Chapter Two

The Fission Bomb Had to Come First

In a 1958 review of Robert Jungk's then newly published Brighter Than a Thousand Suns, Hans Bethe was among those public figures, if not the first, to employ the term "big science" to characterize large-scale, government-sponsored postwar era American scientific and technical research and development. Since then the term "big science" has not only become commonly used by historians of modern science, but has itself been the subject of many studies, beginning with Derek de Solla Price's Little Science, Big Science (1963).[54]

Historians have often acknowledged the Manhattan Project as unprecedented in scale and budget, and as the beginning of big science in the United States. This attribution is misleading. Large-scale government and corporate sponsored research began to evolve in the 1930s at such Institutions as the California Institute of Technology, Stanford University, and the University of California at Berkeley. Physicist Ernest Orlando Lawrence promoted this type of research prior to World War II. Lawrence aggressively sought funding from private industry such as the Pelton Waterwheel Company, and from the Federal and California state governments for his

[54] Hans A. Bethe, review of Brighter Than a Thousand Suns, by Robert Jungk, In The Bulletin of the Atomic Scientists, 14: (1958), 426-428; Peter Galison, "The Many Faces of Big Science," in Big Science: The Growth of Large Scale Research, (Stanford: Stanford University Press, 1992), eds. Peter Galison and Bruce Hevly, 1-17; Derek J. de Solla Price, Little Science, Big Science, (New York: Columbia University Press, 1963).

cyclotron work at Berkeley in the 1930s. Similarly and before the war, physicists at Stanford obtained resources from the Sperry company for work on microwave technology.[55]

In the early years of the twentieth century American physics needed a patron, and found one in industrialists. Historian Daniel Kevles described Lawrence as the "public personification of physics." Lawrence became involved in the atomic weapon project at its beginning, taking the initiative to build a large, 184-inch cyclotron in hopes that it might be useful in designing an industrial-scale Uranium separator. By 1942 Lawrence and his team at Berkeley understood the specifics of building an electromagnetic separator, experimenting with various magnets. AEC historians Hewlett and Anderson note that "Lawrence had swept his laboratory clean of the customary patient research into Nature's laws . . . he demanded results above all else." Moreover, Lawrence's style of scientific research influenced the character of the Manhattan District because the Berkeley physicist became involved early on in building the MED system, based on his cyclotron construction projects.[56]

The majority of big science conducted after the Great Depression had military purposes. Even though the Manhattan District and the postwar era nuclear weapons complex that evolved out of it made up no small part of

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[55] Galison, "The Many Faces of Big Science," 3; John Heilbron and Robert W. Seidel, Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory,Volume I, (Berkeley: University of California Press, 1989).
[56] Daniel J. Kevles, The Physicists: A History of a Scientific Community in Modern America, (Cambridge: Harvard University Press, 1987), 271, 280; Hewlett and Anderson, The New World, 141.

this, large-scale research included numerous projects and organizations other than nuclear weapons development. Very large budgets characterized many postwar period research projects, although not all project managers found sponsorship in the American government. Instead, some projects received sponsorship from private industry. Furthermore, big science occurred in many different environments, for example, at public and private universities, at private corporations, and at federally-sponsored laboratories.

The American nuclear weapons complex, with its many design, production, and assembly facilities, in addition to private contractors, and academic and university affiliations, defies characterization merely by the all-encompassing phrase "big science." Furthermore, this phrase does not reveal the nuclear weapons laboratories' mission of turning out specific technological products for the military, nor the extent of their technological dimension. Finally, categorizing nuclear weapons work as merely big science is not an accurate description of this activity, since it does not help to explain the dynamics of changes within the weapons programs, nor the history of specific projects in this area, such as the early thermonuclear bomb program.

Any study of nuclear weapons development faces the intractable problem of the giant and labyrinthine character of the American atomic energy establishment. Secrecy aside, no study of reasonable length would be able to analyze in an integrative manner all of the numerous facilities and government and military organizations involved with nuclear weapons work at any given time. Therefore, focusing on case studies of specific projects and laboratories within the American nuclear weapons complex provides the most practical means of exploring this establishment.

As already suggested in Chapter One, I will employ Hughes's technological systems thesis as a general framework for analyzing several case studies of critical problems to the early thermonuclear program. In this chapter I will: (1) discuss the founding of the MED and its establishment as a technological system, and introduce several of the system builders, (2) show Los Alamos's founding as part of the MED, (3) highlight a case study of one of the most critical problems Los Alamos faced during the war -- calculating atomic weapons. The case study is appropriate for several reasons. First, mathematical calculations were necessary to predict the overall behavior of nuclear devices and the feasibility of proposed designs, which is why scientists began computations such as cross sections of nuclear materials even before settling Los Alamos. Second, during the course of the war nuclear weapons scientists came to view computing, in the form of punched card machines as a labor-saving technology. Scientists identified computations for nuclear weapons as a critical problem during the war. The final topic I discuss in this chapter is the AEC's founding and Los Alamos's place in this system. Los Alamos's leaders fought for the Laboratory's survival after the war, and also for autonomy in their weapons research and development projects. Understanding both the roots and evolution of the AEC and Los Alamos's place within the Commission provide a prologue to an accurate historical account of the thermonuclear weapons project.

The Manhattan District as a Technological System

In American Genesis Hughes characterizes the Manhattan District as a large technological system, similar in form to other large systems in the private sector; the relationships between scientists, engineers, and managers in inventing and developing the atomic bomb were analogous to relationships encountered in earlier innovative production at General Electric, AT&T, and DuPont. Hughes also attributes particular features to the Manhattan District that set it apart from these and other systems. According to Hughes, in the Manhattan Project the military played the role of system builder and the federal government sponsored the project, since no one system builder led the project. In contrast, large companies and public utilities such as the electric power industry that individual system builders such as Samuel Insull built up.[57]

Following Hughes, my interpretation of the Manhattan District's leadership would spot Brigadier General Leslie R. Groves as the most likely candidate for system builder of the MED, although Hughes argues the contrary, stating that Groves could not have fulfilled the role because he did not provide the inspired technical leadership given by, for example, Henry Ford in building his automobile empire. Furthermore, Hughes believes that Groves did not "elicit a collective creativity during the Manhattan Project similar to that of which Ford had stimulated at the Highland Park plant as the assembly-line system had evolved." Hughes argues that the problems facing

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[57] Hughes, American Genesis, 383; Hughes, Networks of Power, passim.

the effort to build atomic weapons were too complex and the knowledge and skill needed to solve them too specialized for any individual to assume the singular role of system builder.[58]

If the military played the system builder in the Manhattan Project, then Groves clearly led the military in the effort to develop atomic bombs. The military provided a structural framework for the project; Groves organized the project in a military fashion, evident in Los Alamos's hierarchical structured with Oppenheimer in command. In addition, as Hughes correctly states, committees -- not individuals -- often made decisions about the Manhattan District. However, Hughes does not acknowledge that several important individuals stand out as recognizable leaders and system builders in the MED.[59]

Systems can have more than one builder. The size and scale the Manhattan Project would suggest that several system builders were involved in achieving the goal of developing atomic weapons. Towards this effort, several system builders emerged over the course of the war: Groves and Oppenheimer are the most well-known, but Lawrence built the system too. Each individual played different roles in the MED yet provided leadership in attaining the same ultimate technological goal, and had extraordinary influence on the course of the atomic project.

As the late historian Stanley Goldberg stated, "Most Manhattan Project retrospectives simply overlook Grove's importance." The General brought

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[58] Hughes, American Genesis, 385.

many components into the Manhattan Engineer District, including the DuPont corporation in order to build plutonium separation plants, and Tennessee Eastman to operate the electromagnetic (Y-12) plant at Oak Ridge. As military head of the atomic project Groves made contracts with numerous industries to build plants and equipment for nuclear weapons research and development. Goldberg summarized, "Groves was despised and hated by many of those who had to work under him . . . . [H]e drove people mercilessly to get the job done." He both fostered and oversaw an all-out attempt to complete construction on materials production facilities and weapons design work and fabrication in only a few years.[60]

More directly responsible for Los Alamos and its technical program, Oppenheimer served as scientific head of the atomic project. Although Los Alamos operated hierarchically in a quasi-military fashion Oppenheimer allowed some research freedom as long as it did not hinder work on the fission weapons. Oppenheimer had to oversee several technical divisions with large numbers of staff members, as well as direct course of the project and alter its ultimate technical goals out of necessity to meet deadlines. Moreover, he had to coordinate Los Alamos's atomic weapon research efforts with the demands of the other parts of the MED system, and direct procurement of necessary technical equipment. Oppenheimer had to reorganize Los Alamos rapidly to best suit changing goals, thus rearranging

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[59] Ibid., 385-386.
[60] Stanley Goldberg, "Groves Takes the Reins," The Bulletin of the Atomic Scientists, (December, 1992), 32-39; Hughes, American Genesis, 392-402.

entire divisions and their personnel. While committees often made technical decisions at Los Alamos, Oppenheimer still had to direct all of the program changes, and maintain ultimate responsibility for all work, including the most crucial prerequisite to producing an atomic device -- calculating it.

Although Los Alamos could not have produced two different atomic devices within a short period of 3 years without all of its specialized divisions, the Theoretical (T) Division played an especially significant role because its members modeled the proposed weapons, and early in the project had to estimate mathematically calculable properties of fissionable materials. Oppenheimer depended on T Division's estimates of critical mass and efficiency, necessary prior to actual physical bomb design and the production of fissionable materials for the weapons.[61]

Initially, T Division had to estimate neutron diffusion. Hoddeson and her co-authors note that:

> . . . The members of T Division . . . had to create approximate numerical solutions and develop a sense of how the results depended on parameters, to enable extrapolation into new physical regimes. They had to balance the need for speed against the need for accuracy. . . . As illustrated in neutron diffusion calculations, T Division's primary strategy was to make the best possible calculations based on as many

[61] Hoddeson, et. al., Critical Assembly, 408; The critical mass is the amount of material from which neutrons disappear by leakage and nuclear capture at the same rate at which they are born from fissions that occur in the mass, which will just maintain a fission chain reaction; David Hawkins, Project Y, 4; Hansen, U.S. Nuclear Weapons, 13; "Neutron diffusion" is the way which neutrons distribute themselves in a critical mass of nuclear materials. "Efficiency" is the fraction of energy released in an atomic explosion relative to that which would be released if all the active nuclear material were transformed into explosive energy. Efficiency is calculated by dividing the actual yield by the predicted yield; Serber, Primer, 38; Hawkins, Project Y, 65-66, 77; Hansen, U.S. Nuclear Weapons, 14.

known factors as possible, employing extrapolation, approximation, and simplification. . .[62]

Neutron diffusion work had actually commenced the summer before Los Alamos opened, as these problems required solution before any engineering of the weapons could begin. The critical mass and efficiency calculations proved so difficult that T Division's scientists chanced to employ punched cards to speed their work. Scientists took a technical initiative on their own, without an MED's directive, to improving the way that wartime weapons calculation techniques.

Unlike Oppenheimer or Groves, Ernest Lawrence remains one of the least acknowledged MED leaders and system builders, particularly when it came to nuclear materials production. Lawrence's successful experience of aggressively securing federal and state government funding and corporate support for his cyclotron projects in the 1930s benefited his role in the atomic project. Seeking even more funding in the following decade, in 1941 Lawrence offered the Radiation Laboratory's services to James Bryant Conant, head of the S-1 (Section One) Committee of the Office of Scientific Research and Development (OSRD). In charge of studying the properties of uranium, the S-1 Committee took up Lawrence's' offer to experiment with separating U^{235} from U^{238} so that this process could subsequently be done on an industrial scale. Towards this effort, Lawrence vigorously recruited young physicists and graduate students to join the Radiation Laboratory. With the results of

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[62] Hoddeson, et. al., Critical Assembly, 408.

work on the 184-inch cyclotron in hand Lawrence convinced Groves and the Stone and Webster engineers to begin construction on a giant, industrial-size electromagnetic separation facility (Y-12) at Oak Ridge, Tennessee in the fall of 1942.[63]

Calculating Atomic Devices: A Critical Problem for Los Alamos

Lawrence's cyclotron projects themselves did not just influence the style of the Manhattan District and the construction of the Y-12 plant. The location Lawrence had chosen to undertaken his early cyclotron work mattered too. Lawrence's Radiation Laboratory, at the up and coming Berkeley physics department, had already by this time became a mecca for young physicists. In 1929 Berkeley had attracted J. Robert Oppenheimer, who chose Berkeley over Harvard while simultaneously accepting a joint appointment with Caltech. Oppenheimer chose Berkeley as the site for a 1942 theoretical physics conference to discuss the theory of a fast-neutron reaction, and ponder the design of an atomic weapon.[64]

To the Berkeley summer conference Oppenheimer invited a group that he later nicknamed the "luminaries," who were supposed to "throw light" on atomic design. The participants included some the most well known scientists in the U.S.: Cornell physicist Bethe, Stanford theoretician Felix Bloch, Indiana theoretician Emil Konopinski, Hungarian physicist Edward Teller, Harvard physicist John H. van Fleck, and his former student Robert Serber.

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[63] Hewlett and Anderson, The New World, 141-143; Rhodes, Atomic Bomb, 376.

Knowing Oppenheimer since 1934, Serber had previously been appointed to Berkeley on a postdoctoral National Research Council fellowship, then subsequently, at the urging of physicist I. I. Rabi, went to the University of Illinois at Urbana to take a tenure track position, a rare opportunity during the Great Depression. Oppenheimer and Serber had become close friends and colleagues at Berkeley. A week after Pearl Harbor, Oppenheimer went to Urbana to convince Serber to return to Berkeley and join the theoretical conference the following summer.[65]

Serber had also known Lawrence during his first tenure in Berkeley. By the time Serber returned to Berkeley, Lawrence had already begun the calutron project to separate U^{235} from U^{238}. To assist in this, Oppenheimer had assigned several graduate and postgraduate students to work on magnetic field orbit calculations for Lawrence's electromagnetic separator. The most advanced members of this group included two post doctoral fellows, Eldred Nelson and Stanley Frankel, whom Serber put to work on improving the current state of neutron diffusion theory. The calculation of the exact amount of fissionable material needed for a weapon and of the efficiency of the reaction was a difficult but crucial task since the MED's selection of a production process for fissionable materials would depend on accurate estimates of weapons materials requirements.[66]

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[64] Hewlett and Anderson, The New World, 102; Rhodes, Atomic Bomb, 415.
[65] Serber, Primer, xxvii-xxviii.
[66] Hewlett and Anderson, The New World, 103; Author interview with Robert Serber, New York, NY, November 26, 1996; Interview transcription is held at the American Institute of Physics Center for the History of Physics, Niels Bohr Library, College Park, MD.

According to Serber, up until the issuance of the MAUD report no American scientists had published papers on neutron diffusion. If Oppenheimer's predecessor at the atomic bomb project -- Wisconsin theoretician Gregory Breit -- had performed any work on neutron diffusion, he had kept it so secret that no one else knew about it in 1942. Thus, Oppenheimer assembled a series of secret British papers by Rudolf Peierls, Klaus Fuchs, P.A.M Dirac, and others on the diffusion of neutrons through a critical mass, and on efficiency, by the time the Berkeley conference commenced, to serve as a basis for the luminaries' work that summer.[67]

Along with trying to improve the simple diffusion theory the British had used, to Nelson and Frankel also fell the assignment of estimating critical masses of uranium. The former task was a prerequisite for the latter. Qualitatively, the critical mass depends on the diffusion rate of neutrons out of an active mass as compared with the rate that they are generated in it. To calculate the critical mass requires a knowledge of the average way that neutrons distribute themselves in the mass. Ordinary simple diffusion theory is only valid in the range where the mean free path of diffusion particles is small compared to the dimensions of interest. An atomic weapon

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[67] Serber, Primer, xxix; Author interview with Serber, November 26, 1996; Robert Budwine has noted that one of the early British papers on calculating the critical mass of uranium was P.A.M. Dirac, "Estimates of the Efficiency of Energy Release with a Non-Scattering Container," BM-123 (MS D.4), December 1942 (sic); Citation in Robert Budwine, "Technical Chronology of the Development of Nuclear Explosives, Part 1 - Early Fission Explosives: 1942-1946," COPD-93-138, Lawrence Livermore National Laboratory (hereafter LLNL), November 1, 1993, 3, [This Report is Secret-RD]; The MAUD report was prepared by the British based on the theoretical atomic bomb work done by refugee physicists Otto Frisch abd Rudolf Peierls between 1940 and 1941. The MAUD report indicated that an atomic weapon was possible and estimated that a critical mass of ten kilograms would create an enormous explosion.

is more complicated; the number of neutrons in a given small region depends not only on that in adjacent regions, but on the entire distribution throughout the mass. Thus, Nelson and Frankel needed to employ an integral diffusion theory and find methods to apply it in a practical calculation.[68] Serber recounted:

> Nelson and Frankel did better [than merely improve on the British work] and wrote down an exact integral equation for the diffusion problem and found something about its solutions. . . [and in] the literature they found the Wiener-Hopf equation -- an exact solution for the case of flow in one direction. With that background they were in a good position to make accurate diffusion theory calculations.[69]

From the time he arrived in Berkeley in April until the summer conference started, Serber worked by himself on the theory of efficiency and hydrodynamics of the atomic explosion. When the conference began in earnest in July 1942, Serber, Frankel, and Nelson led off with a discussion of their efforts, confident that they understood well the physics of atomic weapons. The group thought that the chief difficulty in constructing an atomic weapon at that point involved building a gun of high enough velocity for the plutonium to assemble. Within two days, the entire group assumed they had nearly solved the fission problems, leaving Teller with the opportunity to present his idea for a thermonuclear device.[70]

Lore about the Berkeley meeting suggests that most of the conference was devoted to the theory of the Super. Serber confirmed this, stating, "It's

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[68] Hawkins, Project Y, 65.
[69] Robert Serber, "The Initial Challenge," lecture at Los Alamos National Laboratory (hereafter LANL), March 30, 1993, videocassette, [This document is Secret-RD]; Author interview with Serber, November 26, 1996.
[70] Serber, "The Initial Challenge."

true and its remarkable that we started out talking about [fission] and Teller brought up his Super. . . .This happened two days after the meeting started . . . everybody jumped on that since the A-bomb was a settled issue now."[71] However, by the end of the conference the participants concluded that an atomic device would constitute a significant scientific and technical effort. Although distracted by Tellers' idea, the group still settled on pursuing the atomic configuration because of several difficulties found with the thermonuclear weapon theory and because an atomic device would require development first to serve as an initiator for the hydrogen device, which they had named the "Super." Regardless of how much the Berkeley group found the Super intriguing, Bethe explained, ". . .the fission bomb had to come first in any case . . ."[72]

Like Serber, Bethe also remembered that because of Serber's, Frankel's, and Nelson's preparatory work, the theory of the fission bomb was "well under control so we felt we didn't need to do much." Therefore, the Berkeley conferees felt that they could spare extra time to theorize about this Super, and did not dismiss it as a possible line of research in the future. I discuss the Super theory and its origins, and the Berkeley Conference participants' reminiscences of it in the next chapter.[73]

Confident about the atomic gun weapon's feasibility, the Berkeley group reported to the S-1 Committee in August 1942 that a fission bomb was

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[71] Author interview with Serber, November 26, 1996.
[72] Rhodes, Atomic Bomb, 417; Hans Bethe quoted in Jeremy Bernstein, Hans Bethe: Prophet of Energy, NY: Basic Books, 1980), 73.

probable but would require a critical mass "6 times the previous [estimated] size[:] 30 kg U^{235}." Established in 1941 to supervise research on uranium, the S-1 Executive Committee, chaired by Conant, supervised all such work. Other members included Lawrence, Lyman Briggs, Arthur Holly Compton, Harold Urey, and Eger Murphree. Upon reading the Berkeley group's report, the S-1 Committee forwarded to head of the OSRD, Vannevar Bush, a recommendation that an atomic bomb could win the war. They also noted that a Super likely could be built at some point in the future.[74]

An atomic device remained the first priority of the Manhattan District, however, and when Los Alamos opened in 1943, Nelson and Frankel continued their work on neutron diffusion calculations for the laboratory's main technical objective, a gun weapon fueled by plutonium or perhaps uranium. In continuing their calculations, Frankel and Nelson ordered the same types of mechanical desk calculators they had used in California -- Marchants, Fridens, and Monroes. But they difficulties achieving any computational accuracy using these machines for calculations related to the gun weapon. Bethe recalled the numerical problems that several of T Division's members tried to solve:

> The first was neutron diffusion . . . . [T]o assemble the bomb by a gun, shooting . . . fissile material [together] . . . . very complicated shapes would result. We wanted to know how neutrons would diffuse in such a complicated assembly, in order to assess the probability that the chain reaction might start prematurely, and the bomb explode with less than the full yield. Even in the final assembly, we might have a cylinder of fissile material rather than a sphere, because this would be

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[73] Rhodes, Atomic Bomb, 417.
[74] Rhodes, Atomic Bomb, 420-421; Hewlett and Anderson, Atomic Shield, 75.

> much easier to fabricate: we wanted to know how much of the energy yield of the bomb we would lose by this. All these problems were insoluble [sic] by analytical means, and while we could set up integral equations describing the process, they were too far complicated to be solved by the desk computing machines.[75]

Despite Nelson and Frankel's earlier work at Berkeley, Los Alamos scientists still found themselves facing the problem of finding a reasonably precise method of determining critical masses. Their results so far remained imprecise, even though at Berkeley the theoretical group had concluded that no significant gaps could be found in the theory of the fast-neutron reaction. In October, 1943, Frankel and Nelson reported that they could not find a way of transforming the integral equation for the infinite cylinder geometry into a form for which they had a solution. As Bethe described above, these problems could not be solved by the laboratory's hand computers, almost exclusively a group of women (many were scientists' wives) employing the desk calculators, under the supervision of New York University mathematician Donald "Moll" Flanders. Several members of this group, including Mary Frankel, Josephine Elliott, Mici Teller, and others, became exceptionally adept at hand computing and indispensable to Los Alamos's T Division.[76]

Even with the hand-computing group employing about 20 persons, the calculations for the gun device strained the mechanical calculators.

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[75] Hans Bethe, "Introduction" in Computers and Their Role in the Physical Sciences, eds. S. Fernbach and A. Taub, (New York: Gordon and Breach, 1969), 2; Serber, "The Initial Challenge."

[76] LA-31, "Multiplication Rate for Untamped Cylinders," October 18, 1943, [This Report is Secret-RD]; Hewlett and Anderson, The New World, 102; N. Metropolis and E.C. Nelson, "Early Computing at Los Alamos," Annals of the History of Computing 4, No. 4, October 1982,

Moreover, the desk calculators often broke down and were shipped back to their manufacturers for repairs. So many calculators broke down that young physicist Richard Feynman and mathematician Nicholas Metropolis began a trial and error method of repairing the machines, mainly by comparing the mechanical motions of a working calculator with a broken one. Metropolis remembered that he and Feynman even placed a sign outside their office door proclaiming their repair service, until the laboratory administration reprimanded them for not following the "proper" procedure of sending the machines back to the manufacturers for repair.[77]

How could the neutron diffusion problems be solved, and reasonably quickly at that? One of the Laboratory staff member's previous experiences at another scientific center proved useful for solving problems related to the gun device. Physicist Dana Mitchell had worked at Wallace J. Eckert's astronomy laboratory at Columbia University where laboratory staff used IBM punched card accounting machines to carry out astronomical calculations.

Eckert, one of the most famous figures in numerical astronomy at this time, had received his Ph.D. from Yale in 1931. Even before completing his degree, Eckert went to Columbia University as an assistant in astronomy, and began to build a small computing laboratory, supported by Thomas J. Watson of the IBM Corporation. In 1933 Eckert persuaded Watson to enlarge the laboratory that later became the Thomas J. Watson Astronomical Computing

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348-357.

[77] Metropolis and Nelson, "Early Computing," 349; Richard P. Feynman, Surely You're Joking Mr. Feynman: Adventures of a Curious Character, (New York: Bantam, 1985), 108.

Bureau. Eckert's laboratory employed IBM-made punched card machines for scientific calculations, and was one of the first to employ commercial punched card machines for basic scientific research.[78]

Herman Goldstine has stated that probably more than any other scientist, Eckert's demands for "emendations of the standard IBM machines to make them more useful for scientific work forced the company to develop an attitude of flexibility toward scientific users of machines." Eckert's desires to mechanize scientific calculations not only influenced IBM's technical strategies, but also likely inspired interest in electronic computers at the University of Pennsylvania and at the Institute for Advanced Study (IAS) at Princeton.[79]

Getting the Job Done on Time: Mechanization of Fission Calculations

After coming to Los Alamos, Dana Mitchell sat on the Laboratory's Governing Board which met weekly; Mitchell was also in charge of equipment procurement. When Bethe mentioned the difficult neutron diffusion equations, Mitchell recalled Eckert's laboratory, and recommended that Los Alamos try IBM 601 punched-card accounting machines (PCAM) for calculations of the behavior of the gun-type weapon. Mitchell estimated that a single calculation of the gun device would take six to eight months if carried out by the laboratory's hand computers. With the help of the IBM machines,

[78]Herman H. Goldstine, The Computer from Pascal to von Neumann, (Princeton: Princeton University Press, 1972), 109-110.
[79] Ibid., 110.

on the other hand, individual calculations might be carried out in three to four weeks.[80]

More specifically, the Laboratory ordered IBM machines for calculating critical masses of odd-shaped bodies in the fall of 1943. They could not arrive at Los Alamos fast enough. In January 1944, Oppenheimer urged that the IBM machines be rushed to the laboratory, stating that the card punches were essential for guiding engineering design; the card punches' results would be used in placing orders for materials whose fabrication would take months.[81]

The IBM machines did not arrive in Los Alamos until the spring of 1944. Because of the secrecy surrounding Los Alamos, the IBM corporation did not know the final destination of their machines, nor could they send an installation crew. The Army requisitioned an IBM maintenance expert (who had been drafted earlier) to Los Alamos in the meantime, but the machines arrived before him, only partially assembled. Feynman, Frankel, and Nelson finished assembling the machines using only the enclosed wiring blueprints.[82]

At this time, very few people at the Laboratory had any experience using IBM accounting machines. Persons who knew how to use punched card machines became a sought-after species at Los Alamos. Mathematician Naomi Livesay had expertise working with IBM machines at Princeton

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[80] Telegram from J. Robert Oppenheimer to S.L. Stewart, January 28, 1944, B-9 Files, Folder 413.51, Drawer 96, LANL Archives, [This Document is Secret-RD].
[81] Hawkins, Project Y, 81; Nicholas C. Metropolis, "Computing and Computers: Weapons Simulation Leads to the Computer Era," in Los Alamos Science 7 , (Winter/Spring, 1983), 132-141; Bethe, "Introduction," 2; Telegram from Oppenheimer to Stewart.
[82] Metropolis and Nelson, "Early Computing," 350.

Surveys; T Division hired her in February 1944, before the machines had arrived. Subsequently she supervised the military and civilian crews running the machines. Because pressure to complete work on the IBM machines steadily increased, in the summer of 1944 Livesay hired an assistant, Eleanor Ewing, who had been teaching mathematics at Pratt and Whitney, to help supervise the teams performing calculations on the machines.[83]

Not until summer 1944 did T Division's members solve the problems of calculating neutron diffusion and critical masses. At Berkeley, Nelson and Frankel had devised the extrapolated end-point method for studying neutron diffusion, although it was far too simple to use to model the complicated movement of neutron through the core of a bomb. In order to model neutrons with many velocities several T Division members tried a "multigroup method" of numerical approximation where they divided the neutrons into several groups, each containing neutrons of the same velocity, reducing the overall problem to a series of smaller, one-velocity problems. This represented a more realistic description of neutron diffusion in a weapon.[84]

Likewise, T Division members often approximated solutions to problems. Finding a suitable solution for critical mass calculations for the gun assembly required several approaches pursued by Bethe, Frankel, Nelson, David Inglis, Robert Marshak, and others. They had essentially solved this

[83] Personal communication with Caroline L. Herzenberg and Ruth H. Howes.

problem by July 1944. Several months earlier, Bethe and Feynman had developed an approximate formula for efficiency.[85]

In 1944, Los Alamos suddenly and abruptly changed its main technical goal. As mentioned earlier, when Los Alamos opened, its scientists concentrated on building a uranium or plutonium gun-type weapon, where two subcritical masses of fissile material would be shot together to form a critical mass. The Berkeley conferees and most of Los Alamos's members initially saw gun assembly as an achievable goal. During the summer of 1944, however, Los Alamos's focus shifted to developing an implosion bomb.[86]

Caltech physicist Richard Tolman suggested implosion as early as 1942, but the implosion method for assembling any fissile material constituted an extremely complicated shockwave phenomena. An implosion configuration basically consists of an amount of fissile material surrounded by high explosives. The explosives are detonated, creating shockwaves that travel inward and compress the fissile material into a super critical mass, creating a fission chain reaction. Although this presented a formidable problem, another Caltech physicist, Seth Neddermeyer, began a small implosion study program after Los Alamos opened. Los Alamos's technical focus began to shift in late 1943 after mathematician John von Neumann visited to lend his assistance to the project.

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[84] Hoddeson, et al., Critical Assembly, 179-180.
[85] Ibid., 183.
[86] Hoddeson, "Mission Change," 267.

A leading expert on shock and detonation waves, by World War II von Neumann served as a consultant to the Army Ballistics Research Laboratory, the OSRD, and the Bureau of Ordnance. Not surprisingly, he became involved with Los Alamos when Oppenheimer requested his help. Von Neumann studied Neddermeyer's small test implosions of cylindrical metal shells, and realized that implosion could be made far more efficient if one used a greater ratio of high explosive-to-metal mass, causing rapid assembly. In addition, the implosion scheme might use less active material and require less costly materials purification schemes.

Nuclear materials issues aside, the plutonium gun assembly had another problem. In the spring and summer of 1944, Emilio Segre's experimental physics group realized that spontaneous fission in Pu^{240} made the plutonium gun idea unworkable; it would not be fast enough to tolerate the added neutrons. Yet, given the state of the MED's production facilities, plutonium was the only material at that time that could be produced in large enough quantities for many bombs. A uranium gun bomb could be made by the summer of 1945, but probably only one. Thus, the Laboratory turned to implosion as the only practical means of utilizing the plutonium available in the summer of 1944.[87]

Generally, an implosion device works in the following way: A subcritical fissile core (in the war this meant Pu^{239}) is surrounded by a shell of high explosives -- part of a lens structure that focuses the blast into a

[87] Goldstine, The Computer, 177; Hoddeson, et al., Critical Assembly, 129; Hoddeson, "Mission

converging, inward moving front. Electrical charges detonate the explosives nearly simultaneously, so the resulting blast wave is relatively symmetric, causing an even implosion of the core and compression of the fuel. Due to this compression, the core becomes supercritical, and begins to expand outward, causing an explosion.[88]

Modeling these processes provided not merely a challenge, but in the summer of 1944 no one knew if implosion would work at all. But, with the change in the project already being considered by the Laboratory in spring 1944, the purpose of the IBM machines changed too, and T Division began preparing problems for the IBM machines in anticipation of modeling an implosion device.

Towards the new fission implosion configuration, Teller and his group in T Division assumed responsibility for developing a mathematical description of implosion, and calculated the time of assembly for large amounts of high explosives. Along with mathematician Nicholas Metropolis and Feynman, Teller calculated the equation of state for highly compressed uranium and plutonium expected to result from a successful implosion. Teller declined, though, to take charge of the group scheduled to perform detailed calculations of an implosion weapon. Thus, Bethe sought a replacement for Teller.[89]

Change," 274-281.

[88] Hansen, U.S. Nuclear Weapons, 21.

[89] Bethe, "Introduction," 3; Hans Bethe, "Comments on the History of the H-bomb," op. cit., 43-53.

In March 1944, Bethe reorganized the Theoretical (T) Division in order to meet the urgency of the implosion program, and in July replaced Teller with Peierls as head of the theoretical implosion group. When Peierls first visited in February, he suggested a step-by-step method of solving differential equations based on his earlier calculational work on blast waves in air. Bethe recognized the importance of Peierls's suggestion and T Division based its implosion calculations on the same form as Peierls's blast wave equations.

Simulating the implosion device required detailed calculations of complicated implosion hydrodynamics. However, the Laboratory's hand computers could not solve the partial differential equations of hydrodynamics employing realistic equations of state applicable to high temperatures and pressures. By February 1944, T Division began to calculate the initial conditions for numerical integration of the implosion differential equations on the IBM machines. The numerical procedure for an implosion simulation, and a general approach to processing the cards through a sequence of machines, were worked out even before the IBM machines arrived. Metropolis and Nelson elaborated on the hydrodynamic problems:

> The numerical procedure evaluated the differential equation for a sequence of points covering one space dimension and then integrated ahead one step in the time dimension. Thus, a punched-card was established for each point in the first dimension, with a deck of cards representing the state of the implosion at a specific time instant . . . Each integration step of the partial differential equation corresponded to one cycle of a deck of cards through the machines . . . . About a dozen separate machine steps were involved in each integration cycle.[90]

[90] Hoddeson, et al., Critical Assembly, 160; Metropolis and Nelson, "Early Computing," 350.

After the IBM machines arrived and Feynman, Frankel, and Nelson assembled them, the card punch computational procedure needed checking out before implosion calculations could begin. Thus, Feynman and Metropolis organized a "race" between the hand computers and the card punches. For two days the hand-computing group kept pace with the IBM machines, as they tried to compute the first few integration steps of an implosion simulation in order to work any bugs out. By the third day, however, the tireless accounting machines pulled ahead and the group abandoned the race.[91]

A race of another sort continued. "Everything we did, we tried to do as quickly as possible," Feynman recalled. But in spring 1944 implosion calculations undertaken on the IBM machines went very slowly. To operate the machines, the army had recruited several high school graduates from all over the U.S. and sent them to Los Alamos. This Special Engineering Detachment (SED) arrived in Los Alamos knowing nothing about the purpose of the project or of their own duties of punching the cards and running them through the machines. One cycle took about three months to complete until Feynman obtained permission from Oppenheimer to inform the SED's about the purpose of the project. Excited about fighting a war, the SED's quickly invented their own programs to speed the effort, and completed about nine problems in three months. Feynman remembered:

> The problems consisted of a bunch of cards that had to go through a cycle. First add, then multiply -- and so it went through the cycle of

[91] Metropolis and Nelson, "Early Computing," 350-351; Feynman, Surely You're Joking, 109.

> machines in this room, slowly, as it went around and around. So we figured a way to put a different colored set of cards through a cycle too, but out of phase. We'd do two or three problems at a time.[92]

Implosion modeling began with simulating the detonation of the high explosive charge surrounding the bomb, computing the propagation of the detonation front through the charge, generating a shock wave when the detonation reached the tamper (a dense, inactive material surrounding the fissile core), propagating the shock wave through the tamper and active material, and reflecting the shock wave when it reached the center.[93]

The first implosion simulations explored different configurations of the high-explosive charge, tamper, and active material. Based on the results of these exploratory simulations, one particular implosion configuration, T Division chose what later became known as the Mark III, for detailed simulation. The Mark III represented the most practical road to an atomic device; when engineering construction on the actual implosion bombs began, engineers and technicians developed this configuration because it was the only one for which detailed data on its expected behavior existed.[94]

During the Manhattan Project the nuclear design process could not have happened in the reverse order. At this time, when nuclear weapons science was a new practice, its practitioners were exploring many unknowns. No one knew how a weapon would work, and the atomic project's success, measured ultimately in a successful fission bomb test, rested largely on

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[92] Feynman, Surely You're Joking, 111.
[93] Metropolis and Nelson, "Early Computing," 354.

theoretical mathematical estimations of a weapon's predicted behavior. The physical design of a weapon had to follow mostly from the theoretical work, although some of those performing the theoretical calculations for the Mark III no doubt had to consider physical limitations imposed on the weapons, such as, for example, the limited available amount of Pu^{239} to fuel the device. To some degree, T Division members had to tailor some theoretical simulations to fit within certain practical engineering parameters. Still, scientists performed much of the theoretical work during the war independently of experimental physical design aspects, considering that time was so crucial.

In a similar fashion, the theoreticians did not view the hand computers or IBM machines as experimental instruments. With the mechanical difficulties involved simulating implosion and given that T Division perpetually tried to accelerate these problems, there was little time for experimentation with the IBM machines. Frankel in particular caught the "computer disease" that physicist Feynman so acutely described: "The trouble with computers is you *play* with them . . . and it interferes completely with the work." Frankel stopped paying attention to supervising the card punch operations and the implosion calculations went too slowly. Bethe too recalled that Frankel became so enchanted with the machines that he forgot that the real aim of the project -- to solve the implosion problem. In order to speed the calculations, Bethe replaced Frankel with Metropolis and put

[94] Ibid., 354-355.

Feynman in charge of the entire IBM group. Frankel eventually ended up in Teller's group working on the Super theory. Under Feynman, Metropolis and Nelson, the whole IBM group of about two dozen machine operators and coders focused exclusively on implosion calculations.[95]

Not only did human folly affect the pace of work on the IBM machines, but so did the natural environment. Metropolis recalled that the machines were, for that time, relatively complex, each one containing several hundred relays as the primary computing element. The unpaved roads in Los Alamos and constant New Mexico dust caused intermittent errors -- at least one in every third integration step -- by sticking to the relay contacts. Luckily for the human operators, the computational procedure was very stable and insensitive to small mistakes; the operators had only to correct errors in the more significant digits.[96]

Over the course of the war, Los Alamos strengthened its ties with IBM. The laboratory needed machines with particular features that would speed the implosion calculations and accelerate the pace of weapons development. In May 1944 the laboratory requested that IBM custom-build triple-product multipliers and machines that could divide. Nelson himself traveled to New York in June to meet with IBM's vice president John McPherson to discuss in detail the new proposed machines. The new punched card models arrived at Los Alamos towards the end of 1944, and helped increase the pace

[95] Feynman, Surely You're Joking, 109-110; Bethe, "Introduction," 5; Author interview with Hans Bethe, LANL, September 14, 1994; Interview transcription held at LANL; Hoddeson, et al., Critical Assembly, 307.

of the implosion simulations, while simultaneously increasing the need for more operators to run them.[97]

Many of the machine operators knew more about using the IBM equipment than T Division's scientific staff and consultants. Von Neumann took a great interest in the punched cards and learned their basic operation from Livesay and Ewing, who shared on office with him.[98] His experience with the IBM machines influenced his views on designing larger, electronic computers in which von Neumann became extremely interested at this time. Metropolis later wrote that von Neumann found wiring the IBM tabulator plugboards extremely frustrating:

> . . . the tabulator could perform parallel operations on separate counters, and wiring the tabulator plugboard to carry out parallel computation involved taking into account the relative timing of the parallel operations. He [von Neumann] later told us this experience led him to reject parallel computations in electronic computers and in his design of the single-address instruction code where parallel handling of operations was guaranteed not to occur.[99]

While in 1944 and 1945 the IBM machines represented the state-of-the-art in punched card technology, large, electronic computer projects got slowly underway at a few military and academic centers in the U.S. Keenly aware of these projects, von Neumann pushed his Los Alamos colleagues to consider the new electronic computers for the Laboratory's problems. In 1944 von Neumann informed T Division about Howard Aiken's Mark I computer at Harvard University. Although an electromechanical relay machine, it was

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[96] Harlow and Metropolis, "Computing and Computers," 134.
[97] Metropolis and Nelson, "Early Computing," 351.
[98] Herzenberg and Howe, op. cit.

still much faster and more precise than punched card devices. Von Neumann suggested that T Division loan one of its implosion problems to Aiken to run on the Mark I, and machine operators completed the problem in Spring 1944.[100]

Metropolis recalled that von Neumann kept Los Alamos's staff informed about "[p]rogress elsewhere in computing. . . . [c]ommunication of these new developments by von Neumann was initially informal, but as their profound implications became apparent, he was requested to present a series of lectures on them, showing the technical links between the separate independent developments." To Metropolis and other staff he also "described his computer of the future, outlining his single-address architecture, later implemented in the IAS computer" and other machines.[101] Von Neumann not only carried to Los Alamos news of computing developments, such as the Bell Telephone Relay-Computer, but he also inspired in T Division a contagious enthusiasm for large-scale computers and mechanizing weapons calculations.[102]

Despite the emergence of electronic computing, during the war the majority of implosion simulations occurred in New Mexico. At Los Alamos, by late April 1944, SED's completed the first implosion problem after about three months. The groups finished seven more IBM problems by the end of

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[99] Metropolis and Nelson, "Early Computing," 351.
[100] Ibid., 351.
[101] Ibid., 352.
[102] Letter from von Neumann to Oppenheimer, August 1, 1944, LANL Archives, MED Files, A-84-019, 310.1, T Division, Box 6, Folder 10.

1944, and seventeen in 1945, on three shifts, six day per week schedules.[103] Nearly from the time the IBM machines arrived at Los Alamos, they ran 24 hours a day to complete the implosion calculations on time. The results of the calculations showed that the Fat-Man type bomb could get a good energy yield with the fissile material strongly compressed in a spherically symmetrical implosion. The July 1945, Trinity test verified the calculations for the Fat Man design.[104]

The Emergence of Labor-Saving Technology

Los Alamos's employment of punched card machines gave a tremendous boost to the implosion calculations and undoubtedly helped to complete these problems in the face of military deadlines. Nevertheless, the IBM machines did not determine the outcome of Los Alamos's technical program; Los Alamos's scientists, not the card punches, held responsibility for developing an implosion device and determining the final design choice for the Trinity test. According to historian of technology Merritt Roe Smith, technological determinism -- the idea that technology is autonomous, and independent of society, yet it impinges on society -- has traditionally been one of the most influential theories of the relationship between technology and society. Technological determinism is deeply imbedded in American culture, with an intellectual heritage dating back to at least the eighteenth-century Enlightenment. Not surprisingly, much history of technology is laden with technological determinism, although such approaches have been challenged

[103] Metropolis and Nelson, "Early Computing," 351.

in recent years by the influence of the sociology of science and in particular the social constructivist schools of thought.[105]

Technology does not choose nor reproduce itself, although existing technical artifacts certainly act as preconditions for new and developing technologies. Technological choices represent a diverse array of human needs and values. They can also be representative of the particular culture or society in which they are developed.

The United States has often been characterized by historians as a society with a tendency towards building machines for automation, and for finding labor-saving technology. Hughes has described the United States as "technology's nation," a country of machine makers seeking a drive for order, system, and control. H.J. Habakkuk explored the American penchant for automation and employment of labor-saving technology in his American and British Technology in the Nineteenth Century (1962). Compared to the British, the US invented and adopted mechanical methods of labor more rapidly. There reasons for this originated in the cultural, environmental, and economic surroundings of the younger nation.[106]

According to Habbakuk, the United States had a scarce labor supply, thus manufacturers had to invent new technical means to make up for the labor scarcity. In addition, the U.S.'s over-abundance of land itself had several

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[104] Bethe, "Introduction," 8-9.

[105] Merritt Row Smith, "Introduction," in Does Technology Drive History?, eds. Merritt Roe Smith and Leo Marx, (Cambridge, MA: MIT Press, 1994), 2-3; Bijker, Pinch, and Hughes, "Introduction," 9-15.

[106] Hughes, American Genesis, 1; H.J. Habakkuk, American and British Technology in the Nineteenth Century: The Search for Labor-Saving Inventions, (Cambridge: Cambridge

effects: The U.S. had an independent and strong agricultural base, thus in order to attract labor away from agriculture, industries had to offer high wages; America had many forms of natural recourses from water power to minerals; the large American terrain meant that not only did agriculturists become creative in mechanizing work, but so did industrialists, unable to rely on others in close proximity.[107]

Industrialists employed all kinds of machines for a wide variety of tasks in the late nineteenth and early twentieth centuries, punched card machines not least among them. Herman Hollerith developed some of the first commercially used punched card machines at the end of the nineteenth century. The U.S. Census Bureau was one of the first large organizations to employ Hollerith's accounting technology in its mammoth task of tabulating statistics on the American population. Later, Hollerith developed a punched card system for the New York Central and Hudson River Railroad, and for the Pennsylvania Railroad Company to account their freight, scheduling, and statistics. Although successful in his punched card business, in 1911 Hollerith sold his small company to the Computing-Tabulating-Recording Company (CTR), which became the International Business Machines Corporation in 1924.[108]

Prior to Wallace Eckert's employment IBM's punched cards at his Columbia University laboratory, the only other situation where scientists

University Press, 1962).
107 Ibid., passim.
108 Williams, Computing Technology, 253; Lars Heide, "Shaping a Technology: American

used accounting machines for basic research occurred in the mid 1920s at the National Almanac Office in London, where L.J. Comrie employed punched card machines to calculate the motions of the moon from 1935 to 2000. By the late 1920s and early 1930s when Comrie and Eckert just began to recognize the value of punched card machines for large scientific problems, calculating equipment of all kinds had been introduced into American businesses and industry and there had been firmly established as a labor-saving technology.[109]

Wartime Mission: Los Alamos Establishes an Approach to Problem-Solving

The introduction of business accounting machines into the wartime theoretical program to design an atomic weapon was a novel one for attempting to overcome a critical problem faced by Los Alamos. This sort of approach to problem solving reflected several characteristics of : (1) the wartime laboratory itself and, (2) its relationship to the Manhattan District. Wartime Los Alamos operated, according to Lillian Hoddeson, in a strict mission-oriented mode. Hoddeson describes the "mission-directed" laboratory as one where "scientific and technological research is oriented by a larger goal, the well-defined 'mission,' which typically is expressed in terms of a contribution to society reaching beyond the laboratory."[110]

From the beginning of the project, Groves, the military, and Oppenheimer imposed a strong mission orientation at Los Alamos. Projects

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Punched Card Systems, 1880-1914," (University of Odense, 1996), 5-6, 15-21, 23.

[109] Williams, Computing Technology, 254.

[110] Lillian Hoddeson, "Mission Change," 265.

in line with the goal of producing a practical military weapon received nearly unlimited funding and material support. Other projects out of line with the main goal of an atomic device (such as the Super) starved. Scientists had to meet Groves's deadline of producing a working weapon by summer 1945, therefore "pure" scientific research was not carried out during the war. In other words, as Hoddeson and her colleagues have shown, scientists had no time to "provide technical solutions based on full understanding of fundamental laws." Instead, scientists were forced to adopt alternative and inexact approaches to problem-solving, even approximating theoretical implosion calculations. Facing a strict deadline, Los Alamos scientists had to pay attention to practicality, and focus on the reliability of methods. Their objectives:

> [S]hifted from understanding to use, and from general conceptions to particular materials and apparatuses. This reorientation encouraged them to diversify their methodological toolkits with approaches typically employed by engineers and craftsmen, whose technical problems were anchored in concrete phenomena.[111]

Finally, Los Alamos had the additional characteristic of being organized like a military institution, enabling civilian division and group leaders to create an effective hierarchical research facility, where deadlines could be rigidly enforced and scientists directed towards particular work or technical goals.[112]

The Laboratory's relationship to the Manhattan District changed over the course of the war. Even though Los Alamos had been established as a

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[111] Hoddeson, et al., Critical Assembly, 4-6.

[112] Hoddeson, "Mission Change," 266; Notably, practical concerns originating in the MED still affected the final shape and component structure of the first fission weapons. An implosion or

theoretical center within the MED's growing system of contractors, production facilities, and universities, the Laboratory quickly began to establish a form of independence from the MED where weapons scientists adopted their own problem-solving approaches to weapons design without the explicit consent of the Manhattan District. T Division's choice of computing methods is one example of this independent approach. Furthermore, and in a more general sense, Los Alamos scientists and engineers had the autonomy to make technical changes to the atomic device as long as the final product still met the military requirement of a useable weapon and if the changes included considerations such as the efficient use of nuclear materials.

Like computing, Hoddeson's example of the "crisis" of spontaneous fission in plutonium demonstrates both Los Alamos's approach to weapons development and the evolution of the Laboratory's relationship with the MED. In 1944, when Segre's group realized that "production" plutonium from the Clinton reactor at Oak Ridge fissioned spontaneously at an alarmingly high rate, they concluded that using this material in a gun type device would cause it to predetonate, and thus "fizzle." In this instance, the technological limits forced a revision of the Laboratory's theoretical program. However, by this time Groves had already ordered construction of the large facilities at Clinton as well as Hanford to produce large amounts of plutonium. The Los Alamos Governing Board's decision to change the

gun device had to be designed not only to fit in the bomb bay of a B-29, but the device had to be technical focus from a plutonium gun to implosion weapon stemmed partly from the huge investment already made in plutonium production. Los Alamos had little choice but to find a means of utilizing this material, but could still decide upon the style of weapon to develop.

Los Alamos constituted only one part of the Manhattan District, which employed thousands of workers, scientists, engineers, and managers. As Hughes indicates, the entire project was an industrial development-and-production undertaking dependent on scientific laboratories, such as Los Alamos, for essential technical data and theoretical understanding of many weapons-related processes. Groves had intended for the MED to function as a temporary organization, but it provided an organizational framework for any successor agency that would take control of atomic weapons development in the postwar period.

Likewise, the relationship that Los Alamos established with and its evolution into a partly autonomous facility of the MED set a precedent for how the weapons laboratory would relate to a new organization responsible for atomic energy. Still, with the end of the war came the end of Los Alamos's mission-orientation, and for several months the Laboratory lacked any well-defined technical goals to strive for. Moreover, the Laboratory's future and its overall value looked uncertain. This lack of mission created uneasiness for Los Alamos's remaining scientific staff and for its new leader, physicist Norris Bradbury.

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constructed to withstand a high-altitude drop.