Tacit Knowledge, Weapons Design, and the Uninvention of Nuclear Weapons[1]

Donald MacKenzie and Graham Spinardi
University of Edinburgh

Tacit knowledge, embodied in people rather than words, equations, or diagrams, plays a vital role in science. The historical record of the development and spread of nuclear weapons and the recollections of their designers suggest that tacit knowledge is also crucial to nuclear weapons development. Therefore, if design ceases, and if there is no new generation of designers to whom that tacit knowledge can be passed, then in an important (though qualified) sense nuclear weapons will have been uninvented. Their renewed development would thus have some of the characteristics of reinvention rather than simply copying. In addition, knowledge may be lost not only as a result of complete disarmament, but also as a consequence of likely measures such as a nuclear test ban.

INTRODUCTION

Over the last three decades, an alternative account of scientific knowledge has gradually emerged to rival the traditional view. In the latter, scientific knowledge and science-based technology are universal, independent of context, impersonal, public, and cumulative; the practice of science is (or ought to be) a matter of following the rules of scientific method. The alternative account emphasizes instead the local, situated, person-specific, private, and noncumulative aspects of scientific knowledge. Scientific practice is not the following of set rules, but "particular

[1] This research was supported by the U.K. Economic and Social Research Council, mainly under the Science Policy Support Group Initiative on Science Policy Research on Defence Science and Technology (grant Y307253006). We would particularly like to acknowledge the help given to our research by David Holloway, William Walker, and, above all, our interviewees from Los Alamos, Livermore, and Aldermaston. Helpful comments on the first draft of this paper were received from Harry Collins, Jeff Hughes, John Krige, Dominique Pestre, Annette Schaper, William Walker, Peter Zimmerman, and three *AJS* referees. Direct correspondence to Donald MacKenzie, Department of Sociology, University of Edinburgh, 18 Buccleuch Place, Edinburgh EH8 9LN, Scotland.

0002-9602/96/10101-0002$01.50

courses of action with materials to hand" (Lynch 1985, p. 5): action that is fully understandable only in its local context and materials that are inescapably heterogeneous, including human as well as nonhuman elements (see, e.g., Knorr-Cetina 1992; Pickering 1993).[2] Universality and context independence, in this new view, are not to be taken as given but must be analyzed as precarious achievements—for example as the result of the successful construction of wide-ranging networks linking human and nonhuman actors (Latour 1986, 1987).

This article focuses on a single thread from the extensive, tangled, and sometimes contradictory web of arguments that constitute this alternative account of science.[3] That thread is the contrast between explicit and tacit knowledge. Explicit knowledge is information or instructions that can be formulated in words or symbols and, therefore, can be stored, copied, and transferred by impersonal means, such as in written documents or computer files. Tacit knowledge, on the other hand, is 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 written or verbal instructions. Instead, someone demonstrates what to do and encourages them in the inevitably slow and error-ridden process of learning for themselves.

That many human activities depend upon tacit knowledge is widely recognized. It is one reason why many occupations are learned through apprenticeship to a skilled practitioner. The role of tacit knowledge is also a major barrier to the encapsulation of human knowledge in artificially intelligent machines (Dreyfus 1979; Collins 1990). However, the focus on *method* in the traditional view of science downplayed the role of tacit knowledge, and the image of technology as "applied science" led to a similar deemphasis there too.[4] Nevertheless, several authors have sug-

[2] There are, of course, evident connections to developments in other areas, in particular the ethnomethodological critique of structural-functionalist sociology (see, e.g., Heritage 1984) and the "situated action" critique of the symbol-processing paradigm in cognitive science (see Norman [1993] and the subsequent papers in that issue of *Cognitive Science*).

[3] For useful surveys from the early 1980s and 1990s, respectively, which indicate some of the tensions within the alternative view as well as common ground, see Knorr-Cetina and Mulkay (1983) and Pickering (1992).

[4] For more general weaknesses in the view of technology as applied science, see Barnes and Edge (1982, pp. 147–85).

gested that tacit knowledge is vital to the successful pursuit of science and technology (see Polanyi 1958, 1967; Arrow 1962; Burns 1969; Ravetz 1971; Collins 1974, 1975, 1985, 1990; Ferguson 1977, 1992; Knorr-Cetina 1981, 1992; Callon 1994).

H. M. Collins, above all, has shown the connections between an emphasis on tacit knowledge and other aspects of the alternative account of scientific knowledge. The dependence of successful scientific experimentation upon tacit knowledge makes experimentation a less solid bedrock of science than the traditional view assumes (Collins 1975, 1985). Because tacit knowledge is transmitted person to person, there are greater barriers to the spread of competence than the traditional view might lead us to expect. If science rests upon specific, hard-to-acquire, tacit skills, then there is a sense in which scientific knowledge is always local knowledge. It is, for example, often small "core sets," rather than wider scientific communities, that resolve scientific controversies (Collins 1974, 1981).

Most important for this discussion is how an emphasis on tacit knowledge indicates one way in which science and technology are not simply cumulative endeavors that result in permanent advances.[5] Barring social catastrophe, explicit knowledge, if widely diffused and stored, cannot be lost. Tacit knowledge, however, *can* be lost. Skills, if not practiced, decay. If there is no new generation of practitioners to whom tacit knowledge can be transmitted it may die out altogether.

Of course, such a loss need not be permanent. Some modern archaeologists, for example, believe they have recaptured the skills, long extinct in industrial societies, of Paleolithic flint knappers. The key point, however, is that the re-creation of tacit knowledge after its loss cannot simply be a matter of copying the original, because there is no sufficient set of explicit information or instructions to follow. The reacquisition of tacit knowledge after its extinction is, therefore, not necessarily any easier than its original acquisition; it may well be protracted and difficult. Furthermore, it is hard to know whether the original skill has been reacquired or a new, different skill created; we cannot know with certainty, for example, whether modern archaeologists knap in the same way as our ancestors.[6]

[5] Of course, the best-known argument against the cumulative nature of science is that of Kuhn (1970), which highlights the incommensurability of successive scientific "paradigms."

[6] A degree of knowledge of how the latter knapped can sometimes be recovered by the technique of "remontage," in which the original stone is gradually and painstakingly reconstructed from the flint implement and the discarded fragments. We owe our information on knapping to discussions with archaeologists at a conference at the Fondation des Treilles in June 1992.

Such considerations may seem distant from modern science and technology, especially in the area discussed here, nuclear weapons. The conventional wisdom about the latter is clear-cut. Knowledge of nuclear weapons cannot plausibly be lost, and those weapons cannot be uninvented. In the words of a group of prominent U.S. defense and international relations scholars, "The discovery of nuclear weapons, like the discovery of fire itself, lies behind us on the trajectory of history: it cannot be undone. . . . The atomic fire cannot be extinguished" (Harvard Nuclear Study Group 1983, p. 5).

Implicitly, however, this conventional wisdom rests on the traditional view of science and technology as impersonal and cumulative. True, if explicit knowledge were sufficient for the design and production of nuclear weapons, there would be little reason to doubt the conventional wisdom. Half a century of official and unofficial dissemination of information from the nuclear weapons laboratories, together with the normal publication processes in cognate branches of physics and engineering, mean that much of the relevant explicit knowledge is now irrevocably in the public domain.

Suppose, though, that the alternative view of science was true of nuclear weapons: in particular, that specific, local, tacit knowledge was vital to their design and production. Then there would be a sense in which relevant knowledge could be unlearned and in which these weapons *could* be uninvented. If there were a sufficiently long hiatus in their design and production (say a couple of generations), then that tacit knowledge might indeed vanish. Nuclear weapons could still be recreated, but not simply by copying from whatever artifacts, diagrams, and explicit instructions remained. In a sense, they would have to be *reinvented* (see Collins 1974, p. 176).[7]

Our concern here is only with these possible consequences of a lengthy hiatus in nuclear weapons development; we do not discuss the desirability, durability, or likelihood of such a hiatus (none of which, of course, is self-evident). However, considerations of tacit knowledge are not relevant only to comprehensive nuclear disarmament. Although the majority of current nuclear powers show no inclination to disarm entirely, they may well in the near future turn current voluntary moratoria into a permanent ban on nuclear testing.

As we shall see, nuclear testing has been a crucial part of the "epistemic culture" (Knorr-Cetina 1991) of nuclear weapons designers. Test-

[7] The Harvard Nuclear Study Group (1983, p. 5) also talks of reinvention, writing that "even if all nuclear arsenals were destroyed, the knowledge of how to reinvent them would remain." The difference between the group's position and that explored in this paper lies in the assumption that the necessary knowledge would still exist intact.

ing has made visible—to themselves and to others—the quality (or otherwise) of the nonexplicit elements constituting their "judgment." In its absence, certification of the safety and reliability of the remaining arsenals, and the design of any new nuclear weapons, will have to rely much more heavily on explicit knowledge alone—in particular, upon computer simulations. That is a prospect that many of the current generation of nuclear designers view with trepidation.

Furthermore, the balance of explicit and tacit knowledge in the design of nuclear weapons has clear implications for their proliferation. Hitherto, the most prominent barrier to the latter has been control over fissile materials. There is alarming—though not yet conclusive—evidence that such control has broken down seriously in the former Soviet Union (see, e.g., *Der Spiegel* 1994; for a skeptical opinion, see Joffe [1994]). If it becomes possible for aspiring nuclear states or terrorist groups simply to buy fissile material in the requisite quantities, then clearly a great deal hangs on precisely what knowledge they need to turn that material into weapons.

Before we turn to such matters, however, we need to assess the evidence concerning the role of tacit knowledge in nuclear weapons design, and most of the article deals with this evidence. After this introduction, we begin with brief accounts of the main types of nuclear weapon and of the current extent of explicit, public knowledge of their design. We then take a "first cut" at the question of whether explicit knowledge is on its own sufficient to design and construct an atomic bomb. This section draws evidence from the history of the wartime effort by the Los Alamos laboratory to turn explicit nuclear physics knowledge into actual working bombs.

The article then moves to a second form of evidence concerning the role of tacit knowledge in nuclear weapons design: designers' own accounts of the nature of the knowledge they deploy. This section is based on a series of semistructured interviews conducted by the authors with nearly 50 current or retired staff from nuclear weapons laboratories, including nuclear weapons designers and computing experts specializing in support for the computer modeling of nuclear explosive phenomena. These interviews dealt only with unclassified matters: no security clearance of any kind was sought by, or granted, the authors, and we neither asked for nor received information on the design features of particular weapons. However, we were able to discuss, in reasonable detail, the *process* of design and the knowledge used in that process.[8]

[8] Interviewees are listed in the appendix. Not all interviews were tape recorded, and the quotations below from the Bergen, Dowler and Talley, Hudgins, McDonald, Miller, Sewell, and Westervelt interviews are from notes rather than transcripts. However, all interviewees whom we wished to quote were sent drafts of intended

The third form of evidence about the role of tacit knowledge in nuclear weapons design is less direct and concerns the spread of nuclear weapons. Despite efforts to prevent the movement of personnel between nuclear weapons programs, five nations, in addition to the technology's American originators, have successfully conducted nuclear explosions, and three more are widely believed to have—or, in the case of South Africa, to have had—the capacity to do so (see table 1 below). A priori, this record of successful spread indicates that the role of local, tacit knowledge in nuclear weapons design is minimal. We draw on what is known of the histories of these programs to suggest that this is not so. Even the Soviet and British programs, both of which began by trying to reproduce an existing U.S. design, have more of the characteristics of reinvention than simple copying.

Our argument is that these three bodies of evidence, although not conclusive, strongly suggest that tacit knowledge has played a significant role in nuclear weapons design. The final section of this article goes on to consider whether the availability of "black box," "off the shelf" technologies eliminates this role. We contend that the history of the Iraqi nuclear weapons program suggests that it does not. We concede, however, that there are three reasons not to overstate the consequences of the role of tacit knowledge in nuclear weapons design: previous programs provide useful information on the "hardness" (Pinch, Collins, and Carbone, in press) of the task; relevant tacit knowledge can come not only from previous nuclear weapons programs but also from civil nuclear power and nonnuclear military technologies; and we cannot rule out a priori the possibility of simpler routes to the construction of crude but workable weapons.

We conclude, therefore, that it is necessary to take a broader view of what it would be deliberately to uninvent nuclear weapons. Even if deliberate uninvention does not take place, however, an accidental uninvention, in which much current tacit knowledge is lost, seems quite plausible: its consequences, we suggest, may well be of considerable significance in the years to come. At the very least, we hope that this investigation of the role of tacit knowledge in nuclear weapons design demonstrates that the sociology of science and technology, sometimes condemned as apolitical and even amoral (Winner 1993), need possess neither of those characteristics.

---

quotations and given the opportunity to correct errors or to withdraw permission for quotation. Only three interviewees exercised that latter right. The course of interviews was to a considerable degree dictated by what interviewees were prepared to talk about, and they dealt with many matters other than those discussed here. It was, therefore, impossible to ensure that all interviewees were asked the same questions. Nevertheless, there appeared to be a degree of consensus on the inadequacy in nuclear weapons design of explicit knowledge alone.

THE SCIENCE AND TECHNOLOGY OF NUCLEAR WEAPONS

Two physical processes are fundamental to nuclear weapons: nuclear fission and fusion. Fission is the splitting of an atomic nucleus by a neutron; fusion is the joining of two nuclei to form a single heavier one. "Atomic" bombs, such as the ones dropped on Hiroshima and Nagasaki, rely on fission. In such weapons, chemical explosives are used to turn a "subcritical" mass or masses of fissile material (in practice usually uranium 235 and/or plutonium 239)[9] into a "supercritical" mass, in which nuclear fission will become a self-sustaining, growing chain reaction.

One way of doing this is the gun method, in which the supercritical mass is created by shooting one subcritical piece of fissile material into another, using propellant explosives. That was the basic design of the bomb dropped on Hiroshima on August 6, 1945. However, the first atomic bomb (exploded at Trinity site, near Alamogordo, New Mexico, on July 16, 1945), the bomb that devastated Nagasaki, and most modern atomic bombs are of the *implosion* design shown in figure 1.

At the heart of an implosion weapon is a subcritical fissile core (typically of uranium 235 and/or plutonium 239). Around the core is a shell of chemical high explosives, built into a lens structure designed to focus its blast into a converging, inward-moving shock wave. Electrical systems detonate the chemical explosives as close to simultaneously as possible, and the resulting blast wave compresses the inner fissile core, the consequent increase in density making it supercritical. In the very short space of time before the core starts to expand again, an initiator (now normally external to the core, but, in early designs, inside it) produces a burst of neutrons to begin the fission chain reaction. The reaction is reinforced by an intermediate shell made of a material that reflects neutrons back inward, and this (or another) intermediate shell also acts as a tamper, helping hold the core together for as long as possible. If the bomb has been designed correctly, the fission reaction in the core is self-sustaining and growing in intensity, and it releases enormous amounts of energy as radiation, heat, and blast.

In a thermonuclear or hydrogen bomb, the destructive energy is provided by fusion as well as by the fission employed in an atomic bomb. The total release of energy, and thus the destructive power of a thermo-

[9] A chemical element (such as uranium) often exists in the form of more than one isotope. The nucleus of any atom of a given element will always contain the same number of positive particles (protons), but different isotopes contain different numbers of electrically neutral particles (neutrons). Isotopes are conventionally distinguished by their mass number, the total of protons and neutrons in their nuclei. Differences between isotopes are crucial to atomic physics. Thus uranium 235 is highly fissile (its nuclei readily split when struck by a neutron), while the more common isotope, uranium 238, is relatively inert.

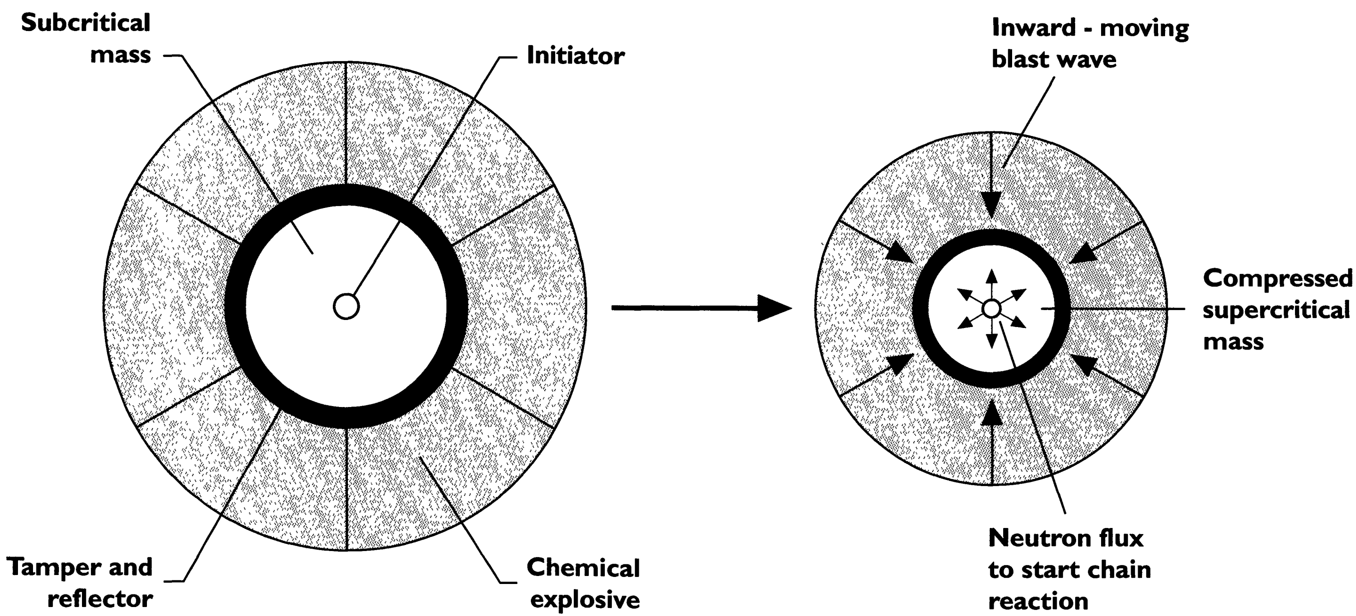

Fig. 1.—Atomic or fission bomb (implosion design). Highly schematic; no attempt has been made to draw to scale. In a boosted fission bomb, gaseous fusion materials are injected into the weapon's core as the chain reaction begins.

nuclear weapon, can be expected to be many times larger than that of a fission weapon; hence it was originally referred to as the "Super." When the latter was first discussed in the 1940s, the design envisaged—the "classical Super"—relied for the initiation of fusion essentially upon the heating, by a fission explosion, of liquid deuterium (one of the isotopes of hydrogen). In early 1951, however, the mathematician Stanislaw Ulam and physicist Edward Teller proposed a new design, in which the explosion of the fission "primary" compresses, as well as heats, a fusion "secondary." That design, or its independently developed equivalents, appears to be the basis of all modern hydrogen bombs; its details, however, need not detain us here.

Public Knowledge

At this general level, the design of a fission bomb is fully public knowledge, and there is little secret left to the hydrogen bomb. A mixture of an idealistic desire for informed public debate and a pragmatic concern to avoid lurid speculation led the U.S. government (to the alarm of the more cautious British) to release in 1945 a reasonably detailed history of the effort to construct an atomic bomb. This history outlined the military significance of the process of nuclear fission, described the basic principle of the "gun" weapon, and described in general terms the various processes used to produce fissile materials (Smyth 1945). Implosion designs were not discussed in the Smyth report. More recently, however, officially sanctioned publications have freely described implosion weapons at a level of detail roughly equivalent to that employed here (e.g., Gowing and Arnold 1974, 2:457), and unofficial sources (notably Hansen 1988) have discussed their designs in far greater detail.

Even without such publications, much could be inferred from relatively elementary physics. As long ago as 1946, it was reported that a "Midwestern teacher of high-school physics" had used the information contained in the Smyth report successfully to calculate the size of an atomic bomb (Friendly 1946, p. 3; see Smith 1970, p. 84). Since then, there have been reports that "undergraduates at Princeton and MIT have drafted roughly feasible atomic weapon designs, drawing only from unclassified documents" (Harvard Nuclear Study Group 1983, p. 219), as had scientists awaiting security clearance at the nuclear weapons laboratories (Hersh 1991, p. 155).

While the precise workings of the Teller-Ulam configuration have never been disclosed officially, the basic role of fusion in hydrogen bombs was openly discussed from the 1950s onward. In 1979, the radical U.S. magazine the *Progressive* sought to publish an article that contained conjectures about the nature of the Teller-Ulam configuration (Morland

1979). Through the law courts, the U.S. Department of Energy tried, ultimately unsuccessfully, to prevent its publication. That effort backfired, because it led to widespread attention and de facto official confirmation of some of Morland's inferences (DeVolpi et al. 1981); indeed, it made gathering and disseminating information on hydrogen bomb design something of a libertarian cause. A student working on behalf of the American Civil Liberties Union discovered, in the public-access stacks of the library at Los Alamos, a mistakenly declassified 1956 technical report on nuclear weapons development, UCRL-4725, which contained detailed information on hydrogen bomb design.[10] By the late 1980s, enough information had entered the public domain for hydrogen, as well as atomic, bomb design to be discussed extensively, if incongruously, in the lavishly illustrated coffee-table format of Hansen (1988).

From Idea to Artifact

Would public knowledge of this kind be sufficient to build a nuclear weapon? Let us narrow the question to a fission bomb: as we have noted, all mainstream designs of a hydrogen bomb rely upon a fission bomb to initiate fusion,[11] so if a fission bomb cannot be built, neither can a hydrogen bomb. One way of approaching the question is historical. Let us first consider the state of relevant, explicit nuclear physics knowledge as it stood at the establishment of the Los Alamos laboratory in 1943 and then examine what more the laboratory had to do to permit the explosion of the first atomic bombs in the summer of 1945.

In April 1943, theoretical physicist Robert Serber gave a five-lecture "indoctrination course" to Los Alamos's first recruits, in which he summarized the most salient aspects of available knowledge relevant to the task in front of them. His lectures (now available as Serber [1992]) show that the idea of an atomic bomb, as described by us above, was essentially

[10] We have not seen UCRL-4725. The acronym stands for University of California Radiation Laboratory; the University of California manages the Los Alamos and Livermore laboratories. For the document's significance, see DeVolpi et al. (1981) and Hansen (1988).

[11] We write "mainstream designs" because of recent reports, emanating from the former Soviet Union, that fusion could be initiated not by a fission bomb but by enhancing the detonation of chemical explosives with a substance called "red mercury." One article that takes this claim seriously suggests that the substance is produced by dissolving mercury antimony oxide in mercury, heating and irradiating the resulting amalgam, and then evaporating off the elemental mercury (Barnaby 1994, p. 81). Russian weapons designers, however, report that red mercury was simply the Soviet code name for lithium 6, which tends to get colored red by mercuric impurities during its separation (Hibbs 1993), and that it is therefore simply a component in the standard solid thermonuclear fuel, lithium 6 deuteride.

in place by early 1943. Indeed, the lectures, whose intended audience was primarily physicists, were considerably more detailed and quantitative than our description. They summarized relevant aspects of a recent but rapidly maturing body of knowledge, already "normal science" in the terminology of Kuhn (1970). Much "puzzle solving" (Kuhn 1970) had still to be done: in particular, detailed investigations of the interactions between neutrons and the nuclei of uranium and plutonium were necessary. By spring 1943, however, while "there was still much work to be done in nuclear physics proper . . . enough was known to eliminate great uncertainties from this side of the picture" (Hawkins 1946, p. 9).

Crucially, the physicists involved were confident enough of the status of their knowledge to feel reasonably sure of the likely destructive power of the weapon they hoped to build. George Kistiakowski, professor of chemistry at Harvard, had argued that "a fission weapon would be only one-tenth as effective" as a chemical one, but the physicists produced calculations predicting that an atomic weapon could have a force at least a thousand times that of a chemical explosive (Hoddeson et al. 1993, p. 41). Indeed, they were more perturbed by Edward Teller's 1942 speculation that the atomic bomb might be *too* powerful, extinguishing all life on earth by setting off runaway fusion of the nitrogen in the atmosphere. However, the "common sense" (Hawkins 1946, p. 15) of the elite physicists involved or consulted suggested that this was implausible. Detailed calculations—on the basis of well-established, explicit knowledge of nuclear forces—supported that common sense (Rhodes 1986, pp. 417–19; Hoddeson et al. 1993, pp. 45–46).

To some physicists, indeed, it seemed that the relevant explicit knowledge was mature enough to make designing an atomic bomb essentially trivial. To produce usable quantities of plutonium and uranium 235 was clearly a major industrial task, but that was not the laboratory's job. Edward Teller recalls being warned by his friend, theoretical physicist and future Nobel Laureate Eugene Wigner, not to join the new laboratory: "The only difficulty, according to Wigner, was the production of the needed nuclear explosive material, that is, plutonium. Once we had enough of that, he asserted, it would be easy and obvious to put together an atomic bomb" (Teller 1993, p. 33).

Even those who set up the new laboratory seem initially to have underestimated greatly the task they were undertaking. In May 1942, the future director of Los Alamos, Robert Oppenheimer, wrote that with "'a total of three experienced men and perhaps an equal number of younger ones,' it should be possible to solve the theoretical problems of building a fast-fission bomb" (Hoddeson et al. 1993, p. 42). When experimental physicist John H. Manley drew up the first plans for the new laboratory in the fall of 1942, he provided accommodation for "six theoretical physicists with six assistants, twelve experimentalists with fourteen assistants, and five secretaries." Oppenheimer originally enlarged Manley's plans only marginally, allowing space for a little expansion, for a low temperature laboratory for research on the "Super" (hydrogen bomb), and for a small engineering and machining facility (Hoddeson et al. 1993, p. 58).

Less than three years later, however, the technical staff of the Los Alamos laboratory numbered around 3,000 (Hoddeson et al. 1993, p. 400). One reason was the decision that it made more sense to purify plutonium at Los Alamos rather than beside the reactors at Hanford in Washington State (Manley 1980, p. 33). More generally, though, what had seemed in advance to be simple practical matters turned out to be far less straightforward than anticipated. To begin with, it was assumed that once the necessary fissile materials were available, fabricating a bomb would be straightforward, at least if the "obvious" (Smyth 1945, p. 127) gun design were adopted (implosion was acknowledged to be more complicated): "We thought we could just go to the military and buy a gun that would blow a couple of pieces [of fissile material] together fast enough to make an explosion. But fast enough turned out to be really very fast. On top of that, the whole business had to be carried by a B-29 and dropped . . . and the Navy or Army just don't make guns for those purposes. All of this put very stringent size and shape and weight requirements on a gun. The upshot was that for the most part the gun was designed and tested at Los Alamos" (Manley 1980, p. 33). Even with help and advice from the Naval Gun Factory, the Naval Ordnance Plant, the navy's senior gun designer, and the Bureau of Mines, the task was a demanding one. Furthermore, the Los Alamos team had to learn both how to refine the uranium 235 produced by the separation plant at Oak Ridge, Tennessee, and how to form it into the necessary shapes, tasks that led them into matters such as the design of crucibles and vacuum furnaces (Hoddeson et al. 1993, chaps. 7, 13).

The "really big jolt" (Manley 1980, p. 33), however, came in the first half of 1944, when it became apparent that reactor-produced plutonium differed in a crucial respect from the same element produced earlier, in tiny quantities, in laboratory cyclotrons.[12] Finding the properties of the latter type of plutonium had been demanding enough: to help in the work, Los Alamos hired an entomologist and other biologists skilled in handling small samples (Hoddeson et al. 1993, p. 35). The new problem was that the reactors were producing not just plutonium 239, the domi-

[12] The cyclotron, in which charged particles such as protons are accelerated, was a key experimental tool of early nuclear physics and one which could be used to produce small quantities of fissile materials.

nant isotope in the cyclotron samples, but also significant quantities of plutonium 240. That had been anticipated, but what was unexpectedly found in the spring of 1944 was that the heavier isotope seemed to have a much higher rate of spontaneous neutron emission. The planned plutonium gun, "Thin Man," seemed likely just to "fizzle"—that is, to suffer a premature, partial chain reaction—and in July 1944 it was abandoned. It was a painful crisis; Oppenheimer had to be persuaded not to resign his directorship (Rhodes 1986, p. 549).

The plutonium gun's problems did not affect the feasibility of a uranium gun, which had originally been given less priority but was now moved center stage. However, the physicists involved were reluctant entirely to jettison plutonium. The new element was, quite literally, their community's creation: unlike uranium, it does not exist in nature. As Manley later put it (1980, p. 33), "The choice was to junk the whole discovery of the chain reaction that produced plutonium, and all of the investment in time and effort of the Hanford plant, *unless* somebody could come up with a way of assembling the plutonium material into a weapon that would explode."

In implosion, the *idea* of how to do that already existed. With a gun design, only a relatively low-powered propellant explosive could be used, for fear of simply blowing the device apart before the nuclear chain reaction had time to develop. Implosion, however, would permit the use of a high explosive, and the resultant sudden creation of a critical mass by compression reduced the risk of a fizzle. But implosion moved the Los Alamos scientists onto new terrain.

In part, the move was into areas of physics with which they were less familiar: implosion is a problem in hydrodynamics rather than just in nuclear physics. To begin with, the Los Alamos team—which was perhaps the most talented group of physicists ever to be gathered together on a single site to achieve a single goal—seem to have felt that this should not be an insuperable barrier. However, "their work suffered from being too formal and mathematical" (Hawkins 1946, p. 29). Rescue came from the British delegation to Los Alamos, which included an immensely experienced hydrodynamicist, Geoffrey Taylor: "Most of the simple intuitive considerations which give true physical understanding came from discussions with Taylor" (Hawkins 1946, p. 29).

Of course, the Los Alamos team could not responsibly proceed on the basis of intuition alone; frantic efforts were also made to achieve a mathematical and experimental understanding of implosion. The former was greatly assisted by a batch of IBM punched card machines received by the laboratory in April 1944, but their results were not entirely trusted. A group of women (largely wives of the almost exclusively male Los Alamos scientists) also ground their way, for weeks on end, using handoperated mechanical calculators, through the massive quantities of arithmetic needed to flesh out a mathematical model of implosion. Different women were assigned different tasks—adding, multiplying, cubing, and so on—in a kind of reconfigurable arithmetical assembly line (Feynman 1980, p. 125; Metropolis and Nelson 1982, p. 359).

The implosion experiments were demanding in a different way. By using an inert core instead of plutonium, implosion could be investigated without risking a nuclear explosion. However, new procedures and new instrumentation had to be developed in order to record what went on in implosion: x-ray "flashes," ultra-fast cameras, placing a gamma-ray source at the center of the sphere and detecting the resultant rays after they passed through the shell and high explosive, and various other methods. Each of these in turn needed other problems to be solved. For example, the gamma-ray source (radiolanthanum 140) had itself to be isolated from radioactive barium, and a "hot" laboratory constructed where test implosions could take place without contaminating large areas.[13]

The results of the experiments were less reassuring than those of the mathematical model. Worryingly, the experimentally measured velocity of implosion appeared to be less than the model predicted. A hollow shell was more attractive than the solid sphere eventually employed, because a shell required less plutonium. However, jets of molten material seemed—the possibility that they were optical illusions was considered (Hawkins 1946, p. 77)—to squirt ahead of an imploding shell, upsetting symmetry and creating turbulence. Detonation waves also seemed to reflect at the surface of the imploding shell, causing solid pieces of it to break off.

Furthermore, the metallurgy of plutonium turned out to be considerably more complicated than that of uranium. Learning how to mold it into whatever shape was eventually chosen was felt to require a separate research program (largely conducted at the Massachusetts Institute of Technology) on the design of suitable crucibles and materials for coating them. Much work also went into how to construct a three-dimensional lens structure of high explosives that would adequately focus the imploding blast. The basic design of a suitable structure was drawn up by mathematical physicist John von Neumann. However, extensive research and development on the high explosives themselves was necessary, since no previous military or civil application of them had called for the high precision needed for implosion. Learning how to mold high explosive into the required shapes without cracks or bubbles appearing was a major

[13] Unless otherwise stated, details in this and the following four paragraphs are drawn from Hoddeson et al. (1993).

difficulty. Most basic of all, for implosion processes to stand a chance of being sufficiently symmetrical to achieve a full nuclear explosion, the explosive shell had to detonate virtually simultaneously at all points, an outcome that required much work on the electric detonators, on the development of firing circuits, and on the requisite timing equipment.

The initiator also posed difficult problems. Again, the basic concept employed—a device that would create a sudden large neutron flux by mixing the elements beryllium and polonium together at the crucial moment—had been outlined in Serber's lectures, but, as his later annotations dryly put it, actually designing and making the initiators for the gun and implosion weapons took "a great deal of effort" (Serber 1992, p. 52). Polonium is highly radioactive, decays quickly, and, like plutonium, had to be made in nuclear reactors. Getting the initiator design right required extensive experiments on ways of achieving the sudden mixing, experiments analogous to, but not identical to, those on implosion.

As a consequence of all these processes, the Los Alamos laboratory changed radically from its original intended form, which was not unlike a big university physics department. The constant flow of new recruits—especially to the ever-expanding Engineering Ordnance Division—had to be assigned to particular, narrowly delimited tasks. The overall weapon still to some degree bore an individual stamp. For example, the Trinity and Nagasaki design, "Fat Man," was also referred to as the "Christy gadget" after the original proponent of its solid core, the young theoretical physicist and astrophysicist Robert Christy (Hoddeson et al. 1993, pp. 270–71). Yet its design, and even that of the simpler uranium gun, were the products, not of individuals, but of a complex, differentiated organization.

TACIT KNOWLEDGE AND THE DESIGN AND PRODUCTION OF NUCLEAR WEAPONS

After reports of the horrors of Hiroshima and Nagasaki reached Los Alamos, those involved had to face (often for the first time) the full human meaning of what they had done. Some simply left to resume distinguished academic careers. Oppenheimer reportedly wanted to give the mesa, with its beautiful vistas and dramatic canyon, "back to the Indians" (Teller 1993, p. 33).

Of course, his wish was not granted. The Los Alamos laboratory continued, as did the design of further atomic—and soon hydrogen—weapons, and a similar laboratory was created in 1952 at Livermore in California. Let us, therefore, now move on in time to the late 1980s and to the process of nuclear weapons design as institutionalized in these laboratories, focusing on common features rather than on differences in style.[14]

"Institutionalized" is indeed the appropriate word, and some of the changes on the face of it suggest that the role of tacit knowledge in the process should be minimal. 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" (DeWitt 1990).

Even more strikingly, the role of computers in nuclear weapons design has expanded enormously. As we have seen, during the Manhattan Project, a "computer" was originally a woman, supplemented by a mechanical calculator or perhaps a punched card machine. During the late 1940s and early 1950s, digital computers were introduced, and they soon gave weapons designers computational capabilities unthinkable a decade earlier, capabilities that continued to grow exponentially in the decades to come. In turn, that has permitted the development and use of vastly more detailed and sophisticated mathematical models. The computer programs (referred to by those involved as the "codes") used to assist the process of nuclear weapons design are now very large and complex. A modern American code will typically involve from 100,000 to a million lines of program (Miller 1990), and many such codes are available to the designer.

Such codes have both a theoretical and an empirical basis. The theoretical basis is predominantly in physics. It is well-established physics, "normal science," and not regarded as a matter for debate and doubt. However, the code, and not merely the theory, is needed because the *implications* (see Barnes 1982) of that well-established knowledge for nuclear weapons as particular, concrete artifacts are not always transparent. Even today, nuclear weapons designers feel that they do not have a full "first principles prediction capability" (Bergen 1990). "You certainly can't do the calculations from first principles, basic physics principles. . . . That's a very frustrating thing" (Haussmann 1990).

The most obvious form taken by this problem is computational complexity. It is one thing to have sound, quantitative knowledge of physical phenomena available, for example, in well-established partial differential equations. It can be quite another matter to infer from those equations

[14] In its early years, Livermore seems to have placed greater emphasis on computer modeling than did Los Alamos and to have regarded itself as technically less conservative. Any remaining differences in these respects are now not great, and on the matters discussed in the text we could detect no systematic difference between the two laboratories.

what exactly will happen in an attempted explosion of a particular nuclear weapon. Often, interactions between different physical processes and nonlinearities in the underlying equations take desired solutions far out of the reach of the traditional physicists' methods of mathematical manipulation and paper-and-pencil calculation. Hence the need for the mechanical assistance of the computer.

The designers we spoke to, however, argued that even use of the most powerful computer—they have always enjoyed unique access to the world's fastest machines (MacKenzie 1991)—does not entirely bridge the gap between physical theory and concrete reality. One "can't even write down all the relevant equations, much less solve them," one designer told us, going on to add that even in the most modern codes, "major pieces of physics" were still left out (Miller 1990). The "codes only explain 95% of physical phenomena at best, sometimes only 50%" (Hudgins 1990).

All codes, they say, involve approximations. This is particularly the case for the "primary" (an atomic bomb or the fission component of a hydrogen bomb) rather than the "secondary" (the fusion component of the latter): "The primary is less well understood than the secondary. Material physics is cleaner in the secondary: everything happens at high temperatures and pressures. The primary involves transitions from cold metal at low pressure and temperatures to high pressures and temperatures" (Bergen 1990).

The difficulty of predicting on the basis of explicit knowledge alone seems to be at its perceived peak with "boosting," in which gaseous fusion materials are injected into a fission weapon as it begins to detonate (van Cleave 1973). The neutrons generated by the fusion of these materials can considerably intensify the fission chain reaction. According to one U.S. weapons designer, "It is boosting that is mainly responsible for the remarkable 100-fold increase in the efficiency of fission weapons" since 1945 (Westervelt 1988, p. 56). If, however, the effects of boosting are insufficient, the small boosted primary in a modern thermonuclear bomb could simply fail to ignite the secondary, and the resultant explosion will be many times weaker than anticipated. Yet boosting is both hard to model numerically and hard to study in laboratory experiments, since the fusion reaction begins only when the fission explosion is underway. Because of the difficulty of accurate prediction, "the design of boosted fission devices is an empirical science" (Westervelt 1988, p. 56).

More generally, though, our interviewees saw *all* codes as needing an empirical as well as a theoretical basis, because they are approximations to reality, rather than simply mirrors of it. Although nonnuclear experiments such as test implosions play an important role in providing this empirical basis, the ultimate check on the validity of the codes is nuclear explosive testing. That allows particular parameters whose values cannot be deduced from theory to be estimated empirically and permits a code to be "normalized": its predictions are checked against measurements made during testing, and the code is adjusted accordingly. Tests "almost never hit the predicted numbers exactly," and, for example, the prediction is reckoned to be "pretty good" if the actual yield (explosive energy released) is "within 25% of prediction" (Bergen 1990).

"No new code is used until it predicts the results of previous tests" (Bergen 1990), and the modeling process is seen as having improved greatly over the years. Even with modern designs and modern codes, though, measured yield sometimes falls significantly short of predicted values for reasons "we have not yet been able to explain" (Miller, Brown, and Alonso 1987, p. 4). On other occasions, codes "would give the right answer [i.e., correctly predict yield], but you didn't know why it gave you the right answer" (Bergen 1990).

The need for testing to develop and check codes does not, however, make testing an entirely unambiguous arbiter of their validity. The yield of a nuclear explosion is not a self-evident characteristic of that explosion but has itself to be measured. Even in the mid-1980s, such measurements were seen as subject to an uncertainty of as much as 5% (Kidder 1987, p. 6); another source suggested to us (in a private communication) that the uncertainty is actually greater than this. Furthermore, even entirely successful prediction of yield, or other "global" characteristics of a nuclear explosion, does not conclusively demonstrate the correctness of a code or model:

> There are many aspects of the designs that we still don't understand well enough, and the reason for that is that most of the data we get is what you might call an integrated result, in that it's the sum of what happened over a period of time. You never know in detail what happened during that short time interval, and because of that there could be several different calculational models that actually explain what happened. And each one of those might look OK for a given set of circumstances but could be completely wrong for some other set of circumstances; and you don't know what those circumstances are, and so you're vulnerable. (Hoyt 1990)

Between 10% and 30% of U.S. nuclear tests are not direct tests of a weapons design but "physics understanding tests" specifically designed to investigate theoretical or computational models of nuclear explosive phenomena (Hoyt 1990). Even these tests have their limitations. Nuclear explosions are both fast and destructive, and thus hard to study empirically: they almost immediately destroy sensors placed close to the blast. Above all, "you . . . don't have the ability to put your instruments inside [the bomb] in the places where you really would like to get the detailed measurements. If you put your instruments in, then the device won't work" (Hoyt 1990).

The Role of Judgment

With the implications of theory not entirely clear-cut, with a continuing gap between model and reality, and with the results of experimentation and testing not always decisive, what remains is "judgment" (e.g., Miller, Brown, and Alonso 1987, p. 4 and passim). Judgment is the feel that experienced 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 (see below). These contingencies are so numerous and the number of nuclear tests so limited by their great expense and increasing political sensitivity that "nuclear warheads cannot be 'thoroughly' tested; the resources simply are not available. As a result, the functional capabilities of nuclear explosives cannot be fully established without a strong dependence on the scientific judgment of the weapon scientists" (Miller et al. 1987, p. 4).

According to our interviewees, that 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 experienced judgment, and some crucial phenomena simply cannot be expressed fully in the codes.

One designer told us he had tried to make all this knowledge explicit, by writing a classified "textbook" of nuclear weapons design, and found that he could not: "It's too dynamic" (Hudgins 1990). "Art," rather than "science," is indeed a word that several nuclear weapons designers reached for to describe their trade. It is "very much an empirical art" (Westervelt 1990); it is "artsy" (McDonald 1990).

As a result, there is "a long learning curve" for new designers (Hudgins 1990): it takes new designers, even with the relevant background in physics, "five years to become useful" (Westervelt 1990), and it takes perhaps 10 years "to really train" them (Hudgins 1990). The number of fully experienced nuclear weapon designers is limited. In the late 1980s, there were "about 50 good designers" in the United States; at its maximum, around 1976, the total was only 80 (Dowler and Talley 1990).

Another interviewee estimated the total in the late 1980s as "about 40" designers at Livermore and 30 at Los Alamos; they were the "only ones who understand nuclear explosions" (Hudgins 1990). The figures they would give for 1994 would be much lower (private communications from interviewees).

Designers' judgment is a communal, and not merely an individual, phenomenon, and "community" is a reasonable term to use, so long as it is not taken to imply harmony (see Knorr-Cetina 1981, pp. 69–70; there has, e.g., often been fierce rivalry between Los Alamos and Livermore). First, judgment is passed on, face-to-face and person-to-person, from "senior designers . . . to younger designers" (Miller et al. 1987, p. 4) in what is essentially a relationship of apprenticeship as they work together in design and analysis. Second, judgment is collective and hierarchically distributed. Individuals may propose new approaches, and many designs are seen (like the Christy gadget) as bearing the imprint of a particular "lead designer." But no nuclear weapon design goes into production without intensive and extensive peer review. As a result, to put it in idealized terms, "Our scientific judgment is broader than just the experience of each individual weapon scientist; the collective judgment of the entire weapon research infrastructure works synergistically to solve the problems we encounter" (Miller et al. 1987, p. 12). More mundanely, "younger designers take the output from their computer simulations and their interpretations of experimental results to test-seasoned senior designers for review and confirmation" (Miller et al. 1987, p. 26). The process is competitive and highly charged. One designer told Hugh Gusterson, who has recently completed a remarkable anthropological study of the Livermore laboratory, that "for every twenty things people propose, maybe one is going to make it onto that shot schedule [i.e., full nuclear explosive testing]. . . . I've seen men all in tears" at the reaction to their proposals (Gusterson 1991*a,* p. 258; see also Gusterson 1991*b,* 1992, 1993).

Tacit Knowledge in Nuclear Weapons Production

Uncodified, personally embodied, and communally sanctioned knowledge thus plays, according to our interviewees, a continuing central role in nuclear weapons design. Tacit knowledge is also important to the process of turning even the most detailed design into a physical artifact. Theory and computation deal with geometric abstractions such as cylinders and spheres, but "nothing is truly a sphere," because there are always "little wobbles on the surface" and a small "difference in radius as you come out in different directions" (Mark 1991). Quality of machining—and thus the skill of machinists—is vital; and numerically controlled machine tools do not entirely remove this skill dependence, as Noble (1984) has found more generally.

Quality of machining can at least be checked independently without damaging the final product. But there are other aspects of nuclear weapons fabrication where such testing is impossible or impractical. An example is the solid-state bonding used in the W84 warhead for the ground-launched cruise missile, "where there is no adequate nondestructive testing technique that can evaluate the quality of the bonds. . . . One of the key features of this process is the assured removal of all oxide from the surface before a layer of another metal is applied. . . . Simple things such as the way in which the part is clamped in its holding fixture can affect the rate of oxide removal. . . . Although we have tried several techniques to make this evaluation with instrumentation, we have found none equal the human eye . . . for detecting the change to a shiny, then slightly hazy, appearance that indicates a clear surface" (Miller et al. 1987, p. 55).

Another—utterly pervasive—issue is that even with the careful handling that the components of nuclear weapons receive, it is inevitable that some of the thousands of separate parts that go to make up such a weapon will receive slight nicks and scratches as they are manufactured and assembled. Often these will be unimportant, but sometimes they would affect the performance of a weapon, and discarding or fully testing each slightly scratched part would be prohibitively expensive. So a procedure has had to be developed where reports on individual components with a nick or a scratch are sent from production plants to the nuclear weapons laboratories, and designers there draw on their experience to make a judgment whether the defects matter. In the late 1980s, designers at Livermore were processing about 150–200 such evaluation requests per system per month (Miller et al. 1987, p. 55).

Yet another issue is that many aspects of high-explosive manufacture, to the specifications required for an implosion weapon, "are as much an art as a science" (Miller et al. 1987, p. 28). While another source suggests (in a private communication) that this may be putting matters too strongly, there is a potentially significant issue here, because nondestructive testing of explosives is hard to envisage—unless, of course, one sample of explosives can be relied upon to be the same as others.

Where tacit knowledge is involved, however, judgments of "sameness" become problematic. Just as the dependence of scientific experimentation upon tacit skills can give rise to controversy over what counts as a competent replication of an experiment (Collins 1985), so the products of a nonalgorithmic production process cannot be relied upon consistently to be identical. In the production of nuclear weapons, "Documentation has never been sufficiently exact to ensure replication. . . . We have never known enough about every detail to specify everything that may be important. . . . Individuals in the production plants learn how to bridge the gaps in specifications and to make things work. Even the most complete specifications must leave some things to the individual's common knowledge; it would be an infinite task to attempt to specify all products, processes, and everything involved in their manufacture and use" (Miller et al. 1987, p. 25).

This issue of sameness has three aspects. First, production weapons can differ from laboratory-produced prototypes, because those involved in the manufacture and assembly of the former may lack the knowledge of those who made the latter. In early bombs, "the fellows who designed the circuits or the mechanical components almost had to be there when they were put together, because they were the only ones who understood how they worked" (Mark 1991). Second, weapons produced to the same design at different times can differ: "Material batches are never quite the same, some materials become unavailable, and equivalent materials are never exactly equivalent; 'improved' parts often have new, unexpected failure modes; different people (not those who did the initial work) are involved in the remanufacturing; vendors go out of business or stop producing some products; new health and safety regulations prohibit the use of certain materials or processes" (Miller et al. 1987, p. 3). Third, even an individual weapon may change over time through radioactive decay, chemical decomposition, corrosion, and materials "creep" (Rosengren 1983). Weapons are regularly inspected and "if parts have deteriorated, they are replaced with parts that do not differ significantly from the original" (Collina and Kidder 1994, p. 25), but this again raises the issue of judging the significance of differences, given that the production of parts cannot be wholly algorithmic.

TACIT KNOWLEDGE AND THE SPREAD OF NUCLEAR WEAPONS

Perhaps, though, all this testimony on the current role of tacit knowledge needs to be taken with a pinch of salt. Some of it—in particular Miller et al.'s (1987) report to Congress—has been part of a continuing struggle to ward off a comprehensive ban on nuclear testing; some of it might even be seen as the self-justification of an elite group whose occupation is under threat. More particularly, the current generation of American nuclear weapons designers has worked primarily on highly sophisticated weapons. Evolving military requirements and the competition between weapons laboratories have created both pressures and incentives to maximize yield/weight or yield/diameter ratios and to economize on special materials such as the hydrogen isotope tritium (used in boosting). These pressures and incentives have pushed the design of boosted primaries "near the cliff," as some of those involved put it: close to the region where performance becomes very sensitive to internal and external conditions, with the result, for example, that the explosion of a "primary" might fail to ignite the "secondary."

Near the cliff, the need for experienced judgment is conceded by all those involved. But further away from it, in the design of more basic, physically larger weapons, "much of the physics of nuclear weapons is quite forgiving" (Haussmann 1990), and the role of judgment is more disputable. Let us, therefore, turn to a third kind of evidence concerning the role of tacit knowledge in nuclear weapons design and production: the record of the spread of nuclear weapons design capability.

This record is relevant because, if explicit knowledge were sufficient for the design of at least basic nuclear weapons, acquiring them would be a straightforward matter for those who possessed both the necessary fissile material and the requisite knowledge: "public" nuclear physics plus, for example, a detailed diagram and associated instructions to cover the more practical side of design. If, on the other hand, tacit knowledge plays a key role, even the most detailed explicit knowledge would not on its own be enough. The recipients and the originators of such knowledge would have to be members of the same or similar technical cultures in order that the recipients can bring tacit background knowledge to bear in order to "repair" the insufficiency of explicit instructions (Collins 1990).

In addition, while explicit knowledge can be copied, tacit knowledge (in the absence of prolonged, hands-on, face-to-face interaction), has to be re-created. It is much easier to copy a book or a computer program than to write it in the first place, but there is no reason in principle to expect the re-creation of tacit knowledge to be any easier than its original creation.[15] Furthermore, precisely because tacit knowledge is not codified, both the skill and the product that is re-created may not be the same as the original. Even if one sets out to copy, one may end up doing and building something that is, from some points of view, different from the original.

As we shall see, the spread of nuclear weapons design capability has generally taken place (at least in the well-documented cases) without extensive personal contact with previous successful programs. Furthermore, at least two programs have attempted to copy the results of previous programs, in at least one case on the basis of explicit knowledge alone. These two predictions—the difficulty of re-creation and the prob-

[15] As we discuss in the conclusion, there are some particular contingencies that may affect the ease of re-creation.

lematic nature of copying—can, therefore, be tested, at least within the limits of the available data.

Livermore

Let us begin with the second American laboratory, Livermore, set up in September 1952. Although there were no formal security barriers between it and Los Alamos, relations between the two were troubled. Los Alamos staff resented criticism of the laboratory by Livermore's founder, Edward Teller, and felt that they had been denied due credit for the first thermonuclear explosion in November 1952 (Bradbury [1954] 1983).

Only a small minority of members of the new laboratory seem to have had direct previous experience of nuclear weapons design. Even Teller himself had, in his wartime Los Alamos work, focused on research on the Super, of which he was the main proponent, rather than on the practicalities of fission bomb design. Teller aside, the core of Livermore's initial cadre was a group at the University of California, Berkeley, of around 40 people, including about 20 physics postdocs, set up in 1950 to study thermonuclear explosive phenomena experimentally (York 1976, p. 126).

For Livermore staff with the appropriate security clearances, there were no barriers to access to the stock of explicit knowledge (diagrams, data, etc.) generated at Los Alamos. "The Los Alamos administration treated the Livermore leadership formally correctly, and provided some much needed technological assistance to the new laboratory," the latter's first director reports (York 1976, p. 134). However, the tension between the two laboratories meant that face-to-face collaboration was not always easy.

The new laboratory's first efforts were failures, although that was in part because of a deliberate Livermore decision *not* to try to copy what Los Alamos had done. Livermore's first two tests (on March 31 and April 11, 1953) were of fission bombs with cores of uranium hydride, rather than metallic uranium or plutonium. The hope seems to have been that use of uranium hydride could help miniaturize atomic weapons (Cochran et al. 1987, pp. 153–54; Hansen 1988, pp. 32–33, 39).

Both tests were embarrassing fizzles. In the first, the weapon failed so badly that the tower supporting it was left standing. Although Livermore staff tried to pull the tower down with a jeep, they did not manage to do so before Los Alamos photographers had captured their rivals' humiliation (Wood and Nuckolls 1988, p. 316). Livermore's first hydrogen bomb test on April 6, 1954 was also a disappointment, producing less than one-tenth of the expected yield. It was March 1955, two and one-half years after Livermore's establishment, before a Livermore test was successful, and only by 1956 was "the Laboratory . . . beginning to be trusted as a nuclear weapons design organization" (Sewell 1988, p. 323).

On the other hand, although overseas nuclear weapons programs were also typically to encounter fizzles at various points in their programs (see, e.g., Spector 1987, p. 35), their first tests all seem to have been successful (there have been rumors of a failed Indian test prior to the successful one in 1974, but an informed source has told us, in a private communication, that these rumors are false, although a serious problem was encountered). Since, as noted in the introduction, this is certainly a priori evidence against a strongly "local knowledge" view of nuclear weapons design, let us now turn to these overseas efforts. Those of them that are believed to have been successful are summarized in table 1.

The Soviet Union and United Kingdom

The Soviet and British efforts are of particular interest from the point of view of tacit knowledge, because both began by trying to copy the Christy gadget, the first American implosion bomb. The Soviets did so on the basis of explicit knowledge alone. Although the Soviet Union had considerable strength in nuclear physics and had set up a small wartime project investigating a possible atomic weapon, no Soviet scientist took part in the Manhattan Project, nor did any member of the latter join the Soviet bomb effort.

Instead, the Soviet team worked from "a rather detailed diagram and description of the first American bomb" (i.e. the Christy gadget), which had been given to Soviet intelligence by Klaus Fuchs, a German refugee physicist who was a member of the British mission to Los Alamos and who had been involved intimately with the design of the core and initiator of the plutonium implosion weapon. In the second half of 1945, the leader of the Soviet fission bomb project, Yuli Khariton, and a small number of trusted colleagues were given the documents from Fuchs. Although they were already working on their own fission bomb design, they decided that it would be safer to make a "copy" (Khariton and Smirnov 1993, p. 22) of the Christy gadget.

Despite the enormous priority their work was granted by Stalin, it took them four years from the receipt of the material from Fuchs, slightly longer than the original Manhattan Project: "in order to build a real device from the American design, it was first necessary to perform a truly heroic feat that required nationwide mobilization: to create an atomic industry, corresponding technologies, a supply of unique, high-quality apparatus, and to train qualified people" (Khariton and Smirnov 1993, p. 22). Although Fuchs's data and the Smyth report (Smyth 1945) gave

TABLE 1

APPROXIMATE CHRONOLOGIES OF SUCCESSFUL NUCLEAR WEAPONS DEVELOPMENT PROGRAMS

Country	Start of Nuclear Weapons Development Program	Date of First Atomic Test Explosion (*) or Weapon (†)	Date of First Thermonuclear Test Explosion (*) or Weapon (†)	Significant Personal Contact with Previously Successful Weapons Design Team?	Began with Attempt to Copy Previous Design?
United States	1942	1945*	1952*	no	no
Soviet Union	1945	1949*	1953*	no	yes
United Kingdom	1947	1952*	1957*	yes	yes
France	1955	1960*	1968*	no	?
China	c. 1955	1964*	1967*	no	no
Israel	c. 1957 (?)	c. 1968 (?)†	?†	?	?
India	c. 1964	1974*		?	?
South Africa	1971	1979†		?	?
Pakistan	c. 1974 (?)	?†		?	yes (?)

SOURCES.—Albright and Hibbs (1992*b*, 1992*c*); Baylis (1994); Gowing (1964); Gowing and Arnold (1974); Hersh (1991); Hewlett and Anderson (1962); Hewlett and Duncan (1969); Holloway (1981, 1994); International Atomic Energy Agency (1993); Institut Charles-de-Gaulle (1984); Khariton and Smirnov (1993); Lewis and Xue (1988); Mongin (1991); Scheinman (1965); Spector (1987); Szasz (1992).

NOTE.—India, South Africa, and Pakistan are not believed to have developed thermonuclear weapons; it is not clear whether Israel has done so.

them the confidence not to pursue as many approaches in parallel as the Americans had done, the Soviet team did, effectively, end up recapitulating much of the work of the Manhattan Project.

In particular, they found that building a "copy," even with the detailed diagram and description Fuchs had given them, was not easy. When Khariton named 70 people he wanted for the first Soviet nuclear weapons design facility, Arzamas-16, he was asked why he needed so many (David Holloway, personal communication, February 15, 1994); in the event, he was to need many times that number. According to Khariton, "the information received from Fuchs did not lessen substantially the volume of experimental work. Soviet scientists and engineers had to do all the same calculations and experiments" (Holloway 1994, p. 199). Although the requisite nuclear experiments were demanding and dangerous, it was the engineering aspects of the work that seem to have caused most problems: "The scientists were too inexperienced and amateurish in the complex processes of mass production" (Zaloga 1993, p. 53). In 1948, an experienced engineer from outside industry, General N. L. Dukhov, had to be brought in as Khariton's deputy to take charge of Arzamas's engineering work (Holloway 1994, p. 199).

Many of the problems faced in this effort had to do not directly with the fissile core but with the practicalities of achieving successful implosion: "Even with the espionage material that had been made available, considerable effort was needed by Soviet chemical engineers to devise the technology to manufacture . . . large castings of homogeneous high explosive. Moreover, extensive testing was needed to ensure that the explosive charges detonated uniformly and predictably" (Zaloga 1993, p. 54). The electrical system required to achieve simultaneous detonation was a further key problem, and another senior engineer, V. I. Alferov, was brought in to Arzamas to take responsibility for it (Holloway 1994, p. 199). Nor, finally, was the device they ultimately produced seen by those involved as entirely identical to the American original; although it was "very close" there were "minor differences" (D. Holloway, personal communication, September 20, 1994; he was, unfortunately, unable to elicit what these differences were).

The British bomb team had both explicit knowledge of the American design and also, unlike the Soviets, a considerable degree of personal involvement in the processes leading to that design. British scientists (native and, especially, refugees from fascism) had indeed led the way in arguing that an atomic bomb was feasible; particularly important were a 1940 memorandum by two of the refugee physicists, Otto Frisch and Rudolf Peierls, and the subsequent program of research in Britain under the "MAUD Committee" in 1940–41. The British role became subordinate to the American from 1942 onward, but a British mission was established at Los Alamos, and some of its members (such as Peierls, Fuchs, Taylor, and experimentalist James Tuck) played central roles in the laboratory's work (Szasz 1992).

Anglo-American collaboration was ended by the U.S. Atomic Energy Act of 1946. When the British atomic bomb project began in 1947, it took the same decision as had the Soviets: to copy the Christy gadget. Under the agreement with the Americans, written records had been left behind at Los Alamos, but the former British team helped compile from memory "a working manual" that they hoped "would enable the American atomic bomb to be duplicated, without all the laborious Los Alamos work" (Gowing and Arnold 1974, 2:456). Particularly helpful was Klaus Fuchs, whose work on behalf of the Soviets meant that his memory of what had been done at Los Alamos was "outstanding" and who, unlike his colleagues, had removed written material from there (Cathcart 1994, p. 105)

Again, though, copying the Christy gadget turned out not to be straightforward. At the level of explicit knowledge, the former Los Alamos team members were well placed: they "were able to list very clearly the bomb components and to set out the principle of the bomb" (Gowing and Arnold 1974, 2:456). At the practical level, however, their knowledge was more patchy. Although they had been over 20 in number and widely dispersed through Los Alamos's divisions, members of the British mission had not had personal involvement in all the aspects of the laboratory's work. Furthermore, knowing what the final product should be like was not the same as knowing how it could be made. For example, although "some convenient plutonium handling tricks were . . . known," the British team's knowledge of plutonium metallurgy was "sketchy" (Gowing and Arnold 1974, 2:458). None of them knew how to make crucibles into which molten plutonium could be poured without it dissolving or reacting with the crucible material (Szasz 1992, pp. 51–52). Similarly, much work had to be done on the chemistry of the initiator's polonium and on how to manufacture and handle it (Cathcart 1994, p. 132).

Indeed, the hope of avoiding "all the laborious Los Alamos work" was largely disappointed. The first (November 1944) plans for a postwar British atomic energy research establishment had envisaged a staff of less than 400, covering reactor development as well as weapons work (Gowing 1964, p. 330). By the start of 1952, however, the program's "nonindustrial" staff numbered over 5,000, with more than 1,000 of these devoted to the weapons work alone (Gowing and Arnold 1974, 2:90). Furthermore, the five years it took to make the intended copy was longer than it had taken to make the original. In part, that was because the atomic weapons program met with obstruction, especially over the release of skilled staff, from organizations within the British state whose priorities were different (Cathcart 1994). In part, it was because Britain had fewer resources to devote to the production of fissile material. In addition, the experiments whose detailed numerical results had been left behind at Los Alamos had to be replicated.

More generally, though, despite all the knowledge inherited from Los Alamos, the problems of the design, fabrication, and testing of weapon components still turned out to be "diverse and most intricate," the work "dangerous and difficult" (Gowing and Arnold 1974, 2:459, 2:72). Even in those areas, like explosive lens design, where the British felt confident of their knowledge, many practical problems arose: for example, despite much work on methods of casting, no way could be found of stopping the lenses from shrinking unevenly in their casts. Techniques for constructing the detonation circuitry "had very often to be invented, and then they had to be practised and perfected" by the women production workers who had to implement them (Cathcart 1994, p. 71). With a 1952 target date set for the first test explosion, the last year of the program became "a frantic race against time with serious problems solved only at the eleventh hour—questions of design, assembly systems, cavities in the castings for the uranium tamper, the firing circuit, the plating of various components, plutonium and polonium supply" (Gowing and Arnold 1974, 2:474).

Despite their initial intentions and a strong, continuing desire not to lose "the safe feeling [of] making an object known to be successful" (Gowing and Arnold 1974, 2:472), the British team found they could not successfully "duplicate" the Christy gadget. The Americans had assembled the Nagasaki bomb on the ground, but the British felt it unsafe for a bomber to take off with a complete weapon onboard and so wanted the final assembly to take place in flight. However, they became worried that the weapon might inadvertently become supercritical while this was being done. In September 1950, the project's leader, William Penney, reluctantly "took the major decision to alter the design at the heart of the bomb" (2:472). As at Los Alamos, a team then set to work to grind out on mechanical calculators a numerical simulation of the likely results of an alternative design. Following this simulation, the design was changed once more in mid-1951 to include a two-inch gap between tamper and core (Cathcart 1994, p. 140). The momentum of the tamper moving inward through the gap intensified the compression of the core, but this third design involved a more complicated mechanical structure and was "more sensitive to implosion imperfections" (Gowing and Arnold 1974, 2:472). This sensitivity was particularly worrying, since the actual shapes of the explosive lenses differed from specification: with no available solution to the problem of lens shrinkage, the team had to resort simply to "the use of PVC adhesive tape to fill up the clearance spaces and minimise settlement" (2:463). Only in the summer of 1952 did high-explosive firing trials provide reassurance that these imperfections would be small enough not to cause failure.

France and China

Less is known about the detailed history of the French atomic weapons program than about the British or even the Soviet effort. Like their Soviet and British counterparts, French physicists had considered the idea of an atomic bomb early in the Second World War (Chevallier and Usunier 1984, p. 127). Some of them had also taken part in the Manhattan project, although their involvement was with the production of fissile materials rather than in weapons design. Unlike in the United Kingdom and the Soviet Union, there was significant political opposition to a French nuclear weapons program, together with a feeling, in the early postwar years, that it was too ambitious an undertaking for anything other than a superpower. The successful British test in October 1952 undermined that latter belief (Goldschmidt 1984, p. 29), and government commitment to nuclear weapons development crystallized in 1954. In 1955, two nuclear weapons research centers were established. One, at Bruyères-le-Châtel, concentrated on the physics, metallurgy, and chemistry of nuclear materials; the other, at Vaujours, dealt with detonics, the study of matters such as high-explosive blast waves (Chevallier and Usunier 1984, pp. 128–29).

In February 1960, the first French atomic device was successfully exploded at Reggane in the Sahara. Like the Soviets and British, the French seem to have focused their efforts on a plutonium implosion weapon (Buchalet 1984, pp. 40–41). We have found no evidence that the French attempted to copy a previous such weapon and presume that their design was developed by themselves. Certainly, though, the development effort was considerable. In 1957, the project employed 750 staff (over and above those devoted to plutonium production); that figure had to be tripled in two years to achieve the 1960 target date (Buchalet 1984, pp. 51, 57). Their tasks were primarily the solution of practical problems: "The atomic bomb is to a large extent an engineering problem" (Chevallier and Usunier 1984, p. 130; our translation). No details are available, though Buchalet (1984, p. 48) writes that plutonium metallurgy caused particular concern.

The history of the Chinese nuclear weapons program has been documented, at least in broad outline, in a remarkable study by Lewis and Xue (1988). Just as no member of the Soviet program had worked on the Manhattan Project, so it appears that no member of the Chinese project had been directly involved with either Soviet or Western nuclear weapons design. Although the 1957 Sino-Soviet Defense Technical Accord committed the Soviet Union to supply China with a prototype atomic bomb, the Soviets reneged on that promise, and they do not even seem to have provided weapon design information at the level of detail that had been supplied by Fuchs. Two nuclear weapons designers were among the Soviet technical experts sent to assist the Chinese. When the latter were withdrawn in 1960, the two nuclear weapons designers left behind them, shredded but still legible, useful data on implosion. In general, though, their Chinese counterparts remember the nuclear weapons designers as "mute monks who would read but not speak" (Lewis and Xue 1988, p. 160).

Although the Soviets were more helpful in other areas—notably in the supply of a nuclear reactor and a cyclotron, in handing over design data for a uranium separation plant, and in the general training of thousands of Chinese nuclear engineers—Chinese nuclear weapons design thus had to proceed without the benefit of hands-on contact between it and a previously successful program. Like the Soviet, British, and French programs, the Chinese program took longer than the original Manhattan Project: in this case, roughly nine years (from 1955 to the first Chinese nuclear explosion in October 1964). It was a massive national effort, involving several hundred thousand people, including a unique mobilization of tens of thousands of peasants who were given basic training in uranium prospecting and refinement.

Again, the obstacles met in this effort seem to have been predominantly practical engineering problems rather than, for example, deficits in explicit knowledge of nuclear physics. There is no evidence that the design of the weapon itself was an attempt to copy a previous device: indeed, the Chinese chose to begin their program differently from the Soviets, British, and French, constructing a uranium implosion weapon rather than a plutonium one. The design and fabrication difficulties encountered seem broadly similar to those faced by previous programs. Particularly problematic areas included the design and molding of explosive lenses, the selection and production of materials for the initiator, and bubbles in the uranium castings (Lewis and Xue 1988, pp. 87–88, 106, 150–69).

More Recent Nuclear Weapons Programs

All nuclear weapons programs subsequent to China's have been covert. The Israeli government has never explicitly admitted to possessing a nuclear arsenal, and South Africa did so only in 1993. India maintains that its 1974 test in the Rajasthan desert was of a "peaceful" nuclear explosive, not a bomb, while Pakistan has admitted officially only to the possession of the "components" of an atomic bomb (Albright and Hibbs 1992*b,* p. 38). Given this desire for secrecy, it is not surprising that little is known with any reliability about the sources of knowledge drawn on in these nuclear weapons programs. There have been widespread reports of assistance given to new programs by previous ones—notably by France to Israel, by Israel to South Africa, by China to Pakistan, and, perhaps, by the Soviet Union to India—but it is impossible to be sure of the nature of such assistance.

What little is known with any confidence seems broadly compatible with what we can learn from the histories of the better-documented programs. To the extent that we can determine their chronologies, all seem to have taken longer than the original Manhattan Project. The few specific development problems that have been reported with any authority are primarily practical: for example, the leader of the Indian program reports particular difficulties with the initiator (Albright and Hibbs 1992*c,* p. 29).

The most interesting program from the point of view of this paper is Pakistan's, because it has been alleged to involve the direct supply of explicit design knowledge from a previous program. U.S. officials have stated that the Chinese government handed over to Pakistan the detailed design of an atomic bomb, reportedly a uranium-implosion missile warhead that had been exploded successfully in a Chinese nuclear test in 1966. Despite this, Pakistan apparently found copying the weapon far from trivial: "It took the Pakistanis several years to master an implosion system, even though they were working from a proven design" (Albright and Hibbs 1991*a,* p. 19). One U.S. official reportedly commented that receiving a "cookbook design doesn't mean that you can make a cake on the first try" (Albright and Hibbs 1992*b,* p. 42).[16]

DISCUSSION

Tacit Knowledge

All three forms of evidence we have examined suggest that tacit knowledge plays a significant role in atomic bomb design. First, the task of the first atomic bomb designers at Los Alamos proved much harder than had been predicted on the basis of explicit nuclear physics knowledge. Filling gaps in the latter (such as, most consequentially, the rate of spontaneous neutron emission in plutonium 240) was important, but many of the most demanding challenges faced were practical, engineering ones. These

[16] In a private communication, one source has questioned the veracity of this account of the Pakistani program. Unfortunately, we lack the data to clarify matters further.

challenges were diverse enough to take their solution far beyond the capabilities of a single individual or even a small group: a large, complex organization had to be constructed to tackle them.

Second, despite the enormous subsequent work to make fully explicit the knowledge needed for nuclear weapons design, and, in particular to embody it in computer programs, current nuclear weapon designers still argue strongly that this explicit knowledge alone is inadequate. They emphasize the ways in which even the best computer models are only approximations to reality. They note the consequent need in their work for nonalgorithmic judgment, forged by working alongside experienced designers and by long personal involvement in design and, crucially, testing. That judgment is communal and hierarchical: proposals by individuals undergo peer review by senior colleagues. Furthermore, the production of nuclear weapons, as well as their design, requires tacit knowledge: it is not a matter simply of following explicit, algorithmic instructions. The designer's judgment has, for example, to be called upon in deciding whether two nuclear weapons produced to the "same design," can actually be treated as identical.

Third, the record of the spread of atomic weapons is at least broadly compatible with the conclusion that tacit knowledge is involved in their design. These weapons have been developed successfully at sites other than the original one, on at least three occasions (the Soviet Union, France, and China) apparently without extensive personal contact with a previously successful design effort.[17] However, these—and, indeed, the other—subsequent efforts have at least some of the characteristics of independent reinvention. All subsequent atomic weapon development efforts by other countries appear to have taken longer than the original American work (see table 1). The possession of explicit information (such as diagrams and detailed descriptions) generated by previous efforts has not eased their task dramatically, even where they were trying "simply" to construct a copy of a previous design. All development efforts about which details are known have had to struggle with a multiplicity of practical problems. As in the original development, their solution has required not merely individual expertise but concerted effort by large numbers of staff. The problems involved are sufficiently diverse that they require significant new work even when, as in the British case, it is possible to call on the knowledge and expertise of a number of individuals with direct experience of a previous successful program.

Of course, no individual aspect of this evidence is entirely compelling. First, while it is clear that explicit physics knowledge was inadequate

[17] Available data leave unclear the extent to which this is true of Israel, India, South Africa, and Pakistan; the British case is one of extensive previous personal contact.

for the original development of atomic weapons, it might still be that what was needed in addition was simply explicit knowledge from within the spheres of other disciplines, notably metallurgy and various branches of engineering. The historical record suggests that this was not the case, but because historical work on the topic has not been informed centrally by the issues addressed here, there is a degree of tentativeness to this conclusion. Furthermore, because the boundary between explicit and tacit knowledge shifts, with some aspects of tacit skills becoming systematized and even embodied in machines, one cannot simply extrapolate from the experience of the Manhattan Project (or other early development efforts) to the present day; we return to that issue below.

Second, as we have pointed out, the testimony of current designers may have been influenced by a desire to argue against a comprehensive test ban. Against this, we would note that the minority of members of nuclear weapons laboratories who favor such a ban do not deny the role of tacit knowledge (see, e.g., Kidder 1987, pp. 7–8; Mark 1988, pp. 40–41). Nor did the computer specialists from these laboratories whom we interviewed—who might be thought to have an interest in arguing for the adequacy of explicit, algorithmic knowledge—actually advance that argument; some, indeed, provided cogent grounds for regarding algorithmic knowledge as inadequate. It is true, however, that the experience of all but the oldest of our interviewees will have been with the design of sophisticated, rather than simple, weapons. This experience—particularly the experience of boosted primary designs that are "near the cliff"—is not necessarily generalizable to the design of simpler weapons.

Third, two issues are confounded in the record of the spread of nuclear weapons: the design of such weapons and the production of the necessary fissile material. With the exception of Livermore, which could call on the general U.S. stock of such material, all other nuclear weapons efforts so far have involved the production of fissile material as well as weapons design. The two activities have, hitherto, always been undertaken in parallel, and so we have no way of knowing how long design itself might have taken had the fissile materials been available from the start of a program. Furthermore, the time taken to design a nuclear weapon will clearly be influenced by the urgency with which the task is pursued, the resources devoted to it, and the equipment and skill available (see below). These considerations make the duration of the various development efforts a less than conclusive indicator of the "hardness" of the task and rule out any quantitative conclusion of the form "it takes x months or years to design a nuclear weapon."

Finally, the evidence suggests that the role of tacit knowledge may be significantly less in the design of a "secondary," to turn an atomic bomb into a hydrogen bomb, than in the atomic bomb itself. Our interviewees seemed more confident of the adequacy of explicit knowledge in understanding secondaries. Furthermore, the record of the spread of the hydrogen bomb is different from that of the atomic bomb: three of the four countries known subsequently to have moved from an atomic to a hydrogen bomb did so faster than the United States (see table 1).[18] The relevance of tacit knowledge may thus mainly be to the first step toward acquiring a nuclear arsenal.

Though important, none of these qualifications seem to us to be decisive. The weight of the evidence, we believe, supports the conclusion that tacit knowledge plays an important role in nuclear weapons design. Nevertheless, before moving to the implications of this conclusion, we need to discuss four further issues raised by these qualifications or by other considerations. The first is the possibility that the record of early nuclear weapons programs may be a poor guide to the future: because previously tacit knowledge has been made explicit; because that explicit knowledge is now available far more widely than it was in the 1940s or 1950s; and, especially, because many technologies relevant to nuclear weapons design and production are now "black boxed" (see Latour 1987).

Black Boxes

What previously had to be done by hand can now be done by machine, and those machines can simply be bought, rather than having to be built; much relevant information can be acquired simply by buying textbooks of nuclear physics and manuals of nuclear engineering; computer programs helpful to nuclear weapons design can, likewise, simply be purchased. That all of this has happened is undoubtedly true. The most obvious example of "black boxing" is that the development of digital computers, and their universal availability, mean that calculations that once had to be done by people (at major cost in time and effort) can now be done automatically. Indeed, it is now neither difficult nor expensive to pur-

[18] There are no detailed histories of the nature of the technical problems encountered in the various hydrogen bomb programs equivalent to those now available for the atomic bomb. The relative slowness of the U.S. effort may in part be accounted for by opposition to hydrogen bomb development; as noted below, there was also doubt about the feasibility of designs prior to the Teller-Ulam configuration. There has also been debate over whether the first claimed Soviet and British thermonuclear explosions deserve such a categorization. Thus "Joe 4," the Soviet test explosion on August 12, 1953, was not of a Teller-Ulam device, but of one with alternating layers of thermonuclear fuel and uranium 238 sandwiched between the high explosive and core of an implosion bomb (Holloway 1994, pp. 298, 307–8). In table 1, we class Joe 4 as a thermonuclear explosion but accept that there is a case for regarding the device as more akin to a boosted fission weapon. For Britain we follow Baylis (1994).

chase computers as fast as those of the U.S. nuclear weapons laboratories of the early 1970s (MacKenzie 1991), and a determined purchaser could acquire even more powerful machines, while probably being able to disguise their intended application. Nor would the programs to run on these machines have to be developed entirely from scratch: for example, derivatives of computer programs developed at the nuclear weapons laboratories have been commercialized and are widely available.[19]

Furthermore, a variety of other relevant black boxes that early weapons programs had to design and construct are now available commercially, although their purchase is currently more difficult and more likely to attract attention. These include specialized metallurgical equipment, diagnostic tools suitable for studying implosion and initiator behavior, and electrical and electronic equipment that could be used in detonation circuitry.[20]

That all this eases the task of nuclear weapons development is undeniable. Rather, the issue is how much it does so and whether it eliminates or minimizes the need for specialized tacit knowledge.[21] The Iraqi nuclear program—which has been dissected in unprecedented detail by international inspectors following Iraq's defeat in the 1991 Gulf War—serves as an experiment on precisely these points. It was a determined, high-priority, extremely well-resourced program conducted by a country with a relatively large scientifically and technically trained workforce and ample computing power. The Iraqi team had conducted a thorough and successful literature search for relevant explicit knowledge and had also obtained "weapons-relevant computer programs" (Albright and Hibbs 1992*a*, p. 33).

Some attempted purchases, particularly of precision electrical equipment for detonation circuitry, were intercepted. However, Iraq was able to buy much of what it needed from Western companies (especially Ger-

[19] The best-known example is DYNA3D, a Livermore-designed program to assist three-dimensional analysis of the response of mechanical structures to stresses and impacts (Allen 1991).

[20] Numerically controlled machine tools are sometimes included in such lists, but our impression is that the tolerances required for the fabrication of simple nuclear weapons are achievable with manually controlled precision machine tools commercially available (e.g., from Swiss suppliers) even in the late 1940s and 1950s. The availability of skilled machinists to operate such tools has, however, on occasion been a constraint, for example on the British program.

[21] In one sense, of course, tacit knowledge is required for the successful operation of all such equipment; Collins (1990) describes the tacit knowledge needed even to perform arithmetic on a pocket calculator. If, however, the relevant tacit knowledge is widely distributed—if, say, it is of a kind that any physics or engineering graduate might be expected to possess—then the need for it would not be a constraint on nuclear weapons development.

man, but also American, British, Italian, French, Swedish, and Japanese). Among the project's successful acquisitions were, for example, vacuum furnaces suitable for casting uranium, plasma-coating machines that can coat molds for uranium, an isostatic press suitable for making high-explosive lenses, and high-speed oscilloscopes and streak cameras useful for the experimental investigation of implosion (Milhollin 1992). Iraq was also markedly successful in making purchases and obtaining explicit knowledge relevant to the production of fissile materials and had enough spare electricity-generating capacity to support even the most energy-intensive route to uranium separation.

Yet the program, which seems to have begun in the mid-1970s, had still not been successful by 1991, and opinions vary on how close to success it was even then (Office of Technology Assessment 1993, pp. 150–51). One reason for its slow progress is specific. A 1981 Israeli bombing raid rendered inoperable the French-supplied nuclear reactor under construction at Osirak and shut off what may have been the Iraqi weapon program's intended source of plutonium (Spector 1987, p. 161). Iraq was, therefore, having to concentrate on what is generally agreed to be the considerably more demanding task of uranium separation.

More generally, though, "Iraq's nuclear Achilles heel" was its "lack of skilled personnel" (Albright and Hibbs 1991*a*, p. 16). This lack hampered both uranium separation—which never reached the necessary scale—and also nuclear weapons design.[22] According to seized documents, the Iraqis' immediate goal was an implosion weapon with a solid uranium core, beryllium/polonium initiator, a uranium 238 reflector, and iron tamper. Extensive theoretical studies had been carried out, and at least five different designs had been produced, which were, in the judgment of one leading U.S. weapons designer, David Dorn, "all primitive [but] each one an improvement over its predecessor" (Zimmerman 1993, pp. 11–12).

However, a final, settled, fully "practical design had not been achieved" (International Atomic Energy Agency, Action Team for Iraq 1994, p. 6). Despite all their purchases, the Iraqis had to develop much

[22] Clearly there is a possibility that achievements may successfully have been hidden from the inspectors (although, had Iraqi leaders believed the program to have been on the verge of producing a usable weapon, it is difficult to see why they did not postpone the invasion of Kuwait until it had done so). In general, the results of the inspections seem plausible. The inspectors were skilled, determined, persistent, and intrusive; they succeeded in finding not just physical artifacts but also extensive (and credible) documentary material on the program. Some of the latter was in the form of progress reports (see the report from the Al-Athir research establishment reproduced in English translation in Zimmerman 1993), and there is thus the opposite possibility that they may have been overoptimistic, as progress reports to sponsors often are.

requisite technology for themselves, relying on local competences in metallurgy, chemistry, and electronics (sources differ on their relative strengths in these fields). The same was true for knowledge of detonics. The Iraqi detonics program unquestionably benefited from explicit knowledge acquired from abroad, but extensive indigenous theoretical work and practical experimentation were still required. By 1991, this work had not yet reached the stage of testing a full three-dimensional implosion system (again, detailed assessments differ on how much further work was needed). Crucially, the Iraqi designers seem to have been constrained to use much less high explosive than in early American designs: unlike the latter, which were delivered to their targets by heavy bombers, the Iraqi design was probably meant to be carried by a Scud ballistic missile. Iraqi designers seem to have lacked confidence that, within that constraint, they could achieve a powerful, highly symmetrical implosion. As a result, the design of fissile core they were contemplating was far closer to criticality than Western experts believed wise, so much so that it could perhaps have been detonated by the results of a fire or minor accidental shock. "I wouldn't want to be around if it fell off the edge of this desk," said one inspector (Albright and Hibbs 1992*a,* pp. 31, 33, 35; Zimmerman 1993; Milhollin 1992, p. 33).

The Hardness of Tasks

The experience of the Iraqi program does seem to suggest, therefore, that tacit knowledge is still required successfully to make use of black box technology. While the latter can be purchased, the former cannot, unless skilled personnel can be recruited or sustained person-to-person contact achieved. The Iraqi program, along with all the other well-documented nuclear weapons programs (apart from the British), had only very limited success in this. Learning from previous programs has thus had to proceed without direct transfer of specific tacit skills.

This does not imply, however, that previous programs are a source of explicit knowledge alone; they can also convey useful information about the hardness of tasks involving tacit knowledge.[23] To put it at its most elementary, while observing others riding bicycles does not enable one to learn the skills of the cyclist, it nevertheless shows that cycling is possible. Knowing that older brothers or sisters have learned to ride can encourage younger siblings not to conclude from early failures that the task is impossibly hard.

Successful previous nuclear weapons programs have had analogous

[23] See the discussion of the "hardness" of surgery in Pinch et al. (in press).

consequences. The confidence—indeed overconfidence—of wartime Anglo-American physicists (including Continental refugees) in the ease of development of a nuclear weapon does not seem to have been widely shared by their French, German, or Soviet colleagues, and the governments of the last two countries were unconvinced prior to 1945 that the task was feasible enough to be worth the kind of resources the Americans devoted to it (see, e.g., Holloway 1981; Goldschmidt 1984, p. 24).[24] Trinity, Hiroshima, and Nagasaki were dramatic demonstrations that the task was not impossibly hard, and this proof (as well, of course, as the perceived threat to the Soviet Union) explains the sudden shift in the USSR in 1945 from a modest research effort to an all-out, top-priority program (Holloway 1981).

As we have seen, the British test explosion in 1952, although no threat to France, contributed to the latter's weapons program by suggesting that developing an atomic bomb was easier than had previously been assumed. Likewise, the Chinese explosion in 1964 showed other developing countries that the atomic bomb was not necessarily the preserve solely of the highly industrialized world. Furthermore, profound questions over the feasibility of early hydrogen bomb designs helped delay the American move from an atomic to a hydrogen bomb (Bethe 1982). By contrast, all subsequent hydrogen bomb programs could proceed with confidence in the basic achievability of their goal, and, in words used in another context by a group of weapons designers (Mark et al. 1987, p. 64), "The mere fact of knowing [something] is possible, even without knowing exactly how, [can] focus . . . attention and efforts."

Because of this, we need to qualify the inference from the role of tacit knowledge in nuclear weapons design to the possibility of uninvention. It is hard to imagine belief in the feasibility of atomic or thermonuclear weapons now disappearing, and that fact alone increases the probability of their reinvention. In addition, more was learned from the Manhattan Project (even by those without personal involvement) than simply the feasibility of an atomic bomb. It was openly disclosed in Smyth (1945) that the project had produced two fissile materials—plutonium and uranium 235—and it was in no meaningful sense a secret that both gun and implosion designs had been developed. The knowledge that both a uranium gun and a plutonium implosion weapon had worked meant that subsequent programs could save significant resources by focusing on only one fissile material and one design. It was, for example, of considerable

[24] The reasons for these divergent beliefs are unclear to us. Some of the physicists remaining in Germany may have allowed their view of the feasibility of an atomic bomb to be influenced by their desire to deny such a weapon to Hitler. The evidence for this hypothesis is, however, not compelling, and in any case the views of French and Soviet physicists cannot be accounted for in this way.

help to the early Soviet project to feel confident that it was safe to concentrate initially on plutonium production and not to have to embark simultaneously on an equally rapid program of uranium separation (Zaloga 1993).

Other Sources of Tacit Knowledge

Furthermore, previous nuclear weapons programs are not the only possible source of tacit knowledge needed for the design of a nuclear weapon. The most important other source is the civil nuclear power industry. The literature on proliferation treats this industry primarily as a potential source of fissile material, but it is also clearly a potential source of knowledge. In South Africa, for example, overseas cooperation in the development of civil nuclear power, while not directly aiding the nuclear weapons program, was nevertheless helpful in increasing "the technical competence of South Africa's nuclear engineers, scientists and technicians" (de Villiers, Jardine, and Reiss 1993, p. 105).

Civil nuclear power can provide crucial experience in matters such as the chemistry, metallurgy, handling, and machining of fissile materials and also in neutronics. This last field is the study of the behavior of neutrons in fissile materials. It is clearly crucial for the nuclear weapons designer, who will want to ensure that a bomb will explode rather than fizzle and also that a critical mass is not formed accidentally during the machining and assembly of fissile material. Designers of civil nuclear reactors, however, also use neutronics to find configurations that can be kept critical without becoming supercritical. Like nuclear weapons designers, they use a combination of physical theory, experimental results, and computer modeling. The two tasks are similar enough (Schaper 1993) that one would expect the explicit and tacit knowledge of neutronics gained in reactor design to help considerably in weapons design.[25]

Because nuclear weapons integrate nuclear and nonnuclear technologies, tacit knowledge acquired in some of the latter is also relevant. Electrical and electronic engineering, needed for the design and construction of detonation circuitry, is obviously important. Perhaps of greatest significance, however, is the field of detonics, and in particular the technology of achieving not simply explosions but blast waves of particular shapes.

This technology is central to the "art of implosion design" (McPhee

[25] Curiously, the historical record known to us contains no evidence that it *has* done so. This may be because, in the relatively well-documented programs, weapons design went on simultaneously with reactor design but was largely the province of distinct groups.

1974, p. 215) in nuclear weaponry. It is, however, also a technology with wider military uses, notably in the design of shaped charges for armor-piercing antitank weapons, as well as some civil applications, such as in diamond production, mining, and metallurgy (Schaper 1993). This field preexisted, and contributed to, the development of nuclear weapons. Los Alamos scientist James Tuck, who first suggested the use of explosive lenses, had previously worked in the United Kingdom on armor-piercing charges (Szasz 1992, p. 23). The leader of the Soviet atomic bomb project, Yuli Khariton, and his colleague Yakov Zeldovitch, had also done wartime work on detonation phenomena in chemical explosives (David Holloway, personal communication, February 15, 1994). Since the 1940s, detonics has developed into a sophisticated technical specialty. The largest concentrations of detonics expertise seem still to be in the nuclear weapons laboratories, but the technology is also practiced at a range of other military and civilian establishments, mainly in the industrialized countries.[26] If experienced personnel from such establishments are available, the design and testing of an atomic bomb implosion system would be eased significantly.

Kitchen Bombs

Despite the effects of these qualifications, all demonstrably successful efforts to develop nuclear weapons have to date still been major enterprises, involving several years of work, design teams numbering (at least in the cases where this information is available) from several hundred to 1,000 or more, as well as major industrial enterprises devoted to the production of fissile materials. These efforts have had themselves to acquire, often painstakingly, much of the knowledge and skills developed in the Manhattan Project or other previous efforts.

Perhaps, though, all these programs, with the possible exception of the South African,[27] have simply been unnecessarily ambitious and la-

[26] See, for example, the registration list at the Ninth International Symposium on Detonation (Portland, Oregon, August 28–September 1, 1989). It notes attendance by representatives from the Al Qaqaa State Establishment in Baghdad, a detonics research establishment at the heart of the Iraqi nuclear weapons program. We owe the reference to Schaper (1993).

[27] Unlike all previous programs for which the information is available, South Africa settled for the simpler of the Manhattan Project designs, the gun weapon, and seems to have concentrated innovative effort on developing a new uranium separation process. (The perceived relative difficulty of uranium separation compared with plutonium production, and the fact that gun designs are believed to require more fissile material and physically to be larger, probably account for other countries' eschewal of the gun.) Unfortunately, we have no information on how difficult the South African team found it to design their uranium gun.

borious. Certainly, all the well-documented efforts after 1945 seem to have seen their first atomic bomb as a stepping-stone to an eventually more sophisticated arsenal, for example one including hydrogen bombs. Therefore, as two members of the French program put it, "The goal was not simply to make a device explode, but to measure the parameters controlling nuclear explosive reactions" (Chevallier and Usunier 1984, p. 129; our translation). Even the Iraqi program was "grandiose" and "over-designed" from the point of view of simply producing a crude weapon (International Atomic Energy Agency, Action Team for Iraq 1994, p. 2).

Perhaps, therefore, the need for tacit knowledge and reinvention could be circumvented by a modest program aiming simply to produce crude weapons as quickly and easily as possible. This issue will be made much more pressing if substantial quantities of fissile materials become available for illicit purchase. Up to now, all nuclear weapons programs, with the partial exception of Israel's,[28] have had to produce their own fissile materials. Typically, this activity has dwarfed weapons design in its expense, visibility, and personnel and resource requirements. For example, the workforce building the nuclear reactors at Hanford numbered 45,000 at its peak; the uranium separation plant at Oak Ridge consumed more electricity in 1945 than the whole of Canada produced in World War II (Gowing and Arnold 1974, 2:76; Albright and Hibbs 1991*b*, p. 19). There was, therefore, no incentive to skimp on weapons design, and enormous effort was devoted to maximizing the chance that the first nuclear explosions would be successful. In 18 months of research, for example, over 20,000 high-explosive castings were supplied for test implosions, and many more were rejected as inferior. Their cost was unimportant, given that Los Alamos management knew the cost of producing the necessary plutonium to have been of the order of $1,000 million in 1940s dollars (Hoddeson et al. 1993, pp. 175, 297).

Were fissile materials to become available for illicit purchase, however, an aspirant nuclear weapons state, or even a terrorist group, might well decide to try a "quick and dirty" route to a nuclear weapon. Would they succeed? Twenty years ago, former Los Alamos designer Theodore Taylor sought to highlight the dangers of the diversion of fissile material, even in the forms in which it is commonly found in the civil nuclear power program. He argued passionately that, if the fissile material can

[28] In the mid-1960s Israel is believed to have obtained illicitly around 100 kilograms of enriched uranium from a privately owned plant in the United States, and there have also been allegations that Israel was supplied with plutonium by France (Spector 1987, p. 131). However, Israel also had substantial plutonium-production capability from its French-supplied reactor at Dimona, which became operational in 1962 (Hersh 1991, p. 119).

be obtained, a crude but workable nuclear weapon could be made using only readily available instruments, artifacts, and knowledge. His arguments were brought to wide public attention by the doyen of American reporting, John McPhee (1974).

Taylor argued, for example, that the weapon's reflector could be built by soldering together, around its fissile core, two stainless steel kitchen mixing bowls and lining them with wax. Modern plastic exlosive need not be cast but, like putty, could be "kneaded and formed, by hand" around the mixing bowls or perhaps on an "upturned salad bowl." The work could be done first by eye, then by poking a measured wire "into the high explosive until it hits the reflector." An initiator might not be necessary at all: what is normally thought of as the disadvantage of "reactor grade" plutonium (its high level of plutonium 240 and thus high rate of spontaneous neutron emission) could be turned to advantage by doing away with the need for this traditionally troublesome component.[29] Nor, according to Taylor, need implosion circuitry and detonators necessarily go beyond what is commercially available: "If you don't care whether you get a tenth of a kiloton [of explosive yield] one time and five kilotons another time, you can be much less fussy about the way the high explosive is detonated" (McPhee 1974, pp. 214–18).

This suggested way of proceeding is both hypothetical—no one is known to have tried to build a bomb in this way[30]—and controversial. For example, Edward Teller (1993, p. 33) peremptorily dismisses as a "myth" the idea that "a nuclear explosive could be secretly developed and completed in someone's garage"; other sources offer more particular counterarguments.[31] Furthermore, Taylor has more recently put his

[29] In standard usage, "reactor-grade" plutonium contains, as well as plutonium 239, upward of 18% plutonium 240 (and, indeed, significant quantities of other isotopes as well), while "weapons-grade" plutonium contains less than 7%; see, e.g., Albright, Berkhout, and Walker (1993). There has, however, been at least one successful test (in 1962) of a nuclear device constructed out of "reactor-grade" plutonium (Office of Technology Assessment 1993, p. 133; Norton-Taylor 1994). The design and fabrication of such a weapon is, nevertheless, seen as harder than working with weapons-grade plutonium (Locke 1992; Mark 1993). There is believed to be a greater risk of a fizzle, and reactor-grade plutonium is a significantly more powerful heat source than weapons-grade, which can cause problems when it is enclosed within a shell of high explosive (which is a thermal insulator).

[30] Taylor suggested that "Los Alamos or Livermore [should] build and detonate a crude, coarse, unclassified nuclear bomb—unclassified in that nothing done in the bomb's fabrication would draw on knowledge that is secret" (McPhee 1974, p. 123). To our knowledge, that suggestion was not taken up.

[31] One source pointed out to us in a personal communication that relatively small amounts of high explosive, in an appropriately shaped charge, can blow a hole through armor plate. Plutonium would be significantly less resistant, so a misjudged implosion design could easily simply blow the fissile mass apart. See also n. 29 on the difficulties of using reactor-grade plutonium.

name to a less alarming diagnosis (Mark et al. 1987).[32] This analysis still concludes that a terrorist group which had acquired fissile material *could* construct a nuclear weapon but places greater emphasis on the barriers—especially of knowledge and skill—the group would encounter.

Thus Mark et al. (1987, p. 58) argue that the necessary detailed design would require "the direct participation of individuals thoroughly informed in several quite distinct areas: the physical, chemical and metallurgical properties of the various materials to be used, as well as the characteristics affecting their fabrication; neutronic properties; radiation effects, both nuclear and biological; technology concerning high explosives and/or chemical propellants; some hydrodynamics; electrical circuitry; and others." Nor would explicit knowledge alone be enough: "The necessary chemical operations, as well as the methods of casting and machining the nuclear materials, can be (and have been) described in a straightforward manner, but their conduct is most unlikely to proceed smoothly unless in the hands of someone with experience in the particular techniques involved, and even then substantial problems could arise" (p. 60).[33] We *hope* that this later account conveys the difficulties better than the earlier one, but—with, fortunately, no direct empirical evidence yet available—there is no way to be sure. The feasibility of a low-skill route to a crude nuclear weapon cannot, therefore, entirely be ruled out.

Uninventing the Bomb

There are thus at least three reasons not to overstate the extent to which lack of tacit knowledge would force full-scale reinvention of nuclear weapons even after a long hiatus in their development. Knowing the task is feasible would encourage and focus efforts, relevant tacit knowledge might be available from sources other than previous nuclear weapons programs, and the elaborate development path of currently existing programs might conceivably be avoided. If nuclear weapons are to be uninvented, therefore, we have to add at least two elements to a "tacit knowledge" view of uninvention.

The first is familiar: control over fissile materials, the key component of the current nonproliferation regime and one that clearly needs urgent reinforcement. The second is what actor-network theorists would call the

[32] The only point of detail on which he apparently changed his mind is a radical upward revision of the quantities needed if a bomb is to be built with "plutonium oxide powder seized from a fuel fabrication plant" (Mark et al. 1987, p. 61). The overall tone of the later piece is, nevertheless, different.

[33] This passage assumes the fissile material to be available as oxide, rather than as metal. If the latter were the case, the need for chemical knowledge and skill is greatly reduced, but the demands of casting and machining remain.

"translation" of interests: the displacement of goals, invention of new goals, the creation of new social groups, and the like. To date, these theorists have looked primarily at this as a part of the process of invention (e.g., Latour 1987, pp. 108–32). However, it must surely be part of uninvention as well. Physicist Wolfgang Panofsky, is, unfortunately, right when he says "ultimately, we can keep nuclear weapons from multiplying only if we can persuade nations that their national security is better served without these weapons" (Panofsky 1994, pp. 67–68). However, verbal persuasion alone is unlikely to be enough. Actor-network research on the translation of interests might well form a useful body of resources for addressing this issue (for some relevant considerations, see Flank [1993/94]).

Issues of tacit knowledge, control over materials, and the translation of interests form, it seems to us, a necessary three-sided approach to nonproliferation and uninvention. To date, public policy has tended to focus on the second of these alone, perhaps because of its physical concreteness. We believe the first and third also need to be taken seriously.

In particular, despite all the reservations we have expressed, we feel that considerations of tacit knowledge (largely neglected hitherto because of the dominance of conventional images of science and technology) could be important to disarmament and nonproliferation. Successful nuclear weapons design, we have been arguing, depends not only on explicit knowledge and algorithmic instructions but also on tacit knowledge gained through processes such as attempts to fabricate real systems and trial-and-error experimentation with their components, processes that take time and effort. The requirement for tacit knowledge thus serves as the equivalent of friction in a physical system: slowing things down and perhaps adding a degree of stability to what might otherwise be unstable situations. For example, after a sufficiently long hiatus, we would expect the effort needed to re-create nuclear arsenals to become quite considerable, even for those who possessed detailed documentary records from their original development. Especially if fissile materials have to be produced afresh, it begins to be imaginable that, in a world with open skies, open borders, and dedicated and sophisticated intelligence agencies, such reinvention efforts would be detectable some time before they came to fruition.[34]

[34] The recent South African and Iraqi inspections and disclosures allow us retrospectively to assess the accuracy of intelligence estimates of the progress of these programs. The broad accuracy of quantitative estimates of the likely status of the South African program (as reported, e.g., in Spector 1987, pp. 220–39) have been confirmed (International Atomic Energy Agency 1993). It appears that in the late 1980s U.S. intelligence agencies underestimated the seriousness of Iraq's nuclear ambitions, but there was nevertheless ample evidence of the program's existence (see Spector 1987, 161–63). Of course, the political arrangements that would permit appropriate action to be taken

More generally, attention to tacit knowledge (and to its possible loss) can help counter the pessimism that can be engendered by the conventional view that nuclear weapons cannot be uninvented. We do not pretend even to have begun to sketch how an abandonment of nuclear weapons might be made durable and permanent (nor, as noted earlier, have we discussed its desirability). Nevertheless, we hope to have contributed to undermining one of the key barriers to starting to think about its possibility.[35]

An Accidental Uninvention?

A world in which the uninvention of nuclear weapons is pursued systematically, however, may well seem utopian. The maintenance of nuclear arsenals by the existing nuclear powers, in continuing uneasy conjunction with attempts to restrain their proliferation, seems more likely. That world has at least the virtue of apparent familiarity, barring a sudden multiplication of attempts to develop nuclear weapons capabilities, triggered by failure to extend the Nuclear Non-Proliferation Treaty (international review of which began in spring 1995) or by the breakdown of control over fissile materials.

As the word's etymology reminds us, however, a technology does not consist simply of artifacts but also of knowledge and understanding of those artifacts. The reader familiar with the sociological studies of controversial scientific experiments (e.g., Collins 1985) will have noted a crucial difference between them and the situation, hitherto, of nuclear weaponry. In the former, there is typically dispute as to what the correct substantive result of an experiment "should be," but, because experiment cannot be reduced to algorithmic procedures, there is no other ultimate test of its competence. Substantive divides (over matters such as the existence of controversial physical phenomena) thus become utterly entangled with disagreements over experimenters' competence.

In nuclear weaponry, in contrast, there has seldom been doubt over what constitutes the success of a nuclear explosive test.[36] Such debate

---

to prevent programs coming to fruition are quite another matter, one we cannot discuss here.

[35] Among the few concrete discussions of the feasibility of abandoning nuclear weapons are Schell (1984) and Karp (1992).

[36] There has, on the other hand, been much debate as to what a *test* consists of: for example, over the precise threshold of nuclear yield above which an event becomes a (detectable) nuclear test. This is a practical matter of great importance both to the formulation of a nuclear test ban and to its consequences for knowledge of nuclear weaponry, but space prevents us discussing it here.

is imaginable,[37] but controversies fully akin to those "classical" in the sociology of science have actually taken place only occasionally: when a putative nuclear explosion has been "observed" only at a distance,[38] or where particular, controversial, nuclear phenomena are concerned.[39] Nuclear testing, therefore, has placed an impersonal constraint on nuclear weapons design that, as we have seen, those involved have valued highly. There has been a great deal of room for arguing over why a particular test failed; and at least the wiser designers knew that a successful test did not ipso facto demonstrate the correctness of their knowledge. Over the fact of success or failure, however, the practical room for argument has been less.

Nuclear explosive testing may, however, soon be at an end; by 1994, the only country still conducting nuclear tests was China. Even if a nuclear test ban were not believed desirable (it is a goal of the current U.S. and Russian administrations), it may well be the minimum quid pro quo that the existing nuclear weapons states have to pay in order to secure extension of the nonproliferation treaty.[40]

[37] For example, events classed as "fizzles" can be quite substantial explosions, the equivalents of tens or even hundreds of tons of high explosive.

[38] Recordings by a U.S. nuclear detection satellite suggested two extremely bright flashes of light in rapid succession over the south Indian Ocean on September 22, 1979, a pattern generally regarded as characteristic of first the initial detonation and then the fireball of a nuclear explosion. However, no other evidence (seismological, radiological, etc.) of an explosion could be found. A U.S. government–appointed group of outside experts, chaired by Jack Ruina of the Massachusetts Institute of Technology, concluded that the twin peaks in the satellite data were probably not the result of a nuclear explosion and may, indeed, not have represented genuine external flashes of light. However, some members of the U.S. Nuclear Intelligence Panel (notably those from Los Alamos) remained convinced that, to quote former Los Alamos Director Harold Agnew, "If it looks like a duck, it's got to be a duck" (Hersh 1991, p. 280). There was intense speculation that there had been a nuclear test by either South Africa or Israel, or possibly both nations in concert; see Hersh (1991, pp. 271–83), who also quotes anonymous Israeli interviewees claiming that Israel had indeed conducted a small series of covert tests. To our knowledge, no definitive resolution of the controversy has been reached.

[39] The clearest instance is the controversy that took place in the United States in the 1980s over whether nuclear tests intended to demonstrate substantial x-ray lasing had indeed done so, or whether positive results were artifacts of the instrumentation employed (see Broad 1992).

[40] The 1993 Energy and Water Development Appropriations Act, passed in September 1992, prohibits U.S. nuclear testing after September 30, 1996, unless another state conducts a test after that date. It also banned U.S. nuclear testing until July 1993, and subsequent presidential decisions have extended this moratorium until September 1996 (Harvey and Michalowski 1994). Current international negotiations over a test ban hinge on the precise definition of "testing" and on the date of such a ban. France and China wish the ban to be postponed to 1996, China wishes exemption for "peaceful" nuclear explosions, and until recently France and Britain were seeking exemption for "safety tests."

If there is a test ban, and if nuclear weapon design continues after it, that design will inevitably involve much greater reliance on computer modeling.[41] Older interviewees in the U.S. laboratories recalled for us the three-year voluntary test moratorium that began in 1958. During that period, dependence on computer programs and subjective confidence in their output increased, especially as, toward the end of the moratorium, some senior staff left: "You start[ed] to *believe* your calculations, and young folks *really* believe them if the old timers have left" (McDonald 1990); "People start[ed] to believe the codes are absolutely true, to lose touch with reality" (Sewell 1990). This confidence then evaporated after the moratorium's end. Crucial was the appearance of doubt about the validity of the modeling of the effects of radioactive decay of the tritium used in boosting. An underground nuclear test to investigate these aging effects was commissioned: "The test showed that these effects had been so severely underestimated that a cloud of then unknown proportions immediately fell over many of our weapons" (Westervelt 1979, p. 62).

The increase, following a test ban, in subjective confidence in computer modeling would almost certainly be much greater nowadays, given the much more refined computer codes and more powerful computers that now exist and, especially, the modern capacity to display simulations visually using "realistic" computer graphics. While those involved believe that the particular phenomena that caused problems after the earlier moratorium are now well understood, they also acknowledge that "weapons of that era were considered 'forgiving' relative to their more modern counterparts" (Westervelt 1979, p. 62).

The consequences of the continued pursuit of nuclear weapons design after a test ban is a topic that deeply concerns some of the current generation of nuclear weapons designers. To mark the fiftieth anniversary of its founding, the Los Alamos laboratory in 1993 gathered together a panel of 22 leading current and retired members to discuss its future. Worries about the atrophying of designers' judgment were prominent in their discussion. Said one, "We'll find far too many people who are willing to certify new or modified nuclear weapons based on very little data, or maybe no data." "The scary part," said another, "is that there will be no shortage of people who are willing to certify untested weapons, especially if they are certifying their own designs, or if they want to please someone in Washington." He went on, "If the laboratories cannot con-

[41] In the United States at least, there has already been considerable investment in sophisticated facilities to expand the range of relevant "hydronuclear" and nonnuclear experiments that are possible (see, e.g., Goldstone 1993; Neal 1993). However, those involved do not yet see such experiments as the full equivalent of nuclear explosive testing.

duct tests, the United States should consider the possibility of eliminating its capability to design and certify nuclear weapons" (Agnew et al. 1993, p. 9).

It is surprising to hear that possibility aired in the establishment that first gave the United States, and indeed the world, the capability whose elimination was being discussed. The record of the discussion, however, reveals no voice raised in dissent. Nor, indeed, would it necessarily be as radical a move as it sounds. Not only has the military situation changed and budgetary constraints grown, but parts of the nuclear weapons production infrastructure in both the United States and Russia are now either closed or in a dangerous condition.[42] Add to these a possible ban on testing, and it is far from clear that the governments of the major nuclear weapons states will in practice commission new types of nuclear weapon in the foreseeable future. In the United Kingdom, for example, it seems probable that the Trident warhead, now about to enter service, will be the last nuclear weapons develop program, at least for a generation.

The nuclear weapons laboratories may well, therefore, face a future in which they are no longer developers of new weapons but custodians of past ones, quite possibly past ones they are unable to subject to nuclear explosive tests. Custodianship may sound like an unproblematic task, but here again questions arise about tacit knowledge and the sameness of artifacts. Even if no new designs are ever introduced, the remaining arsenal will, as noted above, change anyway: through radioactive decay and other processes of aging as well as through maintenance and the replacement of aged components. As designers themselves age, leave, and die, the number who have first-hand experience of development through the point of full nuclear testing will steadily diminish; yet they will have to decide whether the inevitable changes in the arsenal matter. In such a situation, will explicit knowledge be enough? Will tacit knowledge and judgment survive adequately? For how long?

Another senior figure at Los Alamos asks, "Imagine, twenty years from now, a stockpile-surveillance team noticing that one of the weapons being stored has changed in appearance. They will want to know, 'Is this still safe, and would it work if needed?' They will call the Laboratory and ask the experts regarding this weapon. Will they be able to rely on the answer they get?" (Goldstone 1993, p. 52). We do not claim to be

[42] Nuclear warhead production in the United States was suspended early in 1992 following health and safety concerns about the Rocky Flats plant near Denver, Colorado, where the plutonium components for nuclear weapons are fabricated. According to Harvey and Michalowski (1994, p. 286), "prospects are dim for a return to operations in the near term" at Rocky Flats.

able to answer Goldstone's question, which becomes even more pointed if the time span considered stretches from 20 years to more than a generation. That it can be asked, however, is an indicator that, even without disarmament, the nuclear future may in at least one respect be quite different from the past. Hitherto, nuclear weapons have been deeply controversial morally and politically, but the cognitive authority of the nuclear weapons designer has seldom been questioned. If, in the years to come, some untoward event, such as a serious nuclear weapons accident (Sagan 1993), were to generate large-scale public concern, then we would suggest that the challenge to that cognitive authority may well be profound and its consequences major.[43]

APPENDIX A

List of Interviews

Los Alamos National Laboratory, New Mexico: Current and Retired Staff

Harold Agnew, December 13, 1991; Ira Akins, Robert Frank, Roger Lazarus, and Bill Spack, April 12, 1989; Delmar Bergen, December 18, 1990; Bob Carr, December 18, 1990; Tom Dowler, April 12, 1989; Tom Dowler and Thurman Talley, December 18, 1990; Robert Glasser, December 16, 1991; John Hopkins, December 10, 1991; Harry Hoyt, March 14, 1990; Jim Jackson, April 12, 1989; Steve Maarenen, December 18, 1990; J. Carson Mark, March 15, 1990 and December 10, 1991; Norman Morse, April 12, 1989; Robert Osbourne, November 20, 1991; Raemer Schreiber, December 10, 1991; Thurman L. Talley, April 12, 1989; Don Westervelt, December 18, 1990; Roy Woodruff, December 11, 1991.

Lawrence Livermore National Laboratory, California: Current and Retired Staff

Roger E. Anderson and George A. Michael, April 13, 1989; Roger E. Anderson, Norman Hardy, Cecil E. Leith, Jr., William A. Lokke, V. William Masson, George A. Michael, and Jack Russ, April 13, 1989; Roger Batzel, December 4, 1991; James Carothers, December 10, 1990; Hugh DeWitt, December 12, 1990; Sidney Fernbach, Tad Kishi, Francis

[43] For example, Harvey and Michalowski (1994, p. 279) argue that a serious accident to the Trident program "would almost certainly result in [its] extended suspension or termination," a measure that would radically change the strategic landscape, since Trident warheads will probably soon form half of the U.S. strategic arsenal and the entirety of the British one. On the wider vulnerability to challenge of scientific expertise, see, e.g., Barnes and Edge (1982, pp. 233–335).

H. McMahon, George A. Michael, and Harry Nelson, October 16, 1989; Norman Hardy and George A. Michael, April 14, 1989; John Harvey, April 3, 1990; Carl Haussmann, December 10, 1990; Art Hudgins, December 10, 1990; Ray Kidder, December 6, 1991; Michael May, April 2, 1990; Charles McDonald, December 10, 1990; Francis H. McMahon, October 16, 1989; George A. Michael, October 15 and 16, 1989; George Miller, December 10, 1990; Peter Moulthrop, December 10, 1990; Milo Nordyke, December 10, 1990; Duane Sewell, December 10, 1990; Edward Teller, March 24, 1990; Lowell Wood, October 16, 1989; Lawrence Woodruff, December 10, 1990.

In addition, we interviewed two retired staff from the U.K. Atomic Weapons Establishment, Aldermaston, and a small number of people (not weapons laboratory staff) spoke to us on a nonattributable basis; comments from the latter are referenced as anonymous private communications.

We do not claim representativeness for our sample, which was constructed by "snowballing" from laboratory members who were well-known in the outside world. Indeed, the sample is clearly biased toward more senior figures, both by the way it was constructed and by the need for interviewees to possess the confidence and experience to embark upon unclassified discussions that might stray onto sensitive matters. Unfortunately, undertakings given to our interviewees mean that we cannot make transcripts or interview notes available to other scholars.

REFERENCES

Agnew, Harold, et al. 1993. "Taking on the Future." *Los Alamos Science,* no. 21: pp. 4–28.

Albright, David, and Mark Hibbs. 1991*a*. "Iraq and the Bomb: Were They Even Close?" *Bulletin of the Atomic Scientists* (March): 16–25.

———. 1991*b*. "Iraq's Nuclear Hide-and-Seek." *Bulletin of the Atomic Scientists* (September): 14–23.

———. 1992*a*. "Iraq's Bomb: Blueprints and Artifacts." *Bulletin of the Atomic Scientists* (January/February): 30–40.

———. 1992*b*. "Pakistan's Bomb: Out of the Closet." *Bulletin of the Atomic Scientists* (July/August): 38–43.

———. 1992*c*. "India's Silent Bomb." *Bulletin of the Atomic Scientists* (September): 27–31.

Albright, David, Frans Berkhout, and William Walker. 1993. *World Inventory of Plutonium and Highly Enriched Uranium, 1992*. Oxford: Oxford University Press.

Allen, David. 1991. "How to Build a Better Beer Can: DYNA Does It All, from DOE to R&D." *Supercomputing Review* (March): 32–36.

Arrow, Kenneth J. 1962. "The Economic Implications of Learning by Doing." *Review of Economic Studies* 29:155–73.

Barnaby, Frank. 1994. "Red Mercury: Is There a Pure-Fusion Bomb for Sale?" *International Defense Review* 6:79–81.

Barnes, Barry. 1982. "On the Implications of a Body of Knowledge." *Knowledge: Creation, Diffusion, Utilization* 4:95–110.

Barnes, Barry, and David Edge, eds. 1982. *Science in Context: Readings in the Sociology of Science*. Milton Keynes: Open University Press, 1982.

Baylis, John. 1994. "The Development of Britain's Thermonuclear Capability, 1954–61: Myth or Reality?" *Contemporary Record* 8:159–74.

Bergen, Delmar. 1990. Interview by G. Spinardi. Los Alamos, N. Mex., December 18.

Bethe, Hans. 1982. "Comments on the History of the H-Bomb." *Los Alamos Science* (Fall): 43–53.

Bradbury, Norris. (1954) 1983. Press statement to *Santa Fe New Mexican,* September 24. Reprinted in *Los Alamos Science* 4 (7): 27–28.

Broad, William J. 1992. *Teller's War*. New York: Simon & Schuster.

Buchalet, Albert. 1984. "Les Premières Étapes (1955–1960)." Pp. 39–58 in *L'Aventure de la bombe,* edited by Institut Charles-de-Gaulle. Paris: Plon.

Burns, Tom. 1969. "Models, Images and Myths." Pp. 11–23 in *Factors in the Transfer of Technology,* edited by William H. Gruber and Donald G. Marquis. Cambridge, Mass.: MIT Press.

Callon, Michel. 1994. "Is Science a Public Good?" *Science, Technology and Human Values* 19:395–424.

Cathcart, Brian. 1994. *Test of Greatness: Britain's Struggle for the Atom Bomb*. London: Murray.

Chevallier, Jacques, and Pierre Usunier. 1984. "La Mise en œuvre scientifique et technique." Pp. 125–43 in *L'Aventure de la bombe,* edited by Institut Charles-de-Gaulle. Paris: Plon.

Cochran, Thomas B., William M. Arkin, Robert S. Norris, and Milton M. Hoenig. 1987. *Nuclear Weapons Databook*. Vol. 2: *U.S. Nuclear Warhead Production*. Cambridge, Mass.: Ballinger.

Collina, Tom Z., and Ray E. Kidder. 1994. "Shopping Spree Softens Test-Ban Sorrows." *Bulletin of the Atomic Scientists* (July/August): 23–29.

Collins, H. M. 1974. "The TEA Set: Tacit Knowledge and Scientific Networks." *Science Studies* 4:165–86.

———. 1975. "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics." *Sociology* 9:205–24.

———. 1981. "The Place of the 'Core-Set' in Modern Science: Social Contingency with Methodological Propriety in Science." *History of Science* 19:6–19.

———. 1985. *Changing Order: Replication and Induction in Scientific Practice*. London: Sage.

———. 1990. *Artificial Experts: Social Knowledge and Intelligent Machines*. Cambridge, Mass.: MIT Press.

Der Spiegel. 1994. "Schock am Flughaven." (August 15): 18–20.

De Villiers, J. W., Roger Jardine, and Michell Reiss. 1993. "Why South Africa Gave Up the Bomb." *Foreign Affairs* 72:98–109.

DeVolpi, A., G. E. Marsh, T. A. Postol, and G. S. Stanford. 1981. *Born Secret: The H-Bomb, the "Progressive" Case and National Security*. New York: Pergamon.

DeWitt, Hugh. 1990. Interview by G. Spinardi. Berkeley, Calif., December 12.

Dowler, Tom, and Thurman L. Talley. 1990. Interview by G. Spinardi. Los Alamos, N. Mex., December 18.

Dreyfus, Hubert L. 1979. *What Computers Can't Do: The Limits of Artificial Intelligence*. New York: Harper & Row.

Ferguson, Eugene S. 1977. "The Mind's Eye: Nonverbal Thought in Technology." *Science* 197 (26 August): 827–36.

———. 1992. *Engineering and the Mind's Eye*. Cambridge, Mass.: MIT Press.

Feynman, Richard P. 1980. "Los Alamos from Below." Pp. 105–32 in *Reminiscences of Los Alamos, 1943–1945,* edited by L. Badash, J. Hirschfelder, and H. Broida. Dordrecht: Reidel.

Flank, Steven. 1993/94. "Exploding the Black Box: The Historical Sociology of Nuclear Proliferation." *Security Studies* 3:259–94.

Friendly, Albert. 1946. "Atom Control Fight Centers on Secrecy." *Washington Post* (25 March): 3.

Goldschmidt, Bertrand. 1984. "La Genèse et l'héritage." Pp. 23–37 in *L'Aventure de la bombe,* edited by Institut Charles-de-Gaulle. Paris: Plon.

Goldstone, Philip D. 1993. "An Expanding Role for AGEX: Above-Ground Experiments for Nuclear Weapons Physics." *Los Alamos Science,* no. 21: 52–53.

Gowing, Margaret. 1964. *Britain and Atomic Energy, 1939–45*. London: Macmillan.

Gowing, Margaret, assisted by Lorna Arnold. 1974. *Independence and Deterrence: Britain and Atomic Energy, 1945–52*. London: Macmillan.

Gusterson, Hugh. 1991*a*. *Testing Times: A Nuclear Weapons Laboratory at the End of the Cold War*. Ph.D. thesis, Stanford University.

———. 1991*b*. "Keep Building Those Bombs." *New Scientist* (October 12): 30–33.

———. 1992. "Coming of Age in a Weapons Lab: Culture, Tradition and Change in the House of the Bomb." *Sciences* 32 (3): 16–22.

———. 1993. "Exploding Anthropology's Canon in the World of the Bomb." *Journal of Contemporary Ethnography* 22:59–79.

Hansen, Chuck. 1988. *U.S. Nuclear Weapons: The Secret History*. Arlington, Tex.: Aerofax.

Harvard Nuclear Study Group [Albert Carnesdale et al.]. 1983. *Living with Nuclear Weapons*. New York: Bantam.

Harvey, John R., and Stefan Michalowski. 1994. "Nuclear Weapons Safety: The Case of Trident." *Science and Global Security* 4:261–337.

Haussmann, Carl. 1990. Interview by G. Spinardi. Livermore, Calif., December 10.

Hawkins, David. 1946. *Manhattan District History. Project Y: The Los Alamos Project*. Vol. 1: *Inception until August 1945*. Declassified version released December 1, 1961. LAMS-2532. Los Alamos, N. Mex.: Los Alamos Scientific Laboratory.

Heritage, John. 1984. *Garfinkel and Ethnomethodology*. Cambridge: Polity.

Hersh, Seymour M. 1991. *The Samson Option: Israel, America and the Bomb*. London: Faber & Faber.

Hewlett, Richard G., and Oscar E. Anderson, Jr. 1962. *The New World, 1939/1946*. Vol. 1 of *A History of the United States Atomic Energy Commission*. University Park: Pennsylvania State University Press.

Hewlett, Richard G., and Francis Duncan. 1969. *Atomic Shield, 1947/1952*. Vol. 2 of *A History of the United States Atomic Energy Commission*. University Park: Pennsylvania State University Press.

Hibbs, Mark. 1993. "'Red Mercury' Is Lithium-6, Russian Weaponsmiths Say." *Nucleonics Week* (July 22): 10.

Hoddeson, Lillian, Paul W. Hendriksen, Roger A. Meade, and Catherine Westfall. 1993. *Critical Assembly: A Technical History of Los Alamos during the Oppenheimer Years*. Cambridge: Cambridge University Press.

Holloway, David. 1981 "Entering the Nuclear Arms Race: The Soviet Decision to Build the Atomic Bomb, 1939–1945." *Social Studies of Science* 11:159–97.

———. 1994. *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939–1956*. New Haven, Conn.: Yale University Press.

Hoyt, Harry. 1990. Interview by G. Spinardi. Los Alamos, N. Mex., March 14.

Hudgins, Art. 1990. Interview by G. Spinardi. Livermore, Calif., December 10.

Institut Charles-de-Gaulle, ed. 1984. *L'Aventure de la bombe: De Gaulle et la dissuasion nucléaire (1958–1969)*. Paris: Plon.

International Atomic Energy Agency. 1993. *The Denuclearization of Africa: Report by the Director General.* GC(XXXVII)/1075. Vienna: International Atomic Energy Agency.

International Atomic Energy Agency Action Team for Iraq. 1994. *Fact Sheet.* Vienna: International Atomic Energy Agency, January.

Joffe, Josef. 1994. "Nuclear Black Market: Much Ado about Not Much." *International Herald Tribune* (August 26): 6.

Karp, Regina C. 1992. *Security without Nuclear Weapons? Different Perspectives on Non-Nuclear Security.* Oxford: Oxford University Press.

Khariton, Yuli, and Yuri Smirnov. 1993. "The Khariton Version." *Bulletin of the Atomic Scientists* (May): 20–31.

Kidder, R. E. 1987. *Maintaining the U.S. Stockpile of Nuclear Weapons during a Low-Threshold or Comprehensive Test Ban.* UCRL-53820. Livermore, Calif.: Lawrence Livermore National Laboratory.

Knorr-Cetina, Karin. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science.* Oxford: Pergamon.

———. 1991. "Epistemic Cultures: Forms of Reason in Science." *History of Political Economy* 23:105–22.

———. 1992. "The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science." Pp. 113–38 in *Science as Practice and Culture,* edited by A. Pickering. Chicago: University of Chicago Press.

Knorr-Cetina, Karin, and Michael Mulkay, eds. 1983. *Science Observed: Perspectives on the Social Study of Science.* London: Sage.

Kuhn, Thomas S. 1970. *The Structure of Scientific Revolutions,* 2d ed. Chicago: University of Chicago Press.

Latour, Bruno. 1986. "Visualization and Cognition: Thinking with Eyes and Hands." *Knowledge and Society* 6:1–40.

———. 1987. *Science in Action: How to Follow Scientists and Engineers through Society.* Milton Keynes: Open University Press.

Lewis, John W., and Xue Litai. 1988. *China Builds the Bomb.* Stanford, Calif.: Stanford University Press.

Locke, Gerhard. 1992. "Why Reactor-Grade Plutonium Is No Nuclear Explosive Suitable for Military Devices." Paper prepared for the Workshop on the Disposition of Plutonium, Bonn, June 13–16.

Lynch, Michael. 1985. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* London: Routledge & Kegan Paul.

MacKenzie, Donald. 1991. "The Influence of the Los Alamos and Livermore National Laboratories on the Development of Supercomputing." *Annals of the History of Computing* 13:179–201.

Manley, John H. 1980. "A New Laboratory Is Born." Pp. 21–40 in *Reminiscences of Los Alamos, 1943–1945,* edited by L. Badash, J. Hirschfelder, and H. Broida. Dordrecht: Reidel.

Mark, J. Carson. 1988. "The Purpose of Nuclear Test Explosions." Pp. 31–42 in *Nuclear Weapon Tests: Prohibition or Limitation,* edited by Jozef Goldblat and David Cox. Oxford: Oxford University Press.

———. 1991. Interview by G. Spinardi. Los Alamos, N. Mex., December 10.

———. 1993. "Explosive Properties of Reactor-Grade Plutonium." *Science and Global Security* 4:11–38.

Mark, J. Carson, Theodore Taylor, Eugene Eyster, William Maraman, and Jacob Wechsler. 1987. "Can Terrorists Build Nuclear Weapons?" Pp. 55–65 in *Preventing Nuclear Terrorism,* edited by Paul Leventhal and Yonah Alexander. Lexington, Mass.: Lexington Books.

McDonald, Charles. 1990. Interview by G. Spinardi. Livermore, Calif., December 10.

McPhee, John. 1974. *The Curve of Binding Energy*. New York: Farrar, Straus & Giroux.

Metropolis, Nicholas, and E. C. Nelson. 1982. "Early Computing at Los Alamos." *Annals of the History of Computing* 4:348–57.

Milhollin, Gary. 1992. "Building Saddam Hussein's Bomb." *New York Times Magazine* (March 8): 30–36.

Miller, George. 1990. Interview by G. Spinardi. Livermore, Calif., December 10.

Miller, George H., Paul S. Brown, and Carol T. Alonso. 1987. *Report to Congress on Stockpile Reliability, Weapon Remanufacture, and the Role of Nuclear Testing*. UCRL-53822. Livermore, Calif.: Lawrence Livermore National Laboratory.

Mongin, Dominique. 1991. *La Genèse de l'armement nucléaire français, 1945–1988*. Ph.D. thesis, Université de Paris I.

Morland, Howard. 1979. "The H-Bomb Secret." *Progressive* (November): 14–23.

Neal, Timothy R. 1993. "AGEX I: The Explosives Regime of Weapons Physics." *Los Alamos Science,* no. 21: 54–59.

Noble, David F. 1984. *Forces of Production: A Social History of Industrial Automation*. New York: Knopf.

Norman, Donald A. 1993. "Cognition in the Head and in the World: An Introduction to the Special Issue on Situated Action." *Cognitive Science* 17:1–6.

Norton-Taylor, Richard. 1994. "Reactor Fuel Used in N-Weapons Test." *The Guardian* (June 29): 5.

Office of Technology Assessment. 1993. *Technologies Underlying Weapons of Mass Destruction*. OTA-BP-ISC-115. Washington, D.C.: Government Printing Office.

Panofsky, Wolfgang K. H. 1994. "Safeguarding the Ingredients for Making Nuclear Weapons." *Issues in Science and Technology* (Spring): 67–73.

Pickering, Andrew. 1993. "The Mangle of Practice: Agency and Emergence in the Sociology of Science." *American Journal of Sociology* 99:559–89.

———, ed. 1992. *Science as Practice and Culture*. Chicago: University of Chicago Press.

Pinch, Trevor, H. M. Collins, and Larry Carbone. In press. "Cutting Up Skills: Estimating Difficulty as an Element of Surgical and Other Abilities." In *Between Technology and Society: Technical Work in the Emerging Economy,* edited by Stephen R. Barley and Julian Orr. Ithaca, N.Y.: ILR Press.

Polanyi, Michael. 1958. *Personal Knowledge*. London: Routledge & Kegan Paul.

———. 1967. *The Tacit Dimension*. London: Routledge & Kegan Paul.

Ravetz, Jerome R. 1971. *Scientific Knowledge and Its Social Problems*. Oxford: Clarendon.

Rhodes, Richard. 1986. *The Making of the Atomic Bomb*. New York: Simon & Schuster.

Rosengren, Jack W. 1983. *Some Little-Publicized Difficulties with a Nuclear Freeze*. RDA-TR-112116-001. Marina del Rey, Calif.: R&D Associates.

Sagan, Scott D. 1993. *The Limits of Safety: Organizations, Accidents, and Nuclear Weapons*. Princeton, N.J.: Princeton University Press.

Schaper, Annette. 1993. "The Transferability of Sensitive Nuclear Weapon Knowledge from Civil Science to Military Work." Paper presented at the Fifth International Symposium on Science and World Affairs.

Scheinman, Lawrence. 1965. *Atomic Energy Policy in France under the Fourth Republic*. Princeton, N.J.: Princeton University Press.

Schell, Jonathan. 1984. *The Abolition*. London: Picador.

Serber, Robert. 1992. *The Los Alamos Primer: The First Lectures on How to Build an Atomic Bomb*. Berkeley and Los Angeles: University of California Press.

Sewell, Duane C. 1988. "The Branch Laboratory at Livermore during the 1950's." Pp. 319–26 in *Energy in Physics, War, and Peace,* edited by H. Mark and L. Wood. Dordrecht: Kluwer.

———. 1990. Interview by G. Spinardi, Livermore, Calif., December 10.

Smith, Alice K. 1970. *A Peril and a Hope: The Scientists' Movement in America, 1945–47*. Cambridge, Mass.: MIT Press.

Smyth, Henry D. 1945. *Atomic Energy: A General Account of the Development of Methods of Using Atomic Energy for Military Purposes under the Auspices of the United States Government*. Washington, D.C.: Government Printing Office.

Spector, Leonard S. 1987. *Going Nuclear*. Cambridge, Mass.: Ballinger.

Szasz, Ferenc M. 1992. *British Scientists and the Manhattan Project: The Los Alamos Years*. London: Macmillan.

Teller, Edward. 1993. "The Laboratory of the Atomic Age." *Los Alamos Science*, no. 21: 32–37.

Van Cleave, W. 1973. "Nuclear Technology and Weapons." Pp. 30–68 in *Nuclear Proliferation Phase II*, edited by R. M. Lawrence and J. Larus. Lawrence: University Press of Kansas.

Westervelt, Donald R. 1979. "Can Cold Logic Replace Cold Feet?" *Bulletin of the Atomic Scientists* (February): 60–62.

———. 1988. "The Role of Laboratory Tests." Pp. 47–58 in *Nuclear Weapon Tests: Prohibition or Limitation*, edited by Jozef Goldblat and David Cox. Oxford: Oxford University Press.

———. 1990. Interview by G. Spinardi. Los Alamos, N. Mex., December 18.

Winner, Langdon. 1993. "Upon Opening the Black Box and Finding It Empty: Social Constructivism and the Philosophy of Technology." *Science, Technology and Human Values* 18:362–78.

Wood, Lowell, and John Nuckolls. 1988. "The Development of Nuclear Explosives." Pp. 311–17 in *Energy in Physics, War, and Peace*, edited by H. Mark and L. Wood. Dordrecht: Kluwer.

York, Herbert. 1976. *The Advisors: Oppenheimer, Teller and the Superbomb*. San Francisco: W. H. Freeman.

Zaloga, Steven J. 1993. *Target America: The Soviet Union and the Strategic Arms Race, 1945–1964*. Novato, Calif.: Presidio.

Zimmerman, Peter. 1993. *Iraq's Nuclear Achievements: Components, Sources, and Stature*. 93-323F. Washington, D.C.: Congressional Research Service.