ As the two laboratories matured and grew organizationally, these two design cultures were passed on to a new generation of weapons scientists who, instead of inventing a new field de novo as their forebears had, found themselves internalizing an established field of knowledge and making incremental refinements of it.

Routine: The Era of Underground Testing

The period from the Limited Test Ban Treaty of 1963 to the last U.S. nuclear test (so far) in 1992 saw the weapons laboratories grow into larger, more complex, and, inevitably, more hierarchical organizations. The Livermore Laboratory, for example, added 2,000 to its staff of 3,000 between 1958 and 1963 and, by the time I arrived to do fieldwork in 1987, had added 3,000 more employees for a total staff of 8,000 and an annual budget of \$1 billion. The Los Alamos National Laboratory was the same approximate size.⁴⁶ As the laboratories grew, the men who had pioneered the design of nuclear weapons trained a new generation of scientists to reproduce and refine their art. These were years when an increasing gulf between rank-and-file weapons scientists and their managers appeared. (One Livermore designer, lamenting the end of what I am calling the charismatic period of weapons science, told me: "Back in the early days there wasn't a separate group of managers looking out for themselves the way there is now. The scientists would just take it in turns to do managerial work, or they'd take decisions by consulting each other. Then there was a very small number of professional managers—financial people and so on, but they didn't dominate the whole thing. The lab was run by real scientists. Now, do you know how many managers there are at the lab? 105! 105 ADs (Associate Directors), PADs (Principal Associate Directors), DADs (Deputy Associate Directors), and DOODADS.")⁴⁷ Meanwhile, as weapons science became routinized, the weapons design process became increasingly conservative and bureaucratized and weapons design teams within each laboratory more specialized. The new generation of designers learned to further improve the yield-to-weight ratios of U.S. nuclear weapons and to make some safety improvements, but did not make any spectacular breakthroughs in the field of nuclear weapons design. Safety

improvements included the development of Permissive Action Links (PALs)—devices that allow a weapon to be armed only when the correct authorization code is entered; the development of Insensitive High Explosive (IHE), making it less likely that a bomb would detonate if dropped by accident; and the development of Fire-Resistant Pits (FRPs) less likely to detonate if, for example, a plane carrying the weapon crashed and caught fire.⁴⁸

Reflecting on the increasingly conservative design culture at his⁴⁹ laboratory, one younger Livermore designer, recalling Enrico Fermi's dictum that "you were no good if you didn't have some major surprises," lamented that "pushing the design physics" was no longer acceptable in an era when nuclear tests were fewer in number but greater in cost: "Before, when there were a lot more tests. . . then there was more chance taking—having someone pick a novel idea and trying to find out if it's doable. But in the short time that I've been at the lab, people are very careful and conservative. They try to get as much out of each test as possible."⁵⁰ An older designer echoed this: "These days there's tremendous pressure from Washington for each test to be a spectacular success. If the bomb doesn't go off or something, then there's all hell to pay in Washington."⁵¹ One designer who joined the Livermore Laboratory in the early 1960s, only to quit in the 1970s out of, he says, boredom, remarked of a highly respected designer at Livermore: "He's spent his whole life designing the same bomb over and over again, shaving a few centimeters off here and there but never doing anything fundamentally new. It only seems like exciting physics because it's so secret."⁵²

With these comments as prologue, let's now follow the process by which a "typical" young weapons designer was apprenticed in the arts of weapons design in this period of increased routinization and professionalization. Young physicists considering accepting a position in one of the design divisions would make the rounds of different design groups to see if they found what seemed to be a good fit anywhere. They might be drawn toward a specific area of weapons physics or toward a particular group of people. One designer recalls picking an assignment in Livermore's B Division⁵³ because he felt drawn toward a particular group of people whom he described as "very reasonable. I can see some unreasonable people in other groups. I feel that my group is wise, that's the word. The whole group is that way."⁵⁴

New designers were encouraged to maintain broad interests and even to publish in the open literature as they were learning their corner of the art of weapons design. This was partly to ensure that young physicists—accustomed to the university environments where they did their graduate work and unsure if weapons design would hold their interest over the long run—did not become too disaffected from their new specialization and instead focused on the extraordinary opportunities offered by

employment at a laboratory with the fastest supercomputers and the largest community of physicists in the country. The same designer quoted above recalled:

Once you step inside the gates [of the laboratory] it's a very open and university-like atmosphere. It's the closest that I've come to that kind of atmosphere outside a university. The Laboratory has tremendous resources to do work that would not be possible academically. Even now, I still believe that it's possible for someone to come up with an idea and do some interesting work, even if it's not related to the weapons program. They may not get a lot of recognition for it, but there is the possibility.⁵⁵

Similarly, a senior manager in Livermore's other design division, A Division, said: "We like people to keep publishing. When we bring people in, we tell them that: we'll give you a certain percentage of your time, and we want you to keep to that because we want you to interact with people outside the lab. I have continued to publish—not a lot, but I have a colleague who works with me, and over the years we've published quite a few papers. It's low key, but I have an international reputation from that work. It's not my number one priority in life, but. . . ."⁵⁶

Many of the younger weapons designers developed strong apprenticeship relationships with older designers who served as mentors in relationships that carried great intellectual and emotional force. They learned from these mentors not only the arcane and secret knowledge about weapons design of which they were custodians, but also how to navigate the bureaucratic shoals of laboratory life, how to carry one's expert judgment as a weapons scientist, and how to interpret ethical conundrums and geopolitical puzzles in the broader world. One Livermore designer, when I asked him which relationship in his life was most important to him, mentioned the relationship with the weapons designer to whom he had apprenticed himself, not his relationship to his wife of many years. Describing this relationship, he said his mentor "is totally honest and has a total lack of respect for authority. He will be straight with everyone, whether it's an Associate Director or a shop-floor machinist. And, because he's a man of integrity, he's disliked by managers. He's incredibly knowledgeable. If a company is producing a part, he often knows more about what's happening on the shop floor than the company's managers do. That's why they often dislike him! If he'd worked for Morton Thiokol, he would have stopped the *Challenger* launch. He's more responsible than anyone for the integrity of our weapons program."⁵⁷

The heavy emphasis on mentorship and apprenticeship at the weapons laboratories was largely an adaptation to the small scale of the weapons design community and to the powerful effects of official secrecy on the exchange of ideas within that community. In the weapons laboratories of the 1960s, the 1970s, and the 1980s, intellectual life was both compartmentalized and informal. Although weapons designers sometimes told

me that becoming a weapons designer was like doing a second PhD in physics (and, indeed, it took about the same length of time), unlike graduate programs in physics, the weapons laboratories provided neither formal classes nor textbooks. In this period weapons designers did not yet have their own classified journal in which to publish so, when knowledge was written up, it was often in a gray literature of shot reports and other documents that was not well inventoried and was opportunistically and eccentrically stored in the accumulated files of individual scientists building collections of documents over their careers. Moreover, the Department of Energy's classification system, segmenting knowledge and obstructing a scientist's access to knowledge in areas where he or she was deemed not to have a "need to know," meant that there was little in the way of a public roadmap of the entire topography of nuclear weapons knowledge. More important still, a number of scientists worried that some of the most important knowledge about nuclear weapons science at the laboratory was not written up at all, existing instead as tacit knowledge in the heads of the most experienced scientists. "Seymour's retirement will be a blow," one weapons designer told me, speaking of his legendary senior colleague Seymour Sack in Livermore's B Division. "He has such a great memory that he hasn't written down lots of important stuff. How will people know it?"⁵⁸ The emphasis on learning through personal relationships that permeated nuclear weapons pedagogy at the laboratories was an adaptation to this situation where knowledge was not formally codified in carefully evaluated and ranked reports of the kind that circulate in geographically dispersed academic communities unified by open literatures, and where some of the most important knowledge was not written down at all but could only be acquired, experimentally, through trial and error and, dialogically, through interaction with mentors. It is this feature of nuclear weapons pedagogy and expertise that has led Laura McNamara, an anthropologist who has recently written a fine PhD dissertation on weapons scientists at Los Alamos, to describe weapons design knowledge as "a form of situated action, located in a nexus of relationships that linked weapons experts to nuclear artifacts."⁵⁹

To show exactly how this process of knowledge acquisition and elaboration can work, I reproduce here extended excerpts from an interview with a weapons designer who had gone on to become a manager in one of the weapons design divisions. The narrative, self-consciously rendering a neophyte's progress as an abstract ideal type almost as an ethnographer might, shows how a new recruit generates questions and ideas by working through the new field of weapons physics and by performing calculations sub-contracted by senior team members, learns to defend his or her ideas in the often brutal environment of review meetings, and works as part of a team to translate an abstract idea into a test. Recruits who excel are good at working collaboratively in

inter-disciplinary teams and in defending their ideas and calculations in meetings. The narrative also underlines the vital importance of experiments, especially nuclear tests, as core instruments of pedagogy and apprenticeship. As Laura McNamara observes, “throughout the Cold War . . . 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 space for the next event. There was a constant flow of work, a recycling of the same process, year in and year out, so that the activities involved in testing were constantly being exercised on the various experiments that were ongoing at any one point.”⁶⁰ The weapons designer agrees:

You’d come in and you’d learn a lot of new physics which is not known outside the lab, which is very interesting in itself. And in the process of learning that physics you often get new ideas. It’s our fresh people coming in that are really hot with new ideas. So what you’ll do is you’ll mess around with theory at your desk, and you don’t get very far with that usually because we work with very complex physical processes. That’s why we work with these big computer codes. They are to handle many types of physics simultaneously. You can’t do that with equations on paper. And so we train all the people that come in to use these computer codes as tools to study the physics. So you’d mess around with computer codes to study your idea, and, if it looks good, then you’d go to your group leader. Everybody’s in small groups of 4 or 5 people—that’s what we find is most productive—and so you have this great idea and you want to work on it. We work as teams; if your team is about to have an experiment come up, particularly one in Nevada, then your group leader will say to you, “you can’t work on that right now because we need you. Everybody’s working together and people are depending on you to do predictions for this experiment.” If you’ve just done an experiment, then he’ll say “go for it!”

And so there you are with your idea. What you do then if your idea still looks good is you go to your division leader. Someone might come to me and say, “I want to propose an experiment.”⁶¹ At that point you are asking for \$50 million, typically. So that what happens then is that you go through a very intensive peer review process. We start pulling together groups of people, and you’d brief your idea to maybe fifteen or twenty people, and they’d try very hard to find reasons why it doesn’t work because this is a very expensive business and you do everything you can to find errors before you actually go do the experiment. So you keep doing that. These are not nice reviews. They’re very critical. I’ve seen men all in tears.

Question: What percentage of the ideas would survive?

Answer: small percentage. The culture in that division—this is really interesting too—the big reward in our division is to do an experiment, to get your idea tested. It’s highly competitive. For every twenty things people propose, maybe one is going to make it onto that shot schedule. If you have a really good idea. Let’s say it’s a REALLY good one, and it’s recognized to be that way, then what we will do is put everything aside in the shot schedule. We have a lot of flexibility. And, just like magic, we’ll matrix a team together. We can do that in a couple of days. The minute we put an experiment on the shot schedule that happens. So all of a sudden you find yourself with fifty people supporting you, all trying to help you research this idea.⁶²

Of those fifty probably half of them do not have the skills to run a big program, and we will have found that out by having them run smaller projects, which is part of life there; and you can see as you go through that process who has the ability to successfully carry out a big project and who doesn't. . . . We have people, for example, who are forgetful of details, and forgetting certain details in these experiments can be disastrous. That's why we have these reviews. . . . A typical review for a new idea lasts about three hours.

So then we have this big team there, and they've got a goal they're aiming for and they're all starting to work together, and they're all depending on you to tell them, to make predictions for them using computers and theory and whatever else you have, to tell them what to measure. They might be building big spectrometers. They can cost millions of dollars and they get blown up in the experiment. So there's tremendous pressure to get the predictions met. So now you start down this track and the whole thing is just a very fast-moving process. We keep reviewing over and over again. . . . I spend probably fifty percent of my time reviewing things. We have a review on a project almost every day. Then we will go through a long discussion about which [experiments] look best scientifically, which ones have the most national importance, and I hope that what we'll end up doing is making a balance between the various needs we have. We will say, ok we'll spend a third of our resources on the fundamental physics issues, and a third of our resources on the direct national need.

[In preparing for the review] [y]ou do lots and lots of computer calculations. You try and anticipate every question that somebody's going to ask you. And you prepare computer graphs that will answer those things as best you can. You try and identify what are the weak parts of it. Usually there's physics in it that we don't know, otherwise we wouldn't be doing the experiment. You identify what you need to find out, need to measure. Oral briefings are the way we communicate. There's even a culture about the review, and people learn it by osmosis. You start out by presenting what's been done in the past that's relevant, and then you present your idea, everything you know about the physics, what you think the pitfalls are.

Question: How many hours a week does your average physicist work?

Answer: I'd say probably sixty. It varies. When you have a shot coming, a lot. The experimentalists are waiting for you to get them predictions so they can design the diagnostics to fit them, so then you'll be in there night and day keeping the computers going. You kind of ramp up and work and, after the experiment, usually people take a vacation.

As you get closer to shot-day, there are some critical times called "Freezes." First you freeze what the design of the experiment is like, and that's because you have to give people time to build, and so there's a frenzy of activity, and that's the last chance you have to decide what you want in the experiment. Then you'll freeze the diagnostics, the measurement devices, and that's another frenzy because that's when you've got to put it on the line and say "I'll measure 10 to the 22 gamma rays coming out at 30 degrees," and then they set their measuring devices around that number. As soon as they start building the diagnostic can you go out and crawl through it and you peer up at places where you're looking for anything that could possibly go wrong. You crawl all over it, frequently.

And then you reach the day where you go out to the test site, and there's this huge 200-foot canister filled with all this beautiful equipment and they're about to put it down. That's a real gut-wrencher.

That's it. You go through a period where you have a lot of doubts because the computer codes don't cover everything, and you have to use your judgment in a lot of places about what it's going to do physics-wise, and there's a lot of places where it could go wrong and you won't know. Most of our experiments don't come back with data as predicted. That's why we do the experiments. This is a hard period for the designer, especially the younger designers. If it's your first shot, it's really worrisome. Actually, if it's your second shot, it's really worrisome. I find that people believe the codes too much the first time. They always think that those big fancy computer codes can't be wrong.⁶³

The narrative finishes with an invocation of one of the most important words in the weapons designer's lexicon: "judgment." The goal of apprenticeship, teamwork and experimental experience at the Laboratory is to enable weapons designers to acquire this elusive quality of judgment—what we might think of, to bend Evelyn Fox Keller's celebrated phrase, as a "feeling for the bomb." Judgment is demonstrated not simply in the ability to predict whether a design will work, which can be difficult enough if a weapon is designed to operate near what weapons designers call "the cliff"—the point at which a self-sustaining chain reaction fizzles. Judgment is also demonstrated in a more refined ability to predict the exact yield of a particular design and whether small changes in design or in fabrication materials will affect the yield of the weapon.

Donald MacKenzie and Graham Spinardi, in a brilliant and much-noted article on the possibility of uninventing nuclear weapons, speak of judgment as follows:

Judgment is the feel that experienced weapons designers have for what will work and what won't, for which aspects of the codes can be trusted and which can't, for the impact on the performance of a weapon of a host of contingencies, such as ambient temperature, aging of the weapon, and the vagaries of production processes. . . . According to our interviewees, the judgment goes beyond the explicit knowledge that is embodied in words, diagrams, equations, or computer programs. It rests upon knowledge that has not been, and perhaps could not be, codified. It is built up gradually, over the years, in constant engagement with theory, the codes, the practicalities of production, and the results of testing. Knowing what approximations to make when writing a code requires judgment, and some crucial phenomena simply cannot be expressed fully in the codes.⁶⁴

The scientist quoted at length above believed that mature judgment could only be achieved by working on at least 15 nuclear tests. Another weapons scientist I asked about this, himself in the process of apprenticeship at the time, believed it would take 15 years. Donald MacKenzie and Graham Spinardi were told that it takes "five years to become useful" and ten years "to really train" a weapons designer.⁶⁵ The Los Alamos designer Jas Mercer-Smith told a journalist: "After five years you can do work without hurting yourself. After four or five shots a designer knows how to do his job. At fifty million a shot, that's a quarter of a billion dollars in training. . . . In the design group's apprenticeship-journeyman-master system, the hierarchy is based on years of success-

ful testing. After eight years, I'd call myself a senior journeyman, or maybe a junior master. You don't get paid more or get a better office because you have brought off a test. You get the respect of your peers."⁶⁶

A respected weapons designer's judgment was based on the patient accumulation through experience of tacit knowledge.⁶⁷ Tacit knowledge has been variously defined. Harry Collins, writing about the difficulties experienced by laser physicists trying to make their lasers work on the basis of formal descriptions and instructions alone, says "building up tacit understandings is not like learning items of information, but is more like learning a language or a skill."⁶⁸ Philippe Baumard likens tacit knowledge to the master chess player's instinct that enables him or her to know which moves to explore and describes this kind of knowledge as "something that we know but cannot express."⁶⁹ Similarly, Kathryn Henderson defines tacit knowledge as "knowledge that is not verbalized, sometimes because it is taken for granted but often because it is not verbalizable." This would include, for example, "a carpenter's knowledge of how to choose the appropriate nail for a particular kind of wood or the way humans normally recognize a face."⁷⁰ In their article on tacit knowledge and nuclear weapons design Donald MacKenzie and Graham Spinardi define tacit knowledge as "knowledge that has not been (and perhaps cannot be) formulated explicitly and, therefore, cannot effectively be stored or transferred entirely by impersonal means. Motor skills supply a set of paradigmatic examples of tacit knowledge in everyday life. Most of us, for example, know perfectly well how to ride a bicycle yet would find it impossible to put into words how we do so. There are (to our knowledge) no textbooks of bicycle riding, and when children are taught to ride, they are not given long lists of verbal or written instructions."⁷¹

Throughout the Cold War apprenticeship in the art of nuclear testing was the principal means through which this kind of tacit knowledge was transmitted and cultivated. In the period after the end of the Cold War, as nuclear testing slowed and then disappeared entirely, weapons designers and managers at the weapons laboratories have come to fear that, in Laura McNamara's words, "fifty years' worth of weapons-related knowledge might simply evaporate as experienced Cold Warriors retire from the laboratory."⁷²

Virtual Life: Pedagogy after Nuclear Testing

Immediately after the end of the Cold War, the pace of nuclear testing slowed and only tests for safety improvements to existing weapons were approved. Then, in September 1992, President George H. W. Bush signed into law a moratorium on nuclear testing

that Senate supporters of a test ban had shrewdly attached to a bill funding the superconducting supercollider in Bush's home state of Texas—a state he believed would be crucial in that year's presidential election. At the time of writing the United States has conducted no nuclear test since that date, and it has signed but not ratified the Comprehensive Test Ban Treaty of 1996, negotiated by the Clinton Administration.

As I hope the previous section will have conveyed, the test ban ended a way of life at the weapons laboratories. Weapons designers came to understand nuclear weapons physics and engineering, trained the next generation of scientists, and made their reputations by conducting nuclear tests. In Laura McNamara's words: "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."⁷³ Without nuclear tests the laboratories as organizations were in a very real sense adrift—unable to validate new weapons designs, unsure how to assure the continued reliability of old designs, and unclear how to train and test new weapons scientists. In these circumstances, morale plummeted at both weapons laboratories and many older scientists took early retirement. An early retirement drive in 1993, the third in four years, pushed out 9 percent of the staff at Livermore and 11 percent at Los Alamos. The total number of designers at Livermore and Los Alamos fell by about 50 percent in the decade after the end of the Cold War—a trend that left designers feeling that theirs was indeed a dying art.⁷⁴

By the mid 1990s, a group of managers from both weapons laboratories, working together with senior officials from the Department of Energy and select members of Congress and their staffers, had devised a plan to replace nuclear testing at the Nevada Test Site, at least in the short to medium term, with a program of simulated testing called Science-Based Stockpile Stewardship distributed across the laboratories and other facilities in the nuclear complex and funded at almost \$6 billion per year by 2002.⁷⁵ The central components of this program are the following:

Subcritical tests Underground tests at the Nevada Nuclear Test Site that use conventional explosives to shock small quantities of plutonium. The tests are called "subcritical" because the plutonium does not undergo a run-away chain reaction. These tests help scientists to refine their equations of state for plutonium and, in particular, understand its changing behavior as it ages.

Dual-Axis Radiographic Hydrotest Facility (DARHT) A Los Alamos facility to enable scientists to take x-ray snapshots of a nuclear primary made from a non-fissile surrogate

for plutonium as it implodes, disintegrating at almost 6,000 miles per hour. These pictures enable scientists to peer inside the implosion of a pit to check its speed and evenness, to see how different material surfaces interact, and to trace the propagation of shock waves through the pit.

ATLAS A Los Alamos facility that discharges electricity stored in huge capacitor banks to create enormous magnetic fields that briefly create within the laboratory high energy-density regimes like those found inside stars. This helps scientists simulate the conditions inside a nuclear weapon as the imploding primary ignites fusion in the secondary.

National Ignition Facility (NIF) A 192-arm laser being built at the Livermore Laboratory. The laser will create within the laboratory temperatures and pressures higher than those in the sun by using the laser energy to fuse pellets of tritium and deuterium, thus enabling scientists to refine their modeling of fusion processes within hydrogen bombs.

Accelerated Strategic Computing Initiative (ASCI) A lavishly funded program to help the laboratories replace their old supercomputers with massively parallel computing systems with such power and speed that “all of the calculations used to develop the U.S. nuclear stockpile from the beginning could be completed in less than two minutes.”⁷⁶ Codes run on the new computer systems will, finally, be three-dimensional, and will, in theory at least, enable scientists to integrate results from subcritical tests and experiments on NIF and DARHT with old nuclear test data.

There are three principal rationales for the Science-Based Stockpile Stewardship Program: to help diagnose problems in the aging nuclear arsenal and assure the efficacy of repairs; to refine the computer codes that model the behavior of nuclear weapons; and, finally, to provide a new means of nuclear pedagogy. Regarding the first rationale—diagnosing and fixing problems caused by aging—experiments at different facilities can shed light on performance changes as the materials in weapons age and are replaced. Subcritical tests, for example, can show whether the behavior of plutonium changes as helium bubbles form within the aging metal. Experiments on the DARHT can show whether the compressive behavior of conventional high explosive lenses changes as they age and whether substitution of one material in the lenses with another alters their ability to produce perfectly symmetrical implosion waves.

Second, in regard to the supercomputer codes, the plan is to transform the old two-dimensional codes used for weapons design in the 1980s into three-dimensional codes continually refined with data from subcritical tests and shots on DARHT, ATLAS, and the NIF that improve the modeling of particular phenomena within the explosion of a nuclear weapon. As one material scientist working on plutonium put it to me, “all my recent work has been an analysis of data published twenty to thirty years ago, applying correct chemical understanding and mechanisms, and modeling them on modern computers, showing, in fact, that we understand now what happens to chemical bases and then extending that.”⁷⁷ The ultimate dream, as described by science journalist Dan Stober, is of a virtual reality Cave that allows a weapons designer to stand inside an exploding thermonuclear weapon and watch the explosion “in three dimensions on the walls, floors, and ceiling,” with the ability to “zoom in on a piece of plutonium right down to the microscopic level. . . . A physicist might stand inside the cube wearing special 3-D glasses while images of his simulated weapon are projected on the walls of the cube. The view would change as the physicist turns his head or ‘flies’ with an electronic wand.”⁷⁸

The refinement of the codes (the largest of which are a million lines long)⁷⁹ and the development of much more powerful computer systems on which to run the codes will shift the balance of power between weapons designer and code, relocating some of the expert judgment thought to reside in the human designer’s tacit knowledge to the formalized protocols of the computer codes. As with so many other expert systems, from airplane autopilots to accounting software, the result will be a partial deskilling of the human expert—here the weapons designer—and a fetishization of the authority of the code. If in the old days nuclear tests (whether they produced visible mushroom clouds or inky flickers on seismographs) were the embodiment of the efficacy and power of the U.S. nuclear deterrent, today it is the codes that, in a very real sense, signify the power and reliability of the U.S. nuclear deterrent. (It is only in this context, for example, that we can understand the extraordinary outburst of national hysteria in the United States over allegations in the late 1990s that the Los Alamos code developer Wen Ho Lee had transferred computer codes to China—as if he had somehow emailed the U.S. nuclear arsenal to a foreign power.)

There is, however, a limit to the transfer of a weapons designer’s predictive ability to the codes, a limit that returns us to the issue of tacit knowledge and judgment. As one senior manager told Donald MacKenzie and Graham Spinardi, “the codes only explain 95% of physical phenomena at best, sometimes only 50 percent.”⁸⁰ The codes are thought to be more reliable in predicting the behavior of “secondaries” than of “primaries” or “boosting” within a nuclear weapon, and they have limitations because of

“fudge factors” or “knobs” deliberately introduced into the codes as a way of bridging the divide between the weapons scientists’ imperfect theories and the empirical realities of nuclear testing. Dan Stober and Ian Hoffman explain it as follows: “In the desert a bomb might blow up with 20 percent less energy than the code had predicted. If the code writers or weapons designers understood the reason for the mistake, they would change the basic physics of the code. If not, they simply added a ‘fudge factor’ to the code, to make the computer prediction match the ‘ground truth’ of the actual nuclear explosion. The code jocks adjusted these ‘knobs’ in the software until the answers came out right, even if the underlying physics was not understood completely.”⁸¹ One designer told Laura McNamara: “You don’t know exactly what’s going on, but you’ve got a hunch—if you tweak a knob, the model fits the data better. You can’t explain exactly why it fits—it’s intuition.”⁸²

The inherent limitations of the codes underline the importance of the third rationale for stockpile stewardship: as a replacement for nuclear testing in the cultivation of judgment in a new generation of weapons scientists. Testifying to the Senate in 1996, Paul Robinson, director of Sandia, said: “Many of the systems in the stockpile will require replacement at about the same time at some point in the first half of the next century. The engineers and scientists who will do that work are probably entering kindergarten this year. No old-timers will be around in 2025 who have had actual experience in designing a warhead. We must find ways to qualify these people.”⁸³ These new designers are being qualified through an adapted version of the old apprenticeship relationship and through newly formalized ways of archiving and transmitting nuclear weapons design knowledge. There is now a formal class in nuclear weapons design at Los Alamos called TITANS (Theoretical Institute for Thermonuclear and Nuclear Studies), described in the *Los Alamos Insider* as “a formalized training curriculum in nuclear weapons design and analysis”⁸⁴ in which experienced nuclear weapons designers take it in turns to lecture weapons designers in training. Meanwhile the nuclear laboratories have established a peer-reviewed classified journal to which young and old scientists are encouraged to submit articles. The laboratories are also asking older designers to archive their knowledge before they retire and are videotaping interviews with older designers in which they discuss everything from neutron cross-sections to the social customs at the Nevada Test Site.⁸⁵ Insofar as nuclear weapons design instruction is now more formalized, with knowledge transmission partially dislocated from apprenticeship relationships to classroom instruction and the consumption of archived articles and videos, pedagogy at the nuclear weapons laboratories increasingly resembles that practiced in the world of universities, with its heavily routinized conventions for knowledge consumption and production. At the same time, the old emphasis on

intense dyadic apprenticeship relationships endures, albeit in a form adapted to a world without testing. A scientist at Los Alamos explained to me how this system now works:

An experienced designer would show the person he's mentoring where you start. That is, you've got some specs from DoD [the Department of Defense]. They've got a mission that needs an 80-kiloton warhead that can't weigh more than 23 kilograms or whatever. This could be an actual mission from real life or a hypothetical one. They could take a new guy through the design process that is already in place for one of the weapons in the stockpile, show them what the mission was and how they do the scoping study to see if the mission was possible; and if the scoping study showed that it wasn't possible, how they went back to the Department of Defense and said "Look, you're going to give us 25 kilograms, because we can't do it in 23. . . ." Now you've got the rough outline and you've got to fill in the details and take the guy through it. It takes a tremendous amount of time, basically as much time as he probably spent designing it in the first place to take the guy through. And the guy probably would have to go through a couple of times before he would be comfortable doing the same exercise on his own, without the mentor telling him "No, that's a blind alley. Don't do that, do this." It wouldn't be full time because the designer's got other things to do, but he'll be spending a significant fraction of his time for years or more mentoring a new person.⁸⁶

One older scientist at Los Alamos worried that this kind of mentorship experience could not replicate the intensity of the old mentorship relations structured around nuclear testing. "There is nothing that has that level of intensity and excitement, nothing that you pour your heart and soul into. In terms of the intensity of a nuclear test, I'd literally worked days straight without sleep designing a lot of my devices and they had to pull the design out from under my hands in time to go manufacture it for the test. And literally you'd go design it over a period of time and you were lucky if you got six hours of sleep on any night for that entire period. But you had a sense of accomplishment when you were done."⁸⁷ New designers now are encouraged to propose sub-critical experiments or shots on DARHT, ATLAS, or NIF that will enable them to make predictions and get experimental feedback. These experiments will not only enable them to develop judgment, but will also enable laboratory managers to determine which of the new designers have the best judgment when it comes to deciding in the future who can certify the continuing reliability of the U.S. nuclear stockpile. A senior manager at Livermore put it this way:

At a fundamental level the way you evaluate people is the same; that is, you set up situations where people can succeed or fail. In the past, those were things like a nuclear test. You go out and do a nuclear test and make some predictions. If it works, you're a hero; if it doesn't work, your career ends. . . . The same thing is occurring now but, instead of judging on the basis of nuclear tests, you're having to judge on the basis of other kinds of experiments. Ultimately you can only succeed or fail at an experiment. . . . And so, with the National Ignition Facility, for instance, you will be able to judge whether or not the people doing the experiments are good by virtue of whether they succeed.⁸⁸

A number of older weapons designers have told me that the power of the U.S. nuclear deterrent resides not only in the thousands of nuclear weapons that have been built and stockpiled, ready to be used against an adversary with only a few moments' notice, but also in the perceived expert judgment of U.S. weapons designers who must guarantee that these weapons really work as advertised.⁸⁹ In the days of atmospheric and underground nuclear testing, the judgment of American weapons designers was displayed, both to the community of weapons scientists and to foreign adversaries, in elegantly designed and successfully executed nuclear tests. An intricate and intense program of apprenticeship structured around the preparation of nuclear tests enabled older designers to transmit their tacit knowledge accumulated over a lifetime—the elusive quality called “judgment”—to the younger scientists who would replace them. Now nuclear weapons scientists, young and old, worry whether it is really possible to cultivate this kind of judgment in the world of simulations, and wonder how the acquisition of this judgment might be advertised to foreign adversaries to signal that the American nuclear arsenal is still robust. Formulating the notion of “capability-based deterrence,” Steve Younger, until recently Associate Director for Nuclear Weapons Design at Los Alamos, had suggested that the traditionally reclusive Los Alamos scientists, serving as human embodiments of the national nuclear deterrent, should be more active on the international conference and publication circuit, publicly displaying to other national expert communities their excellence in unclassified fields of knowledge related to weapons science—a suggestion he stopped making after the Wen Ho Lee case exacerbated public concerns about loose stewardship of classified information at Los Alamos.⁹⁰

Other designers worry that their managers sold them out by consenting to the replacement of nuclear testing with Science-Based Stockpile Stewardship and that the gulf between simulated and real testing is too wide for pedagogy and the cultivation of judgment in a new generation of designers to be truly possible.⁹¹ Bob Barker, a former designer and Associate Director at Livermore, testified as follows to Congress in 1997:

As a nuclear weapons designer I learned the limitations of simulations and the humility that comes with the failure of a nuclear test. Computer calculations, regardless of how good or fast the computer is, are only as good as the data and models you give them and the knowledge and experience of the person doing the calculations. Even today no computers are big enough or fast enough to simulate all that goes on when a nuclear weapon explodes. The true knowledge of and experience with the limitations of calculations came from understanding the differences between calculations and experiments, including nuclear tests.⁹²

Another older designer told me: “Good judgment comes from experience and experience comes from bad judgment. Now we’re going to have people out there with no experience.”⁹³ Another older designer told me that novice weapons designers relying

on their codes reminded him of drunk drivers: the drunker they were, the better they thought they were driving.

Conclusion

The world of TITANS classes, billion-dollar lasers, and teraflop computer simulations in which nuclear weapons designers can physically stand as they seek to inherit half a century of nuclear weapons science is far removed from the 1940s and 1950s world of men in their twenties and thirties in charge of design divisions trying to understand for the first time the fundamental processes of atomic and hydrogen bombs. At the outset of this essay I referred to a “pedagogy of diminishing returns” at the weapons laboratories. Paradoxically, the more elaborate, drawn-out, and formalized nuclear weapons pedagogy has become over the years, and the more extensive the laboratories’ understanding of nuclear weapons science has become, the more problematic pedagogy has become. If a first generation of young men making it up as they went along created great breakthroughs in nuclear weapons design, their successors in the era of underground testing took much longer to qualify themselves as experts, spent a lifetime acquiring a much more extensive body of knowledge and yet, in a very real sense, achieved much less. And now today we have a new generation of aspiring designers looking into the abyss between simulation and experiment as their mentors wonder whether it is possible for the community of designers to simply jog in place or whether, despite the expenditure of unprecedented sums of money on nuclear pedagogy, their art and science is in the process of being irretrievably eroded.

There is an obvious sense in which this involutory momentum is the result of political developments beyond the weapons scientists’ control. The end of the Cold War, followed by the international prohibition of nuclear tests, deprived the weapons laboratories of their most important experimental practice and their principal way of training new weapons designers: nuclear testing. Some weapons designers have argued that, if only nuclear testing were to resume, then a renaissance of nuclear weapons science would ensue. The reality seems to me, however, to be more complicated. After the weapons scientists’ failure in the 1980s to perfect third-generation nuclear weapons, weapons scientists largely stopped talking about major new breakthroughs in their art. In the 1990s those who argued for further nuclear testing did not argue that major advances in nuclear weapons science would be possible. (Indeed it was the turn to simulations forced by the end of nuclear testing that produced the boldest innovations in the practice, and the rhetoric, of nuclear weapons science.) Instead those who opposed the end of testing argued that continued testing was necessary to ensure the reliability

of old weapons as they decayed or, if they did refer to new weapons, made modest claims: they suggested that it would be possible to develop a bunker-busting mini-nuke or a sort of Maytag nuclear weapon—what designers call a “wooden bomb”—optimized to age well in the absence of testing in the future. In other words, even without the nuclear test ban, nuclear weapons science had by the end of the Cold War hit a sort of wall. It is not that weapons scientists had no ideas for improving their weapons—the reverse was true—but the refinements had become so incremental and the ratio of effort and expense to scientific advancement in the performance of nuclear testing had shifted so far that nuclear weapons science was becoming like, say, Gothic architecture at the end of the nineteenth century: an increasingly repetitious and involuted practice that was losing its energizing edge and its appeal to the best and the brightest. The Oppenheimers and Tellers had given way to smart career physicists who saw themselves largely as custodians of a settled body of knowledge. The end of nuclear testing has disrupted the established and time-tested pedagogical practices for the transmission of that settled knowledge to another generation of the nuclear priesthood but it may be that, by shaking things up, the virtual turn offers this community its last best hope of intellectual revitalization.

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Notes

1. The concept of “involution” is borrowed from Clifford Geertz, *Agricultural Involution: The Process of Ecological Change in Indonesia* (University of California Press, 1963).
2. Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (Sage, 1979); Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Harvard University Press, 1987); Bruno Latour, “Give me a laboratory and I will raise the world,” in *Science Observed*, ed. Karin Knorr-Cetina and Michael Mulkay (Sage, 1983), 141–170; Thomas Kuhn, *The Structure of Scientific Revolutions* (University of Chicago Press, 1962).

3. The B53—an older model of hydrogen bomb with a very high explosive yield.
4. The B61—a newer, lower-yield hydrogen bomb designed at Los Alamos and recently modified to give it an earth-penetrating capability. The new version is the B61 Mod 11.
5. This song, named for the veteran Livermore designer Dan Patterson, is called the “St. Patterson Test Site Blu’s.” My thanks to the Livermore A Division Men’s Chorus for providing the lyrics.
6. This story is told in Richard Rhodes, *The Making of the Atomic Bomb* (Simon and Schuster, 1988); Gregg Herken, *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller* (Henry Holt, 2002); Lillian Hoddeson et al., *Critical Assembly: A Technical History of Los Alamos During the Oppenheimer Years, 1943–1945* (Cambridge University Press, 1993). More eccentric accounts are by Robert Jungk, *Brighter Than a Thousand Sons: A Personal History of the Atomic Scientists* (Penguin, 1960), and Brian Easlea, *Fathering the Unthinkable: Masculinity, Scientists and the Arms Race* (Pluto, 1983). See also Jon Else’s fine documentary film *The Day after Trinity* (KTEH-TV, 1980).
7. See Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (Simon and Schuster, 1995), and, for an account of the political struggles around the scientific breakthrough, Herbert York, *The Advisers: Oppenheimer, Teller, and the Superbomb* (Freeman, 1976).
8. See Harvey Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Harvard University Press, 1972).
9. Dan Stober and Ian Hoffman, *A Convenient Spy: Wen Ho Lee and the Politics of Nuclear Espionage* (Simon and Schuster, 2001), 36–44.
10. For works that feature several of these figures, see Herken, *Brotherhood of the Bomb*, Else, *The Day after Trinity*, and S. S. Schweber, *In the Shadow of the Bomb: Oppenheimer, Bethe and the Moral Responsibility of the Scientist* (Princeton University Press, 2000). On Oppenheimer, see J. Robert Oppenheimer et al. (eds.), *Robert Oppenheimer: Letters and Recollections* (Stanford University Press, 1995); Jeremy Bernstein, *Oppenheimer: Portrait of an Enigma* (Ivan R. Dee, 2004); Peter Goodchild, *J. Robert Oppenheimer: Shatterer of Worlds* (Houghton Mifflin, 1981). On Teller, see Teller’s own autobiographies: Edward Teller, *The Legacy of Hiroshima* (Doubleday, 1962) and (with Judith Shoolery) *Memoirs: A Twentieth-Century Journey in Science and Politics* (Perseus, 2002)—as well as Stanley Blumberg and Louis Panos, *Edward Teller: Giant of the Golden Age of Physics* (Scribner, 1990); Broad, *Teller’s War*; Peter Goodchild, *Edward Teller: The Real Dr. Strangelove* (Harvard University Press, 2004). Ulam’s account of his own life is given in Stanislaw Ulam, *Adventures of a Mathematician* (University of California Press, 1991). Von Neumann’s life is evoked by Norman McCrae, *John von Neumann* (Pantheon, 1992). For York’s own account of his life and work, see Herb York, *Making Weapons, Talking Peace: A Physicist’s Odyssey from Hiroshima to Geneva* (Basic Books, 1987). Ted Taylor’s story is told by John McPhee, *The Curve of Binding Energy* (Ballantine, 1974) and George Dyson, *Project Orion: The True Story of the Atomic Spaceship* (Henry Holt, 2002).
11. On the x-ray laser debacle at Livermore, the definitive source is the reporting of William Broad, a science journalist for the *New York Times*. His first book on the subject has been criticized as too credulous: see William Broad, *Star Warriors: A Penetrating Look Into the Lives of the Young*

Scientists Behind Our Space Age Weaponry (Simon and Schuster, 1985). His second book, more than adequately compensating the blind spots of the first, is the definitive history of the rise and fall of this program at Livermore and in Washington: William Broad, *Teller's War: The Top Secret Story Behind the Star Wars Deception* (Simon and Schuster, 1992). See also William Broad, "Crown jewel of 'star wars' has lost its luster," *New York Times*, February 13, 1990; Deborah Blum, "Weird science: Livermore's x-ray laser flap," *Bulletin of the Atomic Scientists* 44 (1988), no. 6: 7–13; Robert Scheer, "The man who blew the whistle on Star Wars," *Los Angeles Times Magazine*, July 17, 1988.

12. A partial account of these safety improvements is given in Sidney Drell, "How safe is safe?" *Bulletin of the Atomic Scientists* 47 (1991), no. 3: 35–40; Jeffrey Smith, "America's arsenal of nuclear time bombs," *Washington Post National Weekly Edition*, May 28–June 3, 1990.

13. One scientist at Livermore once told me that he was never allowed to know why one of his colleagues had won the Lawrence Award, the most prestigious award within the DOE weapons complex.

14. Another anthropologist who studies Los Alamos, Joseph Masco, has written his own account of the political and emotional phenomenology of nuclear testing organized around the same tripartite chronological schema. See Joseph Masco, "Nuclear Technoethnics: Sensory Politics from Trinity to the Virtual Bomb in Los Alamos," *American Ethnologist* 31 (2004), no. 3: 1–25.

15. See Hugh Gusterson, *Nuclear Rites: A Weapons Laboratory at the End of the Cold War* (University of California Press, 1996); idem, *People of the Bomb: Portraits of America's Nuclear Complex* (University of Minnesota Press, 2004); idem, "Los Alamos: A Summer under Siege," *Bulletin of Atomic Scientists* 55 (1999), June: 36–41; idem, "Exploding anthropology's canon in the world of the bomb: Ethnographic writing on militarism," *Journal of Contemporary Ethnography* 22 (1993): 59–79.

16. On the experience at Los Alamos of what anthropologists would call "communitas"—a sense of community unobstructed by roles and hierarchies—see Jon Else, *The Day after Trinity*.

17. Rhodes, *The Making of the Atomic Bomb*, 454–455.

18. *Ibid.*, 460–464.

19. The ages of the Manhattan Project scientists are taken from *ibid.*, 118, 188, 112, and 460, and from Dyson, *Project Orion*, 29. Ulam's quote is from Ulam, *Adventures of a Mathematician*, 156 (quoted in Dyson, *ibid.*, 22).

20. Sybil Francis, *Warhead Politics: Livermore and the Competitive System of Nuclear Weapon Design* (PhD dissertation, MIT, 1995), 72. By 1955 the Livermore Laboratory had only 300 physicists on its payroll out of a total staff of 1,633 (*ibid.*, 105).

21. Herbert York, *Making Weapons*, 74. See pp. 72–73 for a discussion of the ages of his managers at the Laboratory when it first opened its doors. This initial cohort at Livermore was to play a vital role in shaping the laboratory as it evolved, since the six directors who led the laboratory through its first 35 years were all hired that first year. They are Herbert York, John Foster, Edward Teller, Harold Brown, Michael May, and Roger Batzel.

22. Francis, *Warhead Politics*, 63.
23. York, *Making Weapons*, 67.
24. *Ibid.*, 74–75.
25. Until 1957 even the town of Los Alamos, not just the laboratory, was closed to the public (Jo Ann Shroyer, *Secret Mesa: Inside the Los Alamos National Laboratory* (Wiley, 1998), 7–8).
26. Interview with retired Livermore weapons designer, February 11, 1989.
27. Quoted in Dyson, *Project Orion*, 51.
28. Darol Froman to Alfred D. Starbird, May 4, 1959, quoted in Francis, *Warhead Politics*, 146.
29. In gun-assembly weapons two pieces of fissile material, machined so that they can mate with one another, are fired into one another at high speed within a cylinder to create a critical mass. In implosion devices a spherical (or later ovoid) piece of plutonium, roughly the size of a grapefruit, is symmetrically compressed by surrounding high explosives into a smaller, denser sphere that achieves critical mass.
30. For more detailed accounts of these advances in nuclear weapons design, see Rhodes, *The Making of the Atomic Bomb*; Rhodes, *Dark Sun*; Thomas Cochran, William Arkin and Milton Hoenig, *The Nuclear Weapons Databook: U.S. Nuclear Forces and Capabilities* (Ballinger, 1984); Sam Cohen, *Shame: Confessions of the Father of the Neutron Bomb* (Xlibris 2000); Necah Stewart Furman, *Sandia National Laboratories: The Postwar Decade* (University of New Mexico Press, 1990); Barton Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947–1974* (University of California Press, 1994); Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (Orion, 1988); Donald MacKenzie and Graham Spinardi, “Tacit knowledge, weapons design, and the uninvention of nuclear weapons,” *American Journal of Sociology* 101 (1995): 44–99; McPhee, *The Curve of Binding Energy*; Howard Morland, “The H-bomb secret: To know how is to ask why,” *The Progressive* 43 (1979), November: 14–45; Robert Serber, *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb*, ed. Richard Rhodes (University of California Press, 1992); Edward Teller, *The Legacy of Hiroshima* (Doubleday, 1962). See Dyson, *Project Orion*, for a detailed history of the attempt to create a spaceship, powered by hundreds of nuclear explosions, for inter-planetary exploration and for nuclear warfighting. On the program to use nuclear explosions for civilian excavation, mining and drilling—including a plan to excavate the Panama Canal with nuclear weapons—see Dan O’Neill, *The Firecracker Boys* (St. Martin’s Griffin, 1994) and the documentary film by Gary Marcuse, *Nuclear Dynamite* (Bullfrog Films, 2001).
31. Francis, *Warhead Politics*, 143–144.
32. Donald MacKenzie and Graham Spinardi (“Tacit knowledge,” 59) make a similar point, though with a slightly different periodization, saying that “by the 1980s designing nuclear weapons had lost much of its flavor of virtuoso innovation and had become a more routine task: one, indeed, that some in the laboratories feel to have become bureaucratized, unchallenging, even ‘dull.’”
33. Francis, *Warhead Politics*, 89.

34. Ibid., 64–65.
35. Stober and Hoffman, *A Convenient Spy*, 38.
36. Francis, *Warhead Politics*, 67.
37. Ibid., 115.
38. Ibid., chapter 4.
39. Ibid., chapter 7. Francis quotes marginalia scribbled by a Los Alamos scientist on the Livermore proposal: “material in newspapers like this generally carries in fine print at the bottom of the page (Advt).” (ibid., 123.)
40. Ibid., 140. The test was code-named Rainier. Los Alamos was concerned that underground testing would be too expensive and difficult to instrument, and that proof of the possibility of underground testing would make it more likely that the U.S. government would agree to a treaty banning atmospheric testing.
41. Herbert York, “The origins of the Lawrence Livermore Laboratory,” *Bulletin of the Atomic Scientists* 31 (1975), July: 8–14.
42. Francis, *Warhead Politics*, 134; George Miller, Paul Brown and Carol Alonso, Report to Congress on Stockpile Reliability, Weapon Remanufacture, and the Role of Nuclear Testing (Lawrence Livermore National Laboratory, document no. UCRL-53822); Ray Kidder, Maintaining the U.S. Stockpile of Nuclear Weapons During a Low-Threshold or Comprehensive Test Ban (Lawrence Livermore National Laboratory, document no. UCRL-53820). The last round of tests before the 1958 moratorium had established that one of the Livermore Polaris designs was not inherently one-point safe: in other words, an accidental nuclear detonation of the weapon was possible if, for example, it was struck by a bullet. Rather than replace the Livermore primary in the weapon with a Los Alamos primary, Teller, Livermore’s director at the time, elected to use a mechanical safing mechanism.
43. See Broad, *Star Warriors* and *Teller’s War*; Scheer, “The man who blew the whistle on Star Wars.”
44. James Glanz, “Panel faults laser architect for overruns,” *New York Times*, January 11, 2000; idem, “Laser project is delayed and over budget,” *New York Times*, August 19, 2000; “Top U.S. laser expert admits lack of a PhD and resigns,” *New York Times*, August 31, 1999; David Perlman, “Test lab called \$1 billion over budget,” *San Francisco Chronicle*, May 9, 2001. The ostensible reason for the resignation of Mike Campbell, Associate Director for lasers, was the public revelation that he had claimed to have a doctorate from Princeton despite never having finished his PhD thesis, but most observers agreed that the timing of this revelation was tied to the increasingly evident financial and organizational problems of the National Ignition Facility, a project directed by Campbell.
45. Quoted in Dyson, *Project Orion*, 32.
46. Senate Policy Committee, Berkeley Division of the Academic Senate, University of California, The University of California, the Lawrence Livermore National Laboratory, and the Los Alamos National Laboratory (unpublished paper, 1984); Gusterson, *Nuclear Rites*, 25.

47. Interview with retired weapons designer at Lawrence Livermore National Laboratory, July 22, 1990.

48. See note 12. This is not a fanciful concern: there have been incidents in which American planes carrying nuclear weapons have caught fire on the runway, have dropped their weapons accidentally, and have collided with one another in mid-air. In at least one of these accidents, in Greenland, there was a conventional explosion that dispersed plutonium over a wide area. In another, a bomb was lost off the coast of Georgia and has never been found. For more on these accidents, the definitive source is Scott Sagan, *The Limits of Safety: Organizations, Accidents, and Nuclear Weapons* (Princeton University Press, 1993). See also Smith, "America's Arsenal"; Drell, "How safe is Safe?"; Hugh Gusterson, "Nuclear Weapons and the Other in the Western Imagination," *Cultural Anthropology* 14 (1999): 111–143.

49. Because female nuclear weapons designers are comparatively rare at Livermore and Los Alamos, describing a weapons designer as female makes it much easier to identify her and, thus, undermines the convention of anonymity in contemporary ethnography. Consequently I have over the years developed the practice of representing all weapons designers as male, regardless of their actual gender, unless it is important to the analysis to identify them as female. I continue that practice in this essay.

50. Interview with Livermore weapons designer, January 26, 1989.

51. Interview with Los Alamos weapons designer, February 11, 1992.

52. Interview with retired Livermore weapons designer, February 11, 1989.

53. At the Livermore Laboratory, B Division is responsible for the design of "primaries"—fission devices used either in tactical weapons or as triggers to ignite a larger thermonuclear explosion. A Division is responsible for the "secondaries" whose explosive yield is derived from thermonuclear fusion.

54. Interview with Livermore primary designer, January 26, 1989.

55. Interview with Livermore weapons designer, January 26, 1989.

56. Interview with Livermore manager, June 8, 1989.

57. Interview with Livermore weapons designer, January 9, 1988.

58. Interview with Livermore weapons designer, January 26, 1989.

59. Quotation from draft of Laura McNamara, "Ways of Knowing" about Weapons: The Cold War's End at Los Alamos National Laboratory (PhD dissertation, University of New Mexico, 2001), 114–115.

60. *Ibid.*, 145–146.

61. The narrative here makes it sound as if weapons designers go straight from computer calculations and presentations to review committees to \$50 million nuclear tests. As Laura McNamara points out (131–132), the reality is more complicated: "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*, a high explosive experiment that would provide 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. . . . 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?"

62. The person in charge of designing a device for a test was referred to as the Design Physicist (DP), and weapons designers traditionally built their reputations by being DP for devices they had designed and seeing their tests go well. In the idealized narrative above, our hypothetical designer has an exciting idea and is therefore put in charge as DP. In practice, by the 1980s, shots were designed by teams and the contributions of individual designers were harder to disentangle. After one conversation with a Los Alamos secondary designer I made the following entry in my notes: "They often make younger physicists DP now to give them experience. In the old days there were more tests, and people got more chance to truly do their own tests and be DPs of projects they'd initiated and controlled. These days, with a different design culture and less tests, the tests tend to be based more on teamwork and the decision who gets to be DP is based more on social and political factors: in part, they try to rotate the privilege and the responsibility. He [the designer] sees war-head design becoming more like plane or rocket design, where the product is so much a team effort that you're not sure who did what." On the increasing dissolution of the individual author or inventor in physics, see Peter Galison, "The Collective Author," in Mario Biagioli and Peter Galison (eds.), *Scientific Authorship: Credit and Intellectual Property in Science* (Routledge, 2003), 325–355.

63. Interview with Livermore manager, June 8, 1989.

64. MacKenzie and Spinardi, "Tacit knowledge," 62.

65. *Ibid.*

66. Janet Bailey, *The Good Servant: Making Peace with the Bomb at Los Alamos* (Simon and Schuster, 1995), 38, 81. The discussion here focuses on the cultivation of judgment among the elite of weapons designers at the national laboratories. Laura McNamara's work, however, makes clear that the success of nuclear tests in the Cold War also relied heavily on the judgment and tacit knowledge of experienced engineers and technicians—personnel whose contributions have received less recognition.

67. On tacit knowledge, see Philippe Baumard, *Tacit Knowledge in Organizations* (Sage, 1999); H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (Sage, 1985); H. M. Collins, "The TEA set: Tacit knowledge and scientific networks," *Social Studies of Science* 4 (1974): 165–187; Kathryn Henderson, *On Line and On Paper: Visual Representations, Visual Culture, and Computer Graphics in Design Engineering* (MIT Press, 1999); M. Polanyi, *The Tacit Dimension* (Doubleday, 1966); Ludwig Wittgenstein, *Philosophical Investigations* (Blackwell, 1953).

68. Collins, "The TEA set," 168.
69. Baumard, *Tacit Knowledge in Organizations*, 2.
70. Henderson, *On Line*, 6.
71. MacKenzie and Spinardi, "Tacit knowledge," 45.
72. McNamara, "Ways of Knowing," 112
73. *Ibid.*, chapter 114. See also Gusterson, *Nuclear Rites*, chapter 6.
74. Jonathan Medalia, Nuclear Dilemmas: Nonproliferation Treaty, Comprehensive Test Ban, and Stockpile Stewardship (CRS Report for Congress, Order Code 94-1007F 1994); interview with Livermore Associate Director, March 28, 1997. Donald MacKenzie and Graham Spinardi ("Tacit knowledge," 62–63) put the number of "good designers" in the U.S. at "about 50" at the end of the Cold War, while observing that one Livermore manager put the number even lower, at "about 40."
75. For an overview of this program, see Drell et al., Science-Based Stockpile Stewardship, JSR-94-345 (Mitre, 1994).
76. Randy Christensen, "Computer simulations in support of national security," *Science and Technology Review*, April 1998, available at www.llnl.gov/str (accessed May 21, 2004).
77. Interview with Los Alamos materials scientist, June 22, 1998.
78. Dan Stober, "Byting the big bang," *San Jose Mercury News*, August 11, 1998.
79. MacKenzie and Spinardi, "Tacit knowledge," 59.
80. The manager in question is Art Hudgins, quoted in MacKenzie and Spinardi, "Tacit knowledge," 60.
81. Stober and Hoffman, *A Convenient Spy*, 36.
82. McNamara, "Ways of Knowing," chapter 3, 16.
83. Michael Moore, "Wanted: Wee weaponeers," *Bulletin of the Atomic Scientists* 52 (1996), April: 8–9, quotation on 9.
84. Quoted in Matthew McKinzie, Tom Cochran, and Chris Paine, *Explosive Alliances: Nuclear Weapons Simulation Research at American Universities* (Natural Resources Defense Council, 1998), 76. McKinzie et al. quote *The Los Alamos Insider* as saying that "the students' enthusiasm is a real source of inspiration. Frequently, after class, there are a half-dozen students that remain in the classroom to ask follow-up questions, and one often hears them discussing homework problems in the hallways."
85. See McNamara, "Ways of Knowing."
86. Interview with Los Alamos computational physicist, June 23, 1998.
87. Interview with Los Alamos weapons designer, January 16, 1999.

88. Interview with Livermore manager, March 28, 1997.

89. One weapons designer, being perhaps more blasé than his colleagues would approve, told me about deterrence: “. . . in the end all you’re saying is that we have a lot of really smart guys who worked really hard on these things” (conversation with Sandia weapons scientist, October 28, 1997).

90. Keith Easthouse, “Battle lines,” *The New Mexican*, May 5, 1998.

91. Wen Ho Lee, commenting that “the science of nuclear weapons hasn’t progressed much” since the end of the Cold War, has said of Science-Based Stockpile Stewardship that it is “like eating leftovers for dinner, [but] it’s better than nothing.” Quoted in Walter Pincus, “Los Alamos scientist criticizes FBI in book: Lee calls copied tapes ‘crown junk,’” *Washington Post*, January 16, 2002.

92. Robert Barker, Prepared testimony before Senate Governmental Affairs Committee, Subcommittee on International Security, Proliferation, and Federal Services, October 27, 1997.

93. Interview with Los Alamos weapons designer, January 16, 1999.