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WARHEAD POLITICS
Livermore and the Competitive System of Nuclear Weapon Design

Sybil Francis
(Ph.D. Dissertation)

June 1995

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Livermore and the Competitive System of Nuclear Weapon Design

by

Sybil Francis

B.A. Chemistry
Oberlin College, 1979

Submitted to the Department of Political Science
in Partial Fulfillment of the Requirements of the Degree of

Doctor of Philosophy

at the

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June 1995

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WARHEAD POLITICS

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ABSTRACT

From the 1950s onward, the United States evolved a two-laboratory system to design, develop, and test nuclear weapons. The Los Alamos National Laboratory, located in New Mexico, dates from World War II. The founding in 1952 of what is now called the Lawrence Livermore National Laboratory in California effectively established the two-laboratory system. The decision to maintain civilian control of nuclear energy placed the laboratories under authority of the Atomic Energy Commission and its successors, while the University of California managed their operations. The armed services and Department of Defense were key actors, as was the congressional Joint Committee on Atomic Energy.

Despite essentially identical missions, Livermore and Los Alamos adopted different strategies and approaches to their task, as reflected by the number and kind of nuclear weapons each developed. Why? I looked to their joint history for explanation. How did the two-laboratory system originate and evolve? How did it function? What impact did the system have on nuclear weapons development? Extensive documentary research and interviews with participants led me to conclude that the structure of laboratory competition was key. The incentives and constraints that shaped laboratory strategies and outputs was determined by military demand for nuclear weapons, an informal mandate against laboratory duplication, congressional support for competition, and Livermore's role as the "second lab."

The first chapter provides a brief introduction to the two-laboratory system and presents the thesis questions. Chapter 2 examines the circumstances that led to the founding of Livermore. Chapters 3-9 explore the workings of the two-laboratory system, how the laboratories competed, and with what results. In addition to a review of study findings and conclusions in Chapter 10, I also discuss how the research bears on larger questions: the laboratories' role in the arms race, organizational strategies for coping with changing political environments, the dynamics of technological innovation, and the leverage of policymakers over large organizations.

Thesis Supervisor: Harvey M. Sapolsky
Title: Professor of Public Policy and Organization

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Virginia Woolf was right. A room of one's own—for thinking and writing, for developing a sense of self and work apart from others, for freedom from worldly concerns—is vital. But it must be a room, not a cell, a social space respected by those we work with and understood by family and friends. I found myself such a room and count myself blessed. My deep pleasure and sense of accomplishment in earning my Ph.D. owe a great deal to that room and to those who made it possible.

Harvey Sapolsky maintained an ideal balance between making his presence felt as my thesis advisor and letting me make my own way. Although my dissertation research and writing were conducted far from MIT, Harvey's contributions shaped the contours and substance of the thesis. A creative thinker and superb writer, Harvey set an example for which to aim. But he did more. Insightful comments proved critical turning points, and in the last months Harvey pushed me to think harder and to extract everything I could from the years invested. Harvey also understood when the process was complete, and graciously toasted me when I finished.

All three members of my committee played crucial roles in my graduate education. Long before he became my thesis advisor, Harvey persuaded me to attend MIT. George Rathjens convinced me I should pursue the Ph.D. As a committee member he was enthusiastic and generous with his time. Steve Miller's eloquence in my first course at MIT convinced me I had chosen well. His renowned clarity later helped me gain perspective on my ideas. My thanks to Harvey, George, and Steve for serving on my committee.

My MIT friends and colleagues provided both moral and intellectual support. Dale Murphy coached and cheered me through the last critical months when my energy and confidence flagged. Although I am sure I would have stubbornly continued, he was there to make sure I pulled everything together. Thanks also to Neta Crawford, Dean Cheng, and Steven Flank, who unwittingly played relay in encouraging me. Sanford Weiner provided valuable insights. Judy Spitzer and then Anne Marie Cameron always knew Harvey Sapolsky's whereabouts when I wanted to speak with him. They performed their work with humor and efficiency. Jeanne Washington provided timely information regarding the intricacies of the MIT bureaucracy and a big hug when I finished. To Owen Coté: we made it together.

Physical space and resources are fundamental. The Lawrence Livermore laboratory provided an office and funding. Michael May opened the door; a summer working with him led to my interest in the laboratory. Since then Mike has followed my work, and graciously included me in family activities with his wife Mary and daughter Barbara. Mike's intellectual interests are broad. As director emeritus of the laboratory he was a key figure in the establishment of the Center for Security and Technology studies (CSTS). The Center became my institutional home when I later returned to the laboratory to begin my thesis. Its director, Milo Nordyke, was especially concerned to promote work on the laboratory's history. Paul Brown's desire to attract young scholars to Livermore was equally important in bringing me back. Thanks to Milo for his continued interest, and for his comments on the thesis. I always enjoyed my lively discussions with Paul, and am also grateful for his comments.

Paul Chrzanowski, who succeeded Milo as director of the Center, supported my project in every way. He left me the solitude I needed to work, but was ready to act when I needed assistance. Thanks also to Paul for constructive comments on the manuscript. Karen Kimball, the CSTS Administrator, was an expert navigator of bureaucratic labyrinths and often deployed her talents to my benefit. Kristie Monica, the Center's Administrative Specialist, was a pleasure to work with, skilled, efficient, and accomplished. Thanks to Karen and Kristie for their daily encouragement and confidence in me. Beyond everything else, the laboratory gave me an extraordinary gift: peace and quiet. An institution that fosters this kind of working environment deserves special acknowledgment.

Although research and writing are solitary activities, they rely on help from others. Thanks to the archivists and historians who opened my way to the documents, providing the foundation for the thesis. I especially thank Steve Wofford, Beverly Babcock, and Jim Carothers at Livermore; Roger Anders and Terry Fehner of the Department of Energy; Roger Meade, Bob Seidel, and Jack Carter of Los Alamos; Sandy Meagher of the Office of the Secretary of Defense; and Ron Sodano at the National Archives. Dave Brown of the Livermore Classification Office was an enthusiastic consumer of the history I have written, and worked hard to ensure it could be told. He has read the thesis more times and more carefully than anyone, a real test of endurance.

Working independently, Chuck Hansen has amassed an impressive collection of declassified and unclassified documents relating to the nuclear weapons program. He generously provided me a number of them. Wilson J. (Jim) Frank opened his personal office files to me, an important source of documentation for this study given his long association with Livermore. Jim commented on a key chapter and was always ready to talk with me about his knowledge of the laboratory. Joe Keller of the laboratory supplied valuable data on the evolution of the nuclear stockpile. Admiral John T. (Chick) Hayward, who served in the Atomic Energy Commission and played a key role in the founding of Livermore, gave me transcripts of the diary he kept during this important period of the laboratory's history. Jon Todd in the Nuclear Weapons Council offices was crucial in enabling my access to the records of the Military Liaison Committee. Mark Dickinson of the Sandia National Laboratories provided information necessary for producing the figures.

I am grateful to those who graciously, even enthusiastically, agreed to be interviewed for this study and thank them for their frankness and insights. A complete list may be found in the "Interviews" section. Thanks especially to Jack Rosengren, who bore repeated interviews with good grace and carefully read a key chapter. Wally Decker, Frank Eby, Ollie Loper, Peter Moulthrop, and Larry Germain were always happy to recount their Livermore experiences and share their knowledge of the laboratory with me. The interest of my contemporaries about the history and workings of their laboratory rubbed off on me. Several whose interest fueled my own deserve special mention: Cynthia Nitta, John Futterman, Phil Duffy, Joe Sefcik, and John Ambrosiano. Herb York always seemed eager to learn about my discoveries, encouragement enough given his important role in the events and his own writing about them. Thanks to John Holzrichter of Livermore for his interest and his prodding to know when I would finish. Bill Shuler of Livermore, John Hopkins of Los Alamos, and Rob Rinne of Sandia-Livermore were interested enthusiasts. I enjoyed my discussions with Milt Gillespie and Del Bergen of Los Alamos, who provided keen insights and perspective. Frank Shelton, formerly of the Armed Forces Special Weapons Project when nuclear tests were still conducted above ground, provided valuable information. Robert Wertheim, long associated with the Navy's Special Projects Office was gracious and informative.

Other scholars and practitioners helped me along the way. Thanks to Lynn Eden, Kent Johnson, Peter Lavoy, and Jerry Johnson for their helpful comments on selected chapters, and to Hugh Gusterson for stimulating exchanges. Thanks also to Sarah Mendelson and Jessica Stern for their friendship and for commenting on selected chapters. Debbie Ball and Charles Ball stimulated my thinking. Special credit goes to Alex Gliksman for alerting me to the Livermore summer program which sparked my interest in writing about the laboratory. Scott Sagan arranged for my presentation to the Stanford Center for International Security and Arms Control Social Seminar Series. Also thanks to Gabrielle Hecht and the Bay Area Technology and Culture Discussion Group for a helpful discussion. I am grateful to the Nuclear History Program for the opportunity to present my work, and especially to Allen Greb for his friendship.

Completing a thesis engages not only the mind, but body and spirit. To my computer, which triumphed over Murphy's laws, I owe a great debt. My noontime swims kept me fit and happy. My gratitude goes to the Livermore laboratory pool and staff for that. Thanks to my pool pals for their companionship, especially to Max, Leslie, Carol, Susan, Christy, Chris, Aline, Jose, and Angela. Thanks also to Gigi and Tony for keeping an eye on my progress. The warmth of friends saw me through, not to mention delicious meals we shared. Heartfelt thanks to Rocky Nevin, Charles Ferguson, Irene Jacobson, Mike Lippitz, Paul Mora, and Harry Cartland for breakfasts, dinners, and discussions. Special thanks also to Herb Lin, Liza Tucker, Rob English, Virginia Miller, Tad Homer-Dixon, Laura Reed, Seth Shulman, Skip Stiles, John McCormick, Karen Wise, Dave Lynch, Alain Kondjoyan, and Shira Steinberg for their friendship and encouragement from afar. Lila Coulter walked this path with me. The contours of the room emerged under her gentle guidance. Lila's careful ear, attentive reflections, and steadfast presence were spurs to growth and understanding. The lessons go with me Lila. Thank you.

Despite allegations to the contrary, writing a thesis is not all work and no play. David Dearborn supplied much of the adventure: flying, rock-hunting, and stargazing. He inspired my trip to the Nevada Test Site in September 1992 for what turned out to be Livermore's last nuclear test. Thanks to Tom Thomson, Richard Ward, Joe Bauer, and Tom Kelleher for making the trip so much fun. David also helped me produce the figures that appear in the thesis and commented on several chapters. Besides, I could hardly have resisted his solid presence and irrepressible energy even as I hunkered down to finish. Thanks, Dearborn. Tu tambien.

Families are who we are and where we begin. From mine I learned the importance of education. My youngest sister Gaëtane, committed, determined, and with deep empathy, set me an inspiring example. With Corinna-Barbara, I shared intellectual interests, while her love and wise counsel guided me. My mother, Marie-Cécile Louvet, is also my close friend. She has spent a lifetime teaching others, and with her husband Leonard Maximon formed my cheering section. Their confidence lifted me when I needed it. From my father John Francis I learned to ask questions, and to keep asking. I thank him and his wife Elaine Fultz for their intelligent interest in my work and their son John-Kirk for lessons about growing up. My grandparents, Alice and Roger Louvet, believed in education and supported it. I miss them, as I do my grandfather John Francis, who would have been proud. My grandmother, Martita Francis, always urged me on. Thank you, Tita, for seeing me through.

My deepest thanks go to four people who most directly shaped my dissertation experience. Only they can say why they invested so much time and effort in a young scholar and her work. I simply know they were crucial. To them I owe many of my dissertation's virtues. Such infelicities or errors as remain are my own.

George Bing provided an island of safety in a new and unfamiliar environment. He entertained all questions, and I asked a lot, knowing little of the laboratory's history upon my arrival. George also helped my study stay firmly grounded in observation. Curiosity aroused by our discussions would often prompt him to research the matter, draw a chart, or make a calculation, the results of which he would share with me. He provided much appreciated sustenance of another kind: Peet's coffee and a weekly croissant. Thanks also to George for constructive comments on the manuscript. He has become more than a colleague. He and his lovely wife Virginia are also my friends.

From the start, Bart Bernstein made me feel my ideas were worth listening to, novice historian though I was and he a senior professor at Stanford. Sharing a passion for the details of the origins of the Livermore laboratory, we had many long and lively discussions. But our conversations did not end there. He revealed a keen understanding of such other critical issues as the intricacies of poolside etiquette and the joys of a good burrito. His dry wit and kindness sealed our friendship. Bart commented on early chapters and generously shared documents with me. The consummate teacher and scholar, he encouraged my thinking and was genuinely interested in my findings.

Robert (Bud) Budwine was tireless and patient in instructing me in the technical aspects of nuclear weapon design and provided valuable comments on early chapters. It was always a pleasure to walk down the stairs and up the hall to Bud's office for a session dissecting the technical and political dimensions of weapon development. He not only tolerated my political science orientation, but grew genuinely interested in how I thought about problems. His independent thinking and integrity opened the way for our broad ranging discussions. On a more personal note, Bud made me an honorary member of his family, and I have enjoyed knowing his daughter Rhoda and wife Christine.

Livermore laboratory historian and chef extraordinaire, Bart Hacker read the manuscript closely, suggested countless books and articles to read, often loaned from his vast library or copied himself, and never tired of my questions. A superb and enthusiastic listener, Bart seemed to relish our discussions as much as I did, and I hope he learned from me as much as I from him. Bart provided an entree into the community of historians of science and technology, having spent a lifetime in that world. He reminded me of the value of what I was doing when I lost sight of it. As if this were not enough, he corrected footnotes, insisted on active verbs, and deleted more than a few prepositional phrases. His efforts even extended to helping me lay out the page design. A truer friend and colleague could not be found.

My journey began in Washington where I worked for George Brown. Member of Congress from California, humanist, and thinker, George embodied probity, commitment and hard work. Because I so greatly admired him, my decision to leave was no easy one. We had worked well together and enjoyed successes. George eased the transition, wishing me well when I departed for the new challenge of graduate school. Then truly began the search for a room of my own.

Sybil Francis
Livermore, March 1996

CONTENTS

ABBREVIATIONS

AC&F	American Car and Foundry
adm.	admiral
AEC	Atomic Energy Commission
AFHD	Air Force Historical Division
AFOAT	Air Force Office of Atomic Energy
AFWEP	Armed Forces Special Weapons Project
AIAA	American Institute for Aeronautics and Astronautics
ALOO	Albuquerque Operations Office (AEC)
appx.	appendix
APSA	American Political Science Association
ARDC	Air Research and Development Command
ASN(R&D)	Assistant Secretary of the Navy for Research and Development
ATSD(AE)	Assistant to the Secretary of Defense for Atomic Energy
ATSD(SA)	Assistant to the Secretary of Defense for Systems Analysis
AUI	Association of Universities, Incorporated
B-	Bomb-[number]
BAS	*Bulletin of Atomic Scientists*
BuOrd	Bureau of Ordnance (USN)
CAFH	Center for Air Force History
CalTech	California Institute of Technology
CBO	Congressional Budget Office
CNSS	Center for National Security Studies (LANL)
CNSSM	Center for National Security Studies at Maryland
col.	colonel

comm.	committee
CR&D	California Research and Development Corporation
CRS	Congressional Research Service
CSIA	Center for Science and International Affairs
CTB	Comprehensive Test Ban
DARHT	Dual Axis Radiographic Hydrodynamic Test Facility
DDR&E	Director of Defense Research and Engineering
distrib.	distribution
DMA	Division of Military Application (AEC)
DNA	Defense Nuclear Agency
DOE	Department of Energy
DOD	Department of Defense
ed.	edition; or edited by
ERAB	Energy Research Advisory Board
ERDA	Energy Research and Development Administration
FBM	Fleet Ballistic Missile
GAC	General Advisory Committee (AEC)
GAO	General Accounting Office
gen.	general
GLCM	Ground Launched Cruise Missile
GPO	Government Printing Office
HAI	History Associates Incorporated, Rockville, Md.
HMSO	Her Majesty's Stationery Office
HSPS	*HSPS: Historical Studies in the Physical and Biological Sciences*
ICF	Inertial Confinement Fusion
IFPA	Institute for Foreign Policy Analysis, Inc.
IGCC	Institute for Global Conflict and Cooperation (Univ. of California)
IS	*International Security*
JCS	Joint Chiefs of Staff
JCAE	Joint Committee on Atomic Energy
LASL	Los Alamos Scientific Laboratory
LANL	Los Alamos National Laboratory
LBL	Lawrence Berkeley Laboratory
LLL	Lawrence Livermore Laboratory
LRL	Lawrence Radiation Laboratory
LLNL	Lawrence Livermore National Laboratory
LTBT	Limited Test Ban Treaty
MIRV	Multiple Independently-targeted Reentry Vehicle
MLC	Military Liaison Committee
mm	millimeter(s)
MP	Measurements Project
MTA	Materials Test Accelerator
MX	Missile Experimental
n(n).	note(s)

NAS	National Academy of Sciences
NASA	National Aeronautics and Space Administration
NBS	National Bureau of Standards
NHP	Nuclear History Program
NSC	National Security Council
NVO	Nevada Operations Office (DOE)
OAFH	Office of Air Force History
OAR	Office of Aerospace Research
OHHR	Office of History and Historical Records (LLNL)
OMA	Office of Military Application (DOE)
Ord	Ordnance
OTA	Office of Technology Assessment (U.S. Congress)
p(p).	page(s)
PSAC	President's Science Advisory Committee
PW	Project Whitney
R&D	Research and Development
RD&T	Research, Development, and Test
RB	Reentry Body
RG	Record Group
RV	Reentry Vehicle
SDI	Strategic Defense Initiative
SEAB	Secretary of Energy Advisory Board
SAN	San Francisco Operations Office (DOE)
sect.	section
SLL	Sandia Laboratories Livermore
SNL	Sandia National Laboratories
SPO	Special Projects Office (USN)
SSPO	Strategic Systems Project Office (USN)
subcomm.	subcommittee
TID	Technical Information Department (LLNL)
U-235	Uranium 235
UCRL	University of California Radiation Laboratory
Univ.	University
USA	United States Army
USAF	United States Air Force
USN	United States Navy
vol(s).	volume(s)
W-	Warhead-[number]

1. THE TWO-LABORATORY SYSTEM
Organizational Strategies in Competitive Environments

From the 1950s onward, the United States evolved a two- laboratory system to design, develop, and test nuclear weapons. The Los Alamos National Laboratory, located in New Mexico, dates from World War II. The founding in 1952 of what is now called the Lawrence Livermore National Laboratory in California effectively established the two-laboratory system. Despite essentially identical missions, they adopted different strategies and approaches to their task. Why? I looked to the joint history of the laboratories and the structure and dynamics of laboratory competition for an explanation. How did the two-laboratory system originate and evolve? How did the system function? What impact did it have on the number and kind of nuclear weapons developed?

There are few historical accounts and little analysis of these important U.S. Cold War institutions.[1] The official histories sponsored by the Atomic Energy Commission and its successors

1. On Los Alamos' development of the first atomic bombs see: David Hawkins, "Towards Trinity," in *Project Y: The Los Alamos Story*, Vol. 2 of "History of Modern Physics 1800-1950" (Los Angeles and San Francisco: Tomash, 1983); Lillian Hoddeson et al., *Critical Assembly: A Technical History of Los Alamos During the Oppenheimer Years, 1943-1945* (New York: Cambridge Univ. Press, 1993); Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon & Schuster, 1986). On the postwar environment for nuclear weapons see Gregg Herken, *The Winning Weapon: The Atomic Bomb in the Cold War, 1945-1950* (New York: Vintage Books, 1982); Martin J. Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (New York: Alfred A. Knopf, 1975).

include discussions of both laboratories and were immensely valuable resources for this study.[2] A number of participants have written memoirs. Their accounts touch on their experiences with the nuclear weapon design laboratories.[3] Several scholars are interested in the laboratories, including an anthropologist who studied contemporary nuclear weapons designers at Livermore.[4]

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2. See Richard G. Hewlett and Oscar E. Anderson, *The New World, 1939/1946* (Berkeley: Univ. of California Press, 1990); Hewlett and Francis Duncan, *Atomic Shield, 1947/1952* (Berkeley: Univ. of California Press, 1990); Hewlett and Jack M. Holl, *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission* (Berkeley: Univ. of California, 1989). These are respectively vols. 1, 2, and 3 of "A History of the United States Atomic Energy Commission."

Both laboratories have produced anniversary publications, useful sources of basic information. On Livermore, see: "20 Years in Livermore," *Newsline* [LLL] 3 (Sept. 1972), entire issue; "LLL: 1952-1977," *Newsline* [LLL] 8 (August-Sept. 1977), entire issue; "Lawrence Livermore National Laboratory: Its History, Its Work, and Its Relationship with the University of California and the Community," TID report LLL-TB-017, March 1980; "Thirty Years of Technical Excellence: Lawrence Livermore National Laboratory, 1952-1982," n.d. (c. 1982); "Defense and Energy: Meeting the Challenge," in "Thirty-fifth Anniversary Special Issue," *The Quarterly* [LLNL] 18 (Oct. 1987); "The Laboratory Celebrates its 40th Anniversary A Heritage of Excellence, a Commitment to the Future," *The Quarterly* 23 (Sept. 1992), entire issue; "Documentation and Assessment of the History of the Lawrence Livermore National Laboratory Livermore Facility, and Site CA-SJO-173H. The Carnegie Town Site at Lawrence Livermore National Laboratory Site 300, Alameda and San Joaquin Counties, California," Subcontract No. M-0191-E1, by William Self Associates, August 1992.

On Los Alamos see: "The First Twenty Years at Los Alamos: January 1943-January 1963," *LASL News* 5 (Jan. 1963), entire issue; "The Evolution of the Laboratory: Los Alamos at 40," *Los Alamos Science* 4 (Winter/Spring 1983), entire issue; "The Laboratory's 50th Anniversary," *Los Alamos Science* 21 (1993).

For comparative purposes, see: Lorna Arnold, *A Very Special Relationship: British Atomic Weapon Trials in Australia* (London: HMSO, 1987); Marcel Duval and Yves Le Baut, *L'Arme nucléaire française: Pourquoit et comment?* (Paris: Kronos, 1992); David Holloway, *Stalin and the Bomb: The Soviet Union and Atomic Energy, 1939-1956* (New Haven: Yale Univ. Press, 1994); John Wilson Lewis and Xue Litai, *China Builds the Bomb* (Stanford: Stanford Univ. Press, 1988); Georges Henri Soutou, *The French Military Program for Nuclear Energy, 1945-1981*, trans. Preston Niblack, NHP Occasional Paper 3 (College Park: CNSSM, 1989).

3. Gordon E. Dean, *Forging the Atomic Shield: Excerpts from the Diary of Gordon E. Dean*, ed. Roger Anders (Chapel Hill: Univ. of North Carolina Press, 1987); John McPhee, *The Curve of Binding Energy: A Journey into the Awesome and Alarming World of Theodore B. Taylor* (New York: Noonday Press, 1973); Duane Sewell, "The Branch Laboratory at Livermore During the 1950s," in *Energy in Physics, War and Peace: A Festschrift Celebrating Edward Teller's 80th Birthday*, ed. Hans Mark and Lowell Wood (Dordrecht: Kluwer Academic Press, 1988), pp. 319-326; Edward Teller with Allen Brown, *The Legacy of Hiroshima* (London: Macmillan, 1962), pp. 58-76; Herbert F. York, *The Advisors: Oppenheimer, Teller, and the Superbomb* (Stanford: Stanford Univ. Press, 1976); Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987).

4. On the founding of Livermore and the weapons programs of the two laboratories see three papers presented at the workshop on the "Decade of Innovation: Los Alamos, Livermore, and National Security Decisionmaking in the 1950s," Pleasanton, Cal., 19-21 Feb. 1992: Barton J. Bernstein, "Teller, Lawrence, and a New Lab"; Terrence Fehner, "The World is Bigger than Los Alamos and Livermore: The Nuclear Weapon Laboratories during the Eisenhower and Kennedy Administrations, 1958-1962"; and Sybil Francis, "Workhorses vs. Racehorses: Competition Between the Nuclear Weapons Labs in the 1950s." See also Barton C. Hacker, "A Modest Proposal: Berkeley and the Second Laboratory, 1949-1954," presented at the Symposium on Postwar Big Science and Technology, XIXth International Congress of the History of Science, Zaragoza, 22-29 August 1993. For an anthropologist's view of Livermore, see Hugh Gusterson, "Testing Times: A Nuclear Weapons Laboratory at the End of the Cold War," Ph.D. diss., Stanford Univ., 1991; for a journalist's, William J. Broad, *Star Warriors: A Penetrating Look into the Lives of the Young Scientists behind Our Space Age Weaponry* (New York: Simon & Schuster, 1985).

And numerous government studies have addressed various management issues related to the laboratories.[5] None of these studies analyzes the origins, history, and functioning of the two-laboratory system for the design and development of nuclear weapons, the subject of this analysis.

This first chapter introduces the two-laboratory system and the questions explored. Study questions are framed in the context of the theoretical literature, to be revisited in the final chapter. Chapter 2 examines the circumstances that led to the founding in 1952 of the Livermore laboratory. Chapters 3-9 explore the workings of the two-laboratory system. How was the competition between the laboratories waged, and with what consequences? The study concludes with a discussion in Chapter 10 of the findings, and some observations about the sources of technical innovation, the leverage of policy makers over large organizations, the role of the laboratories in the arms race, and their future.

The Nuclear Weapon Design System

Los Alamos was established in 1943 as part of the Manhattan Project to develop the first atomic bomb. Livermore was founded nine years later, in 1952. Together, Livermore and Los Alamos designed and developed the prototypes which provided the basis for the production of the tens of thousands of nuclear weapons deployed by the United States since World War II. The decision to maintain civilian control of nuclear energy placed the laboratories under authority of the Atomic Energy Commission (AEC) and its successors, while the University of California managed their operations. In addition to the design laboratories, the complex included facilities for fabricating nuclear material and high explosives, for nuclear weapon testing, assembly, and disposal.[6] The Sandia National Laboratories were responsible for arming, fuzing, and other non

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5. These include, e.g., in chronological order: JCAE, *The Future Role of the Atomic Energy Commission Laboratories* (Washington, D.C.: GPO, 1960); ERDA, *Funding and Management Alternatives for ERA Military Application and Restricted Data Functions*, with appendices, ERDA-97B, Jan. 1976; Albert H Teach, "Bureaucracy and Politics in Big Science: Relations Between Headquarters and the National Laboratories in AEC and ERA," presented at the APSA annual meeting, Washington, D.C., 1977; OTA, *National Laboratories: Oversight, Legislative, and Authorization Issues* (unpub. draft), 27 Nov. 1978; DOD/DOE Long Range Resource Planning Group, chaired by Alfred D. Starbird, *Long Range Nuclear Weapon Planning Analysis* [Starbird Study], 15 July 1980; SEAB, "A Report to the Secretary on the Department of Energy National Laboratories," July 1992; ERAB, *The Department of Energy Multiprogram Laboratories*, Report DOE/S-0015, 3 vols., Sept. 1982; Office of the President, Univ. of California, Blue Ribbon Task Group on Nuclear Weapons Program Management, chaired by William Clark, *Report*, Berkeley, July 1985; DOE, Nuclear Weapons Complex Modernization Report, Jan. 1985; DOE, *Nuclear Weapons Complex Reconfiguration Study*, Report DOE/DP-0083, Jan. 1991; GAO, *Department of Energy National Laboratories Need Clearer Missions and Better Management*, Report GAO/RCED-95-10, Washington D.C., Jan. 1995; SEAB, Task Force on Alternative Futures for the Department of Energy Laboratories, chaired by Robert W. Galvin [Galvin Committee], *Alternative Futures for the Department of Energy National Laboratories*, 2 vols., Washington, D.C., Feb. 1995.

6. For a good overview and introduction to the U.S. nuclear weapon design, research, test, and development complex, see Thomas B. Cochran et al., *U.S. Nuclear Warhead Production* and *U.S. Nuclear Warhead Facility Profiles*, vols. 2 and 3 of "Nuclear Weapons Databook" (Cambridge, Mass.: Ballinger, 1987).

nuclear weapon components as well as for the interface between the warhead and delivery system. Concerned as it is with the nuclear design process, this study will not address the Sandia laboratories in depth, though they served critical functions and worked closely with the design laboratories.[7]

Los Alamos and Livermore did not compete directly for funding but rather for weapon development assignments. Responsibility for weapon development did not usually confer additional funding since programs were operated at a level of effort. It did legitimize laboratory claims on resources. But too many weapons assignments might detract from general research, jeopardizing the chances for winning future assignments. Accordingly, the laboratories tried to balance weapon development responsibilities with general weapons research.

The development of nuclear weapons for military users was the laboratories' principal mission. A major objective of this study is to explain the dynamics of laboratory competition for weapon development assignments. Neither the Department of Defense nor the armed services paid for the nuclear warheads produced by the AEC complex, a consequence of the decision made after World War II to place nuclear weapons design and development under civilian control.[8] Warhead design, development, production, and retirement costs were incurred by the AEC. The military paid only for delivery systems. Authority for making weapon development assignments resided with the AEC, although the military had obvious interests in the outcome. This study examines their role in shaping weapon developments.[9]

Together, Livermore and Los Alamos designed almost ninety nuclear warheads (Figure 1).[10] Of these, only 63 were actually deployed because 26 were cancelled before they reached stockpile (Table 1). Los Alamos developed and deployed a total of 46 warhead types since 1945. Taking 1957 as the starting point, five years after the founding of Livermore, Los Alamos designed

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7. On Sandia's early years, see Necah Stewart Furman, *Sandia National Laboratories: The Postwar Decade* (Albuquerque: Univ. of New Mexico Press, 1990); on its Livermore branch, Sandia National Laboratories, *8100 Directorate: The First Thirty Years* (Livermore, c. 1986).

8. See *Nuclear Regulatory Legislation through the 95th Congress, 2nd Session*, Appendix 4, "The Atomic Energy Act of 1946 with Amendments through the 83rd Congress (1st Session)" (Washington: GPO, 1979), pp. 284-305, 319-324.

9. The process of nuclear weapon approval, development, and deployment is summarized in "How Nuclear Weapons Are Authorized The Paper Trail," appx. A to Donald R. Cotter, "Peacetime Operations: Safety and Security," in *Managing Nuclear Operations*, ed. Ashton B. Carter, John D. Steinbruner, and Charles A. Zraket (Washington, D.C.: Brookings Institution, 1987), pp. 57-59.

10. Nuclear warheads and bombs are assigned numbers, such a W-68 or B-61. Substantial modifications are sometimes made without renaming the warhead and assigning it a different number. In this sense, the number of warheads designed and developed might be considered to be higher than the figures given.

27, Livermore 17, of the 44 warhead types deployed in the stockpile (Figure 2). Counting only warheads deployed in 1960 and after, the laboratories are about even: Livermore 16; Los Alamos 17 (Figure 2). These figures can only provide rough estimates of the productivity of the laboratories, given that extensively modified warheads were not necessarily renumbered. This was the case with the Los Alamos-designed B-28, for example, which was produced in numerous variations and yields.[11] Livermore-designed warheads were deployed in weapon systems that tended to be less long-lived in the stockpile, an average of just over 17 years, compared to an average of almost 19 years for Los Alamos warheads, taking weapons deployed in 1958 as the starting point (Table 1).[12] The U.S. produced tens of thousands of nuclear weapons based on laboratory designs. Livermore's overall share of total nuclear warheads in the stockpile reached a high of approximately 40 percent in the late 1970s (Figure 3). As of Fiscal Year 1992, Los Alamos warheads represented 85 percent of the total in the stockpile, to Livermore's 15 percent (Figure 3). Implementation of the Strategic Arms Reduction Treaty (START I) will leave five Los Alamos warheads in the stockpile to Livermore's four.[13]

These numbers are small enough that relying on them to tell the story about Livermore and Los Alamos would be a mistake. They are not meaningful absent context and interpretation. Rather, they help raise questions and indicate avenues of research. Other parameters of interest are harder to quantify. For example, Livermore has a reputation for being technically innovative, while Los Alamos is reputed to be more conservative in its approach to weapons development. Livermore has often been associated with weapons systems surrounded by political controversy, including the neutron bomb, the MX missile, and the Strategic Defense Initiative.

Did the laboratories drive the arms race? What are the sources of technical innovation in nuclear weapons design? Can policy makers exert control over large technical bureaucracies, particularly if they don't understand technology? Can competition help policy makers gain leverage over large organizations? How did the laboratories compete for resources and seek autonomy? Scholars have considered questions similar to these, though not in the context of the nuclear weapons laboratories. Their approaches helped illuminate my own research.

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11. Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), pp. 150-152.

12. Calculations based on figures in Table 1.

13. Under START I, Los Alamos-designed nuclear weapons remaining in the stockpile will be the B-61 tactical and strategic bombs, W-76 Trident C-4 warhead, W-78 Minuteman III warhead, W-80 Tomahawk and ALCM warhead, W-88 Trident II warhead. Livermore-designed systems will be the W-62 Minuteman III warhead, B-83 strategic bomb, W-84 GLCM warhead, W-87 MX warhead.

Testing Assumptions

Critics of the two-laboratory system have argued that laboratory competition helped fuel the arms race, driving the laboratories to propose unnecessary, redundant, and even dangerous new weapons systems. The end of the Cold War has not halted the process, or, as one critic put it, new laboratory proposals have "moved steadily forward, acquiring internal legitimacy through bureaucratic survival and expansion . . . Scientists and nuclear planners have . . . pursue[d] their own ideas, creating their own reality about the need for new weapons.[14] In *How Nuclear Decisions Are Made*, Scilla McLean despaired of controlling military technology. She believes that "runaway technologies [and] scientific passions" are an important part of the problem.[15] So are large bureaucracies. Assured they will remain in operation long after legislators are gone, they circumvent the will of policy makers. Acknowledging that interservice rivalries can inflate military demand for new weapons, including nuclear, McLean still finds scientific and technical momentum the fundamental problem driving the arms race. Although she does not discuss the nuclear design laboratories explicitly, these would certainly come under her definition of the problem.

Matthew Evangelista's model of military innovation also takes as its starting point a form of technological determinism, at least in his U.S. cases. Innovation in the United States (he also considers the Soviet case, which operates differently) is a "bottom-up" process, initiated by scientists and carried up through the bureaucracy. Bureaucratic interests are a filter through which scientific and technical entrepreneurs must pass to achieve their goals. Here, too, however, technology seems to arise independently, with no other logic than the interest of scientists. This is why Evangelista has difficulty explaining why scientists might disagree among themselves as they did over H-bomb development.[16]

These models of technical innovation and the nuclear weapon development process contain implicit assumptions about the laboratories: that they largely operate outside social or political control. Technology is portrayed as an independent and self-perpetuating force. The nuclear weapons laboratories emerge as engines of the arms race, although even the most critical observers would agree the truth is more nuanced and complex.

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14. William Arkin, "Nuclear Junkies: Those Lovable Little Bombs," *BAS* 49 (July-August 1993): 22-27, at 25, 27.

15. *How Nuclear Weapons Decisions are Made*, ed. Scilla McLean (London: Macmillan, 1986), pp. 257-258.

16. Matthew Evangelista, *Innovation and the Arms Race: How the United States and the Soviet Union Develop New Military Technologies* (Ithaca: Cornell Univ. Press, 1988). For a discussion of the bottom-up process of technical innovation see chap. 3, especially p. 52.

Proponents of the two-laboratory system offer a different view, one animated by belief in rationality and objectivity. Each laboratory reviews the other's weapon design proposals in a process more like idealized scientific peer review than an exercise in bureaucratic politics. The process is supposed to encourage technical innovation and ensure a safe, reliable, and secure nuclear weapons stockpile. To do otherwise risks all of these things, so proponents argue. A senior Livermore scientist warned as recently as 1990, for example, that to give one laboratory a monopoly in nuclear-weapon expertise would inhibit proper assessment of technical and policy issues. "There would . . . be no credible source of an expert second opinion. . . . A single center of expertise would have no competing center to challenge its judgments. . . . The stimulation of competition is extremely important. Without it a laboratory would become more complacent and less productive."[17]

Of the two laboratories, Livermore has been the most ardent proponent of the two-laboratory system, and we shall see why. In short, Livermore's nuclear weapon program has always been, and continues to be, most vulnerable to changes in national priorities. Livermore has thus disproportionately carried the burden of justifying the two-laboratory system.[18] This study explains why.

Proponents and critics of the two-laboratory system have one thing in common. Both downplay the important interaction of the laboratories with other organizations. But the nuclear weapon design laboratories did not operate in a vacuum. They cannot be understood independently of other organizations with which they interacted and on which they depended for their mission, resources, and legitimacy. The organizations comprising the two-laboratory system of nuclear weapon design and development included the military, the Congress, and the Atomic Energy Commission. Nuclear weapons development, as this study will show, was driven by military customers, mediated by the Atomic Energy Commission, shaped by the laboratories' drive to survive, and reinforced by congressional support for civilian control of the nuclear weapons complex.

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17. Jack W. Rosengren, "Reconfiguration of Nuclear-Weapon RD&T: Why Two Design Laboratories," unpublished paper, 18 Oct. 1990, p. 4.

18. See GAO, "Nuclear Weapons Complex: Issues Surrounding Consolidating Los Alamos and Lawrence Livermore National Laboratories," Report GAO/T-RCED-92-98, statement of Victor S. Rezendes, Director, Energy and Science Issue, Resources, Community, and Economic Development Division before the House Comm. on Science, Space, and Technology, 24 Sept. 1992, pp. 2, 20, for suggestion that the weapons program be consolidated at Los Alamos. A CBO study also indicates a preference for retaining Los Alamos as the sole laboratory responsible for weapons design and stockpile stewardship responsibilities should a choice have to be made; see *The Bomb's Custodians*, CBO Papers, July 1994.

Organizations and Uncertainty

My theoretical focus is organizations: how they define and defend their domain of activities in bureaucratic arenas, their strategies of survival and adaptation, and how they deal with uncertainty. Students of organizations have frequently observed that coping with uncertainty is the central problem for complex organizations. Uncertainty is presented to organizations from a variety of sources, external and internal. External uncertainty can arise from lack of clarity in cause and effect relationships in the society at large. Sources of external or environmental uncertainty also stem from contingency, the impact of other organizations or elements of the environment over which the focal organization has no control.[19] The greater the complexity of the operating environment or the number of other environmental entities with which the focal organization interacts, the greater the potential for uncertainty. Another way of thinking about this is to consider the level of interconnectedness of the focal organization, or the extent to which its fate is linked to other organizations whose actions may impinge upon it.[20]

Controlling uncertainty is the fundamental organizational problem, and all organizations seek self-control or the ability to act independently of environmental forces.[21] Not all organizational theorists, however, believe organizations are capable pf develop strategies for dealing with uncertainty. Put in other terms, not all scholars agree that organizations are able to adapt to their environment. Population ecologists, for example, model organizational life and death on biological evolution. They stress the inertia of organizational forms and the difficulty or impossibility of organizational change. The principal mechanism for change is thus environmental selection, which leads to the demise of some organizations and the creation of new ones better suited to the environment. [22]

The ecological school studies long-term trends and large numbers of organizations. The resource dependency perspective, on the other hand, treats organizations as capable of adapting to their environment. The most comprehensive development of the resource dependence approach is found in the work of Jeffrey Pfeffer and Gerald R. Salancik.[23] They and other resource

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19. E.g., James D. Thompson, *Organizations in Action: Social Science Bases of Administrative Theory* (New York: McGraw-Hill, 1967), pp. 159-160.

20. W. Richard Scott, *Organizations: Rational, Natural, and Open Systems*, 3rd Edition (Englewood Cliffs, N.J.: Prentice-Hall, 1991), pp. 134-135.

21. As discussed in Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge: Harvard Univ. Press, 1972), p. 252.

22. *Organizational Sociology*, ed. W. Richard Scott (Aldershot, Ill.: Dartmouth, 1994), pp. 113-114.

23. Jeffrey Pfeffer and Gerald R. Salancik, *The External Control of Organizations: A Resource Dependence Perspective* (New York: Harper & Row, 1978).

dependency theorists assume organizational actors can choose and create organizational structures best suited to given environments. They also assume that such strategies have some probability of success. In other words, organizations can make choices that can alter their fate. The resource dependency school is best suited for considering individual or small numbers of cases over a relatively short period of time, i.e. shorter than the life of the industry in which they are situated.[24]

An important source of uncertainty for organizations is their dependence on the environment for resources. The laboratories relied directly on the AEC and the Congress for their funding, and indirectly on the military. An organization's dependence on some element of its environment is in proportion to its need for resources which that element can provide.[25] Organizations will thus devise strategies for achieving predictability and self-control over the uncertainty caused by their interdependence with the environment.

If uncertainty is the fundamental problem for complex organizations, coping with uncertainty is the essence of the administrative process.[26] The three levels of organization Thompson describes are the technical, managerial, and institutional. The technical level, sometimes called the technical core, is the part of the organization which produces outputs from inputs. The managerial level is responsible for designing and controlling the production system, for procuring inputs, producing outputs, and for securing and allocating personnel. The institutional level relates the organization to its wider environment, determines its domain, establishes its boundaries, and secures its legitimacy.[27] Uncertainty is greatest at the institutional level because this is where organizational managers interact with the environment. The organization has no formal authority or control over the environment, yet relies on it for necessary resources.[28] Organizational strategies of survival, expansion, and growth are devised at the institutional level, where their implementation is directed. Organizational administrators and managers assess the environment, communicate their intentions to external actors, and give direction to internal actors.

Scholars have described a number of organizational strategies to manage environmental uncertainty. One approach consists of strategies designed to buffer the organization's technical core: building protective boundaries between the activities of the technical core and uncertainties deriving from both the environment and internal sources. A second group of strategies involves

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24. For uses of each school, see Scott, *Organizations*, pp. 217-218.

25. Thompson, *Organizations in Action*, p. 30.

26. Ibid., p. 159.

27. Ibid., pp. 10-12.

28. Ibid., p. 12.

boundary modification, in which the organization reduces its dependence on the environment by altering its relationship to it.[29] Such strategies might include vertical integration, growth, cooptation, diversification, and negotiation.[30] Although these are terms generally applied to the study of firms operating in competitive market environments, we shall see that government sponsored organizations like the laboratories can exhibit behaviors similar to private firms. The economist F. M. Scherer analyzed the nuclear arms race, examining key events and decisions between 1939 and 1956 from the perspective of oligopoly theory, largely by analogy.[31] Scherer also analyzed the weapons acquisition process using the economist's techniques. While recognizing his cases fell short of the essentials of an ideal market system, Scherer hoped to show that economists could illuminate international relations and weapons acquisition processes.[32] Concepts developed in analyzing competing private firms might thus fruitfully be applied to analyzing government organizations as long as the limitations are recognized. Terms like customer, supplier, and product differentiation, are most often used in the context of studying the private sector.[33] While recognizing their limitations in the context of government bureaucracies, they can nevertheless do useful work and will be used in this study where appropriate.

To Compete or to Cooperate?

The laboratories operated in an environment of competitive interdependence, defined as the condition in which two or more organizations compete for the resources of another party. Under these conditions, the survival of each organization is tied to the actions of others over which it has no formal authority or control.[34] Organizations sometimes compete in order to reduce the uncertainty of competitive interdependence, sometimes they cooperate. This study adopts the definition of cooperation developed by Thompson as the mutual commitment of two or more or-

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29. Scott, *Organizations*, p. 194.

30. See, e.g., Scott, *Organizations*, chap. 8; Jeffrey Pfeffer, "Merger as a Response to Organizational Interdependence," pp. 123-135, in Scott, *Organizational Sociology*.

31. F. M. Scherer, "The Nuclear Weapons Race: A Case Study of Behavior Under Conditions Analogous to Oligopoly," unpublished paper, n.d., c. 1963.

32. Frederic M. Scherer, *The Weapons Acquisition Process* (Cambridge: Harvard, 1962), p. 57. Scherer is also the author, with David Ross, of *Industrial Market Structure and Economic Performance*, 3rd ed. (Dallas: Houghton Mifflin Company Boston, 1990); Scherer, "The Nuclear Weapons Race," n.d., c. 1963, p. 1.

33. See, e.g., Michael E. Porter, *Competitive Strategy: Techniques for Analyzing Industries and Competitors* (New York: Free Press, 1980).

34. Scott, *Organizations*, p. 198.

ganizations to exchange the capacity to reduce uncertainty.[35] Similar organizational goals and functions fosters competition.[36] Competition creates uncertainty because it increases contingency. That is why organizations will try to avoid it, either by cooperating or by differentiating their goals and functions, if possible. In their study of the behavior of firms, for example, Richard M. Cyert and James G. March observed that organizations in competitive duopolies seek to establish negotiated environments.[37] Pfeffer and Salancik similarly argued that cooperation was a typical solution to the uncertainties created by organizational interdependence.[38] Of course, cooperation creates its own uncertainties, since it involves the mutual control of organizations over each other.

Of what concern is competition to policy makers? While competition is seen as a good in the private sector, it is often viewed as wasteful in government. But competition among government bureaucracies can also drive innovation. It is a commonly recognized way for compelling alternative policy options from otherwise secretive bureaucracies. Interservice rivalry, for example, has been credited—or blamed—for shaping U.S. strategic doctrine and weapons procurement.[39] What Aaron Wildavsky has called the institutionalization of advocacy can therefore be a tool, albeit a blunt one, for generating policy options.[40] Arnold Kanter argues similarly. He suggests that rather than trying to overcome bureaucratic rivalries, policy makers should exploit them. They can do this by paying attention to how relationships among bureaucracies and their sponsors are structured. In his own work, Kanter analyzed the causes of split Joint Chiefs of Staff (JCS) decisions.[41] Divisions permit, and Kanter suggests even compel, the administration to intervene in making decisions when its military advisors disagree. Confronted with divided

35. Thompson, *Organizations in Action*, p. 34-35.

36. William M. Evan, "The Organization-Set: Toward a Theory of Interorganizational Relations," in *Approaches to Organizational Design*, ed. James D. Thompson (Pittsburgh: Univ. of Pittsburgh Press, 1966), pp. 173-191.

37. Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963).

38. Pfeffer and Salancik, *The External Control of Organizations*, p. 43.

39. See, e.g., Edmund G. Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York: Columbia Univ. Press, 1976); Arnold Kanter, *Defense Politics: A Budgetary Perspective* (Chicago: Univ. of Chicago Press, 1979); Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge: MIT Press, 1990).

40. Aaron Wildavsky, *The New Politics of the Budgetary Process* (Glenview, Ill.: Scott, Foresman, 1988), p. 355.

41. Kanter, *Defense Politics*.

military counsel, the administration can pick and choose among the menu of recommendations generated by the articulation of different viewpoints.[42] It is just for this reason the JCS prefers unanimous decisions. Split decisions expose the JCS to outside scrutiny, weakening its claim to exclusive expertise. Consensus helps avoid charges of political or service partisanship and reduces the likelihood of civilian "interference" in military affairs. In short, the bargaining advantage of policy makers or users is enhanced when there is competition, and diminished when there is consensus.

Samuel Huntington's study of strategic programs and national politics in the 1940s and 1950s also illuminates this dynamic. Huntington found that a service's willingness to compromise within the JCS depended on the consequences for itself were the Chiefs unable to achieve consensus. There was no point in dissenting if the service were left the worse for it. He found that reliability of administration support for a particular service was the best predictor of its behavior in the JCS. The greater the service's confidence its appeal to the administration would be supported, the less willingly it would make concessions to the other services. Conversely, unpredictable administration support increased the probability of JCS consensus.[43]

Kanter's later work modified Huntington's findings. He found that the most important variable for predicting split decisions was not consistent favorable treatment of a service by the administration, but persistent <u>unequal treatment</u>. But a service would dissent in the JCS only if it expected external support for doing so, namely from Congress.[44] Without such assurances, dissent was too risky. Though it was the least favorably treated service in the 1950s, for example, the Army rarely dissented because it did not expect better treatment in Congress.[45]

Under what conditions will organizations cooperate and when will they compete? Policy makers responsible for deciding on the merits of competing claims for resources are interested in this question. Competitive strategies are more likely to generate the information they need to make their decisions. Bureaucratic competition can help policy makers gain leverage over the organizations that are accountable to them. Conversely, cooperation reduces information and the leverage of policy makers, keeping them out of the decisionmaking process. The calculus is different from the organizational perspective. It is true that in the absence of countervailing pressures organizations will tend to compete for resources in their drive for self-promotion and ex-

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42. Ibid., pp. 27-28.

43. Ibid., p. 30.

44. Ibid., pp. 36-37.

45. Ibid., pp. 41-42

pansion.[46] But competition has drawbacks: it increases environmental contingencies, exposing organizations to risk. Organizations prefer stable, or at least predictable environments, so they will try to reduce uncertainty.[47] One way of reducing uncertainty is to cooperate with other organizations. Whether or not an organization will compete or cooperate will depend on which strategy offers the best prospects for getting what it wants. The two laboratory-system offer a good case for considering organizational strategies for dealing with uncertainty. When did they compete? What prompted cooperation? And what were the consequences for weapons development?

Thompson recommended making systematic comparisons of organizations in different task environments. This can help illuminate the impact of the environment on organizational behavior and organizational strategies.[48] Comparable organizations in similar task environments are expected to exhibit similar behaviors, while variations will occur if they operate in dissimilar environments. The Lawrence Livermore and Los Alamos laboratories had similar missions—to design and develop nuclear weapons—but they operated in different task environments. Their different geographical settings was only one factor. As we shall see, laboratory sponsors had different expectation of the roles and functions each would perform. How these different expectations influenced organizational strategies is explored and explained in the following chapters. What can we expect to find? What, precisely, was the impact of the environment on laboratory strategies?

Based on Kanter's work, we might expect the least favored organization—the one experiencing the most environmental uncertainty, for example—to "dissent" or compete. Although Kanter did not use these terms, the relationship he described for the JCS was one of competitive interdependence, defined above as the condition in which two or more organizations compete for the resources of the same sponsor or sponsors. Kanter found that the least favored service would dissent from a JCS opinion, but only if it could expect support in Congress. Generalizing these findings we posit that the least favored organization in a relationship of competitive interdependence will compete if it can expect support for doing so. What should we look for in the interactions of the two nuclear weapons laboratories? During periods in which the laboratories experience similar levels of uncertainty in their environment—be it high or low—we expect them to cooperate.[49] But if one of the two laboratories experiences greater uncertainty than the other it

46. Ibid., pp. 28-29.

47. Cyert and March, *A Behavioral Theory of the Firm*, pp. 118-120; Kanter, *Defense Politics*, p. 28; James G. March and Herbert A. Simon, *Organizations* (New York: Wiley Press, 1958), p. 159.

48. Thompson, *Organizations in Action*, p. 161.

49. This would be consistent with the findings of S. Levine and P. E. White who observed that competi-

will compete—or dissent, in Kanter's terms—but only if it can expect outside support. When one organization is persistently least favored by the sponsor we will call this asymmetric competitive interdependence. When sponsors agree on outcomes there is little room for the least favored organization to exploit differences among them, so the laboratories will cooperate. If sponsors disagree about desired outcomes, however, there is room for exploitation. In this case, the least favored organization is likely to take advantage of the opportunity by competing for sponsor support.

We shall learn more about the laboratories and their strategies for dealing with their environments in the chapters that follow. Where might we look for sources of uncertainty and what might be the laboratories' responses? Uncertainty might arise from the simple fact of the existence of two laboratories which could lead sponsors to ask, why two? Charges of "duplication of services" can arise when competing organizations are operating at less than capacity, as Levine and White observed.[50] One laboratory might seem sufficient, particularly if demand for nuclear weapons declines or if the political environment is less favorable to their development. The least favored organization is more likely to suffer this scrutiny and be charged with duplication. Differentiation of product or service is an important strategy for dealing with such problems, as Michael Porter discusses in his book on the competitive strategies of firms.[51] In general, risk will characterize the choices of organizational strategists in the least favored organization. Risk-taking strategies are those whose outcome can lead to either the highest payoff for the organization or the lowest. Cooperation—or consensus—can be risky because it might mean accepting a less than ideal situation. That is what the Army did in the 1950s when it did not dissent in the JCS even though this meant accepting its disadvantaged position relative to the other services. Competition is risky because it exposes the organization to the vagaries of the environment. For the least favored organization, however, these risks might be better than taking no action. The greater security enjoyed by the most favored organization will make it predisposed to more conservative strategies.

Risk-taking is just one among a number of possible entrepreneurial strategies the least favored organization might use. Other strategies might involve seeking out new military clients, which are likely to be similarly situated, that is, to have least favored status within their organi-

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tion is low between two organizations operating near capacity. See S. Levine and P. E. White, "Exchange as a Conceptual Framework for the Study of Interorganizational Relationship," *American Studies Quarterly* 5 (1961): 583-601 at 598, as cited in William M. Evan, "The Organization-Set," p. 182.

50. S. Levine and P. E. White, 1961, p. 598, as cited in Evan, "The Organization-Set," p. 182.

51. Michael E. Porter, *Competitive Strategies: Techniques for Analyzing Industries and Competitors* (New York: Free Press, 1980).

zational group. In other words, the least favored laboratory is likely to seek out military sponsors who have not yet or have only recently achieved acceptance for their nuclear role among the services or the Congress. Another entrepreneurial strategy could involve the least favored laboratory trying to impress potential sponsors with its ability to meet their needs.[52] This might include technical innovation, one of the "services" the laboratories offer their military clients.

Innovation as Strategy

James Q. Wilson has defined organizational innovation as a fundamental change in a significant number of organizational tasks.[53] Innovation can be a strategy for adapting to environmental change and can determine whether an organization survives or perishes. Historical and case study evidence has shown that some organizations have successfully adapted to environmental change through innovation, including the YMCA and the Red Cross. Others, such as the Women's Christian Temperance Union and the Townsend Movement, failed to innovate when faced with environmental change, and perished.[54]

Much has been written about innovation and its causes, yet there is as yet no general explanation for what makes organizations innovate. Does organizational slack or plenty promote it? Do mavericks promote innovation, or is it the work of established organizational leaders?[55] In the private sector, both monopolistic and competitive firms have been shown to innovate. Bureaucracies are thought to resist it, except perhaps under duress. Students of military organizations have proposed competing explanations for how and why military organizations innovate. Edmund Beard linked innovation, or the lack thereof, to bureaucratic interests: Air Force pilots suppressed development of intercontinental ballistic missiles, which would have competed with airplanes.[56] In this case bureaucratic interests inhibited innovation. But they can also promote

52. Thompson, *Organizations in Action*, p. 90.

53. James Q. Wilson, "Innovation in Organization: Notes Toward a Theory," in Thompson, *Approaches to Organizational Design*, ed. Thompson, pp. 193-218, at 196. See also James Q. Wilson, *Bureaucracy: What Government Agencies Do and Why They Do It* (New York: Basic Books, 1989), chap. 12, for an excellent discussion of innovation in government organizations.

54. Joseph R. Gusfield, "Social Structure and Moral Reform: A Study of the Woman's Christian Temperance Union," *American Journal of Sociology* 61 (1955): 221-232; David L. Sills, *The Volunteers* (Glencoe, Ill.: Free Press, 1957); Wilson, "Innovation in Organization," pp. 195-196; Mayer Zald and Patricia Denton, "From Evangelism to General Service: The Transformation of the YMCA," *Administrative Science Quarterly* 8 (1963): 214-234.

55. Harvey M. Sapolsky, "Notes on Military Innovation: The Importance of Organizational Structure," unpublished paper, 24 Feb. 1994.

56. Beard, *Developing the ICBM.*

it. The Navy's desire to avoid competing with the Air Force for the counterforce mission resulted in a doctrinal innovation: assured destruction. Only when secure in its strategic role did the Navy take on the Air Force, as Donald MacKenzie has shown.[57]

Organizational leaders are the source of innovation in Stephen Rosen's model. While MacKenzie and Beard explain doctrinal innovation as the outcome of bureaucratic processes and competition, Rosen treats organizations as independent of their environments.[58] He fails to account for how and why innovative leaders came to be in positions of authority. Were they selected for leadership positions because their views were in accord with powerful external actors who supported innovation? If so, it weakens Rosen's argument that organizations generate innovation from within.

Matthew Evangelista models technical innovation as a five-stage process, which begins with invention, and is followed by the search by scientists for advocates of new weapons development in the broader scientific community, the military, and ultimately in Congress.[59] Evangelista's model is similar to Rosen's in that innovation is generated from within organizations. This fails to capture the possibility that innovation might be an organizational response to the environment. As John M. Staudenmaier and others have argued, technical innovation may involve invention, development, and dissemination, but these do not necessarily follow one another in sequence.[60] Evangelista's model implies the process is linear, and thus discounts environmental influences.[61]

Can organizational innovation be explained without reference to the environment and other organizations? Annalee Saxenian's recent study linking patterns of innovation successes and failures to the structure of inter-organizational relationships suggests not.[62] Barry Posen and James Q. Wilson do link the organization's relationship to its environment to its propensity to innovate. Wilson argues that organizations are unlikely to innovate unless faced with a crisis, an "extreme change in conditions for which there is no adequate, programmed response."[63] Similarly, Posen

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57. MacKenzie, *Inventing Accuracy*.

58. Stephen Peter Rosen, *Winning the Next War: Innovation and the Modern Military* (Ithaca: Cornell Univ. Press, 1991).

59. Evangelista, *Innovation and the Arms Race*, p. 25.

60. On the meaning of technical innovation, see John M. Staudenmaier, *Technology's Storytellers: Reweaving the Human Fabric* (Cambridge: MIT Press, 1985), pp. 50-55.

61. Evangelista, *Innovation and the Arms Race*, p. 25.

62. Annalee Saxenian, *Regional Advantage: Culture and Competition in Silicon Valley and Route 128* (Cambridge: Harvard Univ. Press, 1994).

63. Wilson, "Innovation in Organization," p. 208.

argues that the organizational search for predictability, stability, and the control of uncertainty impedes innovation.[64]

Can organizations be prompted to innovate short of organizational crisis? Sanford L. Weiner a means for it: constrained autonomy. Such a condition will occur when an organization has short term autonomy of means balanced by a longer term demand that it be accountable to higher authority. Accountability creates uncertainty because it promises consequences for failure. At the same time, short term independence creates freedom to take risks required for innovation.[65] Deborah Avant similarly argues that organizations can be induced to innovate short of a crisis situation. The mechanism is embedded in the electoral system, which rewards organizations for performing in accord with the desires of electoral system representatives. Dependency renders the organization potentially responsive to its environment, auspicious for innovation.[66]

The least favored organization in a relationship of asymmetric competitive interdependence might thus be expected to innovate short of a crisis situation. That is because it operates in a constant state of low level crisis even though its existence may not be seriously threatened. In Wilson's terms, innovation becomes the programmed response to the crisis of being the least favored organization.[67] Implicit in this model is the assumption that organizations can respond to their environment. It follows that policy makers might be able to create incentives for innovation by structuring organizational relationships appropriately, perhaps by institutionalizing crisis. They might also thereby elicit information and policy options from the organizations they oversee. We return to how this might be accomplished in the final chapter. We now consider the history and dynamics of the two-laboratory system for the design, test, and development of nuclear weapons.

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64. Barry R. Posen, *The Sources Of Military Doctrine: France, Britain, and Germany Between the World Wars* (Ithaca: Cornell Univ. Press, 1984).

65. As discussed in Sapolsky, "Notes on Military Innovation," p. 4.

66. Deborah D. Avant, *Political Institutions and Military Change: Lessons from Peripheral Wars* (Ithaca: Cornell Univ. Press, 1995).

67. Wilson, "Innovation in Organization," pp. 208-209.

2. RELUCTANT RIVALS

The Air Force, the AEC, and the Origins of the Second Lab 1950-1952

The story of Livermore's origins contains some surprises. The Atomic Energy Commission, the government entity in charge of nuclear weapons design, development, and production, was opposed to the founding of a second laboratory. Not surprisingly, the prospect of a rival for resources also found Los Alamos squarely in the opposing camp. The speed with which Los Alamos was making progress in the development of the H-bomb was central to the debate. The Department of Defense was neutral at first, satisfied with Los Alamos' progress.. The Air Force, however, promoted the proposal aggressively. The remarks of a veteran of interservice rivalries provide a clue for why: Russia may be the target, but the Army is the enemy. The congressional Joint Committee on Atomic Energy charged with overseeing the nuclear weapons enterprise also strongly favored the founding of a second laboratory. Its overwhelming goal was expansion of the nuclear weapons complex and integration of nuclear weapons into U.S. military strategy. This, despite the expectation that civilian control of nuclear weapons might help balance military appetites. Though known by many as "Teller's laboratory" after Hungarian-born physicist and Manhattan Project scientist Edward Teller, he did not initially favor Livermore as the site for the second laboratory. How Livermore emerged from this competing mix of interests and organizational actors is the subject of this chapter.

The AEC and the Second Lab

Edward Teller had differed on a number of occasions with the Los Alamos administration regarding the organization and resources devoted to long-range nuclear weapons research.[1] Theoretical Division director Hans Bethe asked Teller to lead implosion research during the Manhattan Projects. Understanding how to implode high explosives is critical to the development of atomic bombs. Teller was unenthusiastic reportedly because he did not feel the task well matched to his interests or abilities.[2] In June 1944 Teller left the Theoretical Division to form an independent group reporting directly to Los Alamos director J. Robert Oppenheimer. This arrangement lasted until September 1944 when F Division was formed under Enrico Fermi with Edward Teller directing F-1, the subgroup devoted to general theory and the 'Super' thermonuclear weapon.[3] He thus spent much of the war investigating prospects for thermonuclear weapons while most of his Los Alamos colleagues worked to develop the first atomic weapons. Despite the laboratory's growing postwar staff, Teller felt dissatisfied with Los Alamos' commitment to nuclear weapons research and development.[4] He left the laboratory, heading for the University of Chicago in early 1946.[5]

The first Soviet atomic test in late summer 1949 intensified U.S. interest in thermonuclear weapons. Truman ordered the Atomic Energy Commission in January 1950 to develop the hydrogen bomb.[6] North Korea's invasion of South Korea in June 1950 was followed by a series of decisions by the Truman administration to increase nuclear weapons production and U.S. defense spending.[7] Teller had by then returned to Los Alamos, still pressing for an accelerated

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1. For an authorized biography of Teller see, Stanley A. Blumberg and Louis G. Panos, *Edward Teller: Giant of the Golden Age of Physics* (New York: Charles Scribner's Sons, 1990).

2. Telephone discussion with Robert Budwine, 19 Sept. 1995.

3. See David Hawkins, "Towards Trinity," in *Project Y: The Los Alamos Story*, vol. 2 of "History of Modern Physics, 1800-1950" (Los Angeles and San Francisco: Tomash, 1983), p. 76 and 184.

4. For Teller's differences with the Los Alamos administration on the nuclear program, see Richard G. Hewlett and Francis Duncan, *Atomic Shield, 1947/1952*, vol. 2 of "A History of the United States Atomic Energy Commission" (Berkeley: Univ. of California Press, 1990), chap. 12, 13, and 16.

5. Teller's official date of departure from Los Alamos was 21 Jan. 1946. See Hawkins, "Towards Trinity," p. 305.

6. For an insightful analysis of the role of scientists in Truman's H-bomb decision, see Barton J. Bernstein, "Four Physicists and the Bomb: The Early Years, 1945-1950," *HSPS* 18:2 (1988): 231-263; Bernstein and Peter Galison, "In Any Light: Scientists and the Decision to Build the Superbomb, 1942-1954," *HSPS*, 19:2 (1989): 267-347.

7. Gordon E. Dean, *Forging the Atomic Shield: Excerpts from the Diary of Gordon E. Dean*, ed. Roger Anders (Chapel Hill: Univ. of North Carolina Press, 1987), pp. xxiv-xxv.

thermonuclear program. Until the scientific principals of thermonuclear weapon design were better understood, however, infusing the program with substantial resources was unlikely to speed progress. Single-minded devotion to thermonuclear weapons research could mean sacrifices in the fission weapon program. Most of the obstacles were theoretical.[8] Norris E. Bradbury, who followed J. Robert Oppenheimer as director of Los Alamos, believed the scientific uncertainties substantial.[9] Others agreed, including physicist Hans Bethe.[10]

Soon after Truman's announcement on the H-bomb Bradbury asked Teller to head a newly formed committee to investigate advanced nuclear design concepts, including thermonuclear. Committees were a common way for Los Alamos to coordinate the work of the weapon divisions, bringing leaders and staff together on a periodic basis. The Family Committee, as it was called, held its first meeting in March 1950.[11] An important theoretical hurdle was overcome in March 1951 when Teller and the mathematician Stanislaw Ulam discovered an approach to thermonuclear design which proved the breakthrough for which H-bomb proponents had hoped.[12]

The Teller-Ulam invention, while solving a major design challenge, also opened up new areas of investigation and created new demands for resources and staff. Teller wanted a new division devoted exclusively to thermonuclear research. Bradbury opposed the idea, arguing that a separate division would disrupt the laboratory's overall program. Instead, he proposed the creation of a special coordinating committee.[13] An important factor in Bradbury's thinking was the laboratory's Theoretical or 'T' Division. Home of the laboratory's theoretical physicists, its principal task was weapons design calculations.[14] A separate thermonuclear division would have divided the personnel and resources of T Division to the detriment of the overall weapon program.

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8. J. Carson Mark, "A Short Account of Los Alamos Theoretical Work on Thermonuclear Weapons, 1946-1950," informal report LA-5647-MS (July 1974). The foreword of this document indicates that a classified version of the report was first issued on 1 Oct. 1954.

9. Hewlett and Duncan, *Atomic Shield*, p. 414.

10. Hans A. Bethe, "Memorandum on the History of the Thermonuclear Program," 28 May 1952; Edward Teller to Garrison Norton, 15 August 1952, "Comments on Bethe's History of the Thermonuclear Program" attached; Bethe, "Comments on the History of the H-Bomb," *Los Alamos Science* 3 (Fall 1982): 42-53. The latter provides a declassified and slightly edited version of a 1954 Bethe article on the H-bomb history.

11. So called because the weapon design concepts considered were named daddy, sonny, uncle, mother, and elmer. See Hewlett and Duncan, *Atomic Shield*, p. 536.

12. Dean, *Forging the Atomic Shield*, p. xxvi. The Teller-Ulam concept relied on radiation from a fission weapon to compress and ignite a fusion reaction in a secondary. Radiation implosion still forms the basis on which thermonuclear weapons are designed today.

13. Hewlett and Duncan, *Atomic Shield*, pp. 539-541.

14. Mark, "A Short Account," p. 3.

It would also divert resources and staff from the laboratory's immediate and pressing task of stockpiling new fission weapons, or so Bradbury and Los Alamos division leaders feared.

Bradbury instead tried to accommodate Teller's interests within the established laboratory organizational framework. He was anxious to do so given that Teller was one of the few top theoretical physicists willing to devote substantial and sustained effort to the program after the war. Most, including Hans Bethe, had returned to their academic posts. Bradbury also recognized Teller's growing political connections and felt the ramifications when the scientist brought his concerns to Washington.[15] Bradbury proposed that a group of about twenty-five people, working under Teller, be devoted exclusively to thermonuclear weapons research. Such a move would leave the Theoretical Division intact and would not disrupt the basic divisional organization of the laboratory. Besides, it was as far as Los Alamos division leaders were willing to go.[16]

Not satisfied, Teller met with AEC Chairman Gordon E. Dean in April 1951. Among the topics discussed was the advisability of establishing a second nuclear weapon design laboratory. Dean may have first heard of the proposal from Willard F. Libby who had the AEC chairman to propose creation of a second laboratory.[17] Libby was a member of the General Advisory Committee, the AEC's scientific advisory panel, and a colleague of Teller's at the University of Chicago. Whether they had coordinated their efforts is unknown, though likely. At Dean's request, Teller put his thoughts on the subject in writing. He proposed establishing the new laboratory in Boulder, Colorado, at the site of a National Bureau of Standards facility. Teller called for 50 senior scientists, 82 junior scientists, and 228 assistants which he thought could be in place and operating by summer 1952. The Boulder site housed cryogenic equipment that could handle liquid deuterium, required for work on the then-leading design concept for thermonuclear weapons.[18] Meanwhile, Commissioner Thomas E. Murray requested that his own June 1951 second laboratory proposal be placed on the AEC's meeting agenda. The commissioners considered the proposal over the summer, agreeing with Dean that it merited further study but that any decisions were premature.[19] Summer 1951 was active on a number of other related fronts. Participants at a mid-June conference in Princeton felt confident H-bomb design challenges could be

15. Hewlett and Duncan, *Atomic Shield*, pp. 554-556.

16. Ibid., pp. 539-541.

17. Dean, *Forging the Atomic Shield*, p. 132.

18. Edward Teller to Gordon E. Dean, 20 April 1951.

19. AEC Meeting No. 582, 26 July 1951, pp. 317-318. According to the meeting minutes, Murray's memorandum on the establishment of a new laboratory facility for thermonuclear weapon development was transmitted to the commissioners on 21 June 1951 and discussed during an AEC executive session.

resolved. The JCAE and the military pressed for additional production of plutonium and uranium 235, critical ingredients for nuclear weapon production.[20]

The AEC planned to discuss the second laboratory at its September 1951 meeting and Dean again asked Teller for his ideas. This time, Teller suggested Rocky Flats, Colorado, where a Dow Chemical Company facility would be located. The advantage of the "Rocky Mountain Laboratory" was its proximity to Dow facilities and scientists. Teller argued for starting on a small scale. And in what would become a recurring theme, argued that in order to insure "competition" the new effort should not be subordinate to Los Alamos.[21] Meanwhile, Los Alamos was deep into its thermonuclear program.[22] Bradbury confidently reported to the commissioners in mid-September that theoretical issues surrounding the first thermonuclear test had been resolved. Only "engineering, development, and logistic problems" remained, and Bradbury proposed a full-scale thermonuclear test in fall 1952.[23] This did not satisfy Teller, who believed that the Los Alamos approach, if it worked, would not yield data on the precise reasons for its success. If the test failed, Teller felt there would be insufficient data to explain why. He wanted a more systematic approach, testing individual components, and sooner than Los Alamos planned, preferably in summer 1952.[24] The commissioners were nevertheless impressed by Los Alamos' progress. They decided against founding a second laboratory, but also requested a staff study of the organizational issues involved.[25] Teller resigned the same day, although he changed his mind at least once before his final decision to quit Los Alamos at the end of the month.[26]

The Role of Congress

Who would control atomic weapon development, the military or a civilian agency, had been hotly debated following the end of World War II. Brien McMahon, the Democratic Senator from

20. Hewlett and Duncan, *Atomic Shield*, pp. 546-547.

21. Edward Teller to Gordon E. Dean, 11 Sept. 1951.

22. For an account in considerable detail of both the U.S. and Soviet thermonuclear weapon development programs, see Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (New York: Simon & Schuster, 1995).

23. "Atomic Energy Commission-Military Liaison Committee Notes on an Informal Conference," 9 Sept. 1951.

24. Hewlett and Duncan, *Atomic Shield*, p. 556; John S. Walker to William L. Borden, "Thermo Nuclear Program," 7 April 1952.

25. AEC Meeting No. 603, 11 Sept. 1951, p. 412.

26. Dean, *Forging the Atomic Shield*, pp. 161-164

Connecticut and chairman of the congressional Joint Committee on Atomic Energy, was an ardent advocate of atomic weapons development.[27] He had played a key role in winning passage of the Atomic Energy Act of 1946, which gave the civilian Atomic Energy Commission sole jurisdiction over nuclear research.[28] The act also established the Joint Committee on Atomic Energy and the Military Liaison Committee (see below). McMahon became the first chairman of the JCAE in 1946, a position from which he sought to promote the development of "thousands and thousands" of nuclear weapons.[29] He also advocated the creation of "an atomic army, an atomic navy, and an atomic air force."[30] He supported development of the H-bomb as well as tactical nuclear weapons, subjects which divided some advocates of nuclear weapons as discussed below. The political and bureaucratic structure of nuclear weapon development did as much to create an advocate in McMahon as did his own interest and personal ambitions, which included a run for the presidency. The importance of the JCAE grew with the importance of nuclear weapons. Its sole and exclusive authority over the atomic weapons complex centralized advocacy in the committee. Had the atomic weapons enterprise been under military control, nuclear weapons would have competed with other Department of Defense (DOD) programs for funding. Instead, they were essentially a free resource. DOD was responsible only for the development of delivery systems, which in any case tended to cost more than the nuclear warhead.

McMahon soon learned about the second laboratory proposal, probably through Commissioner Murray, the lone advocate on the AEC. He took it as another opportunity to argue for accelerated H-bomb development and also urged Teller meet with Murray to discuss their ideas.[31] Gordon Dean defended the AEC against McMahon's charges it was moving slowly on the H-bomb. He told the chairman in August 1951 the AEC was studying the second laboratory proposal.[32] Soon after, McMahon found two more reasons to press his views. He learned in Oc-

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27. For biographical information about McMahon, see *Biographical Directory of the United States Congress, 1774-1989* (Washington: GPO, 1989), p. 1479.

28. Robert Byrnes, "McMahon Aspired to Lead U.S. in Foreign Relations," newspaper obituary, 26 July 1952, from the files of the United States Senate Historical Office. See also Allen Johnson et al., *Dictionary of American Biography*, 29 vols. and 8 supplements (New York: Charles Scribner's Sons, 1928-1990), Supplement 5 (1977), p. 455.

29. Hewlett and Duncan, *Atomic Shield*, p. 548.

30. JCAE, "Policy and Progress in the H-bomb Program: A Chronology of Leading Events," 1 Jan. 1953, entry for 18 Sept. 1951. This chronology is informally referred to as the "Borden-Walker chronology," after JCAE executive director William L. Borden and staff member John S. Walker.

31. Borden to McMahon, 2 Oct.

32. JCAE, "Policy and Progress in the H-bomb Program," entry for 21 August 1951; Hewlett and Duncan, *Atomic Shield*, p. 558.

tober that the Soviets had conducted their second nuclear test.[33] He also learned of military interest in a second nuclear weapons laboratory.[34]

Committee members were skeptical of Dean's and Bradbury's claims about the dangers a new laboratory would pose to Los Alamos. They did not want to damage AEC or Los Alamos credibility by criticizing them too harshly, so did not force the issue. Gentle prodding was as far as committee members would go, leaving the AEC and Los Alamos little incentive to act. McMahon thus constrained himself to making suggestions, including the proposal that a new laboratory focus on fission weapons.[35] This might have offered a way around the AEC's concern about a second laboratory's interference with the Los Alamos thermonuclear program.

McMahon reserved his harshest criticism for the military, charging military leaders with "a complete lack of imagination . . . in seeing the military possibilities of atomic weapons." He singled out the Military Liaison Committee (MLC), the joint DOD/AEC organization in which DOD atomic weapons expertise largely resided and which served to convey military interests to the AEC. The MLC, McMahon asserted, was doing little to stimulate atomic weapons development. He urged more rapid integration of nuclear weapons into military thinking and criticized the three civilian secretaries for their "appalling lack of knowledge in the atomic energy activities of their own service."[36]

MLC Chairman Robert LeBaron told AEC commissioners in November 1951 of military support for a second laboratory. Although the matter was not strictly speaking one under MLC jurisdiction, LeBaron did not believed "competition and new ideas in weapons development" would occur without new weapons research and development laboratories.[37] The JCAE may have been the intended audience for such shows of support. Military interest may also have resulted from interservice rivalries. Excluded from a major role in nuclear strategy and by the high priority placed thermonuclear weapons, Army and Navy aspirations may have been frustrated. In the words of an Air Force history, the Army and Navy "feared that the thermonuclear program

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33. Hewlett and Duncan, *Atomic Shield*, p. 558. Truman announced the second Soviet nuclear detonation on 3 Oct. 1951.

34. William L. Borden to Brien McMahon, untitled memo, 2 Oct. 1951. Borden was not specific about the locus of military interest.

35. Brien McMahon to Gordon E. Dean, 5 Oct. 1951; JCAE report to Congress cited in JCAE, "Policy and Progress in the H-bomb Program," entry for 19 Oct. 1951, urges the consideration of a second weapons development laboratory "to supplement Los Alamos"; see Dean, *Forging the Atomic Shield*, p. 177, for Dean's discussion with Borden.

36. Robert LeBaron, "Summary of Remarks by Mr. Blair of Time Magazine 19 Oct. 51," 31 Oct. 1951.

37. Minutes of 62nd AEC-MLC Conference, 20 Nov. 1951, p. 41.

would be pursued to the detriment of the fission program and thereby limit the number of bombs stockpiled for tactical purposes."[38] A second laboratory would expand the capacity of the nuclear weapon development complex, better meeting Army and Navy interests, as Livermore eventually did.

Most members of the General Advisory Committee shared AEC concerns regarding the establishment of a second laboratory. Comprised largely of veterans of the wartime Manhattan Project and chaired by former Los Alamos director Robert Oppenheimer, they understood what was involved in managing the nuclear weapons program. But like many advisory committees, the GAC's role was not only to give advice. Its endorsement also helped confer legitimacy to AEC decisions. Skeptical of the benefits of a crash program to develop the H-bomb, the GAC had argued in 1949 against a "high priority . . . all-out" effort. It thus found itself on the losing side of Truman's 1950 decision.[39] Its credibility was damaged as a result, but the GAC continued to play a role in advising the AEC.

GAC members were impressed with Los Alamos' progress on the H-bomb discussed at the October 1951 meeting with laboratory leaders. The first thermonuclear test was scheduled in the Pacific for fall 1952.[40] The GAC sympathized with Bradbury's concern that a new laboratory would dilute resources and draw on a limited pool of qualified scientific staff.[41] Echoing Bradbury's view that establishing a "new Los Alamos" was a bad idea, the GAC was

> convinced that the establishment of another weapons laboratory was neither necessary, nor in any real sense feasible. It appears to us unnecessary because of the rapid progress that Los Alamos has made and is likely to make in the future . . . [and] the establishment of a new 'Los Alamos' or a 'thermonuclear laboratory' would tend to impair the effectiveness of Los Alamos.[42]

Instead, GAC Chairman Oppenheimer urged adoption of Bradbury's suggestion to transfer routine weapon development activities to another site, or sites, relieving Los Alamos of production responsibilities. This would leave Los Alamos free to focus on the thermonuclear research

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38. Lee Bowen, *The Development of Weapons*, vol. 4 of "A History of the Air Force Atomic Energy Program, 1943-1953 in Five Volumes" (USAF Historical Division, n.d., c. 1959), p. 441.

39. "The GAC Report of October 30, 1949," appx. I in Herbert F. York, *The Advisors, Oppenheimer, Teller, and the Superbomb* (Stanford: Stanford Univ. Press, 1976), p. 153.

40. Minutes of 27th GAC Meeting, 11-13 Oct. 1951, p. 11.

41. Norris K. Bradbury to Kenneth E. Fields, 9 Oct. 1951, as discussed in Minutes of 27th GAC Meeting, 11-13 Oct. 1951, Washington, D.C., pp. 10-11.

42. J. Robert Oppenheimer to Gordon Dean, 13 Oct. 1951, p. 2, attached to Minutes of 27th GAC Meeting, 11-13 Oct. 1951.

and test program.[43] The lone dissenter was Willard Libby, who had first proposed a new laboratory in spring 1951. Libby felt the H-bomb development program should be "attack[ed] with more vigor and energy." Weighted down by its responsibilities in the fission weapon program, the laboratory needed the "spirit of competition" injected into its outlook.[44] Views on the value of competition divided the GAC. In any case, the proposal itself, as Dean remarked to JCAE executive director William L. Borden, would "hurt out there [at Los Alamos]" who resented the implication they were not working as hard or as smart as they might.[45] The GAC voted against the second laboratory in October 1951, lending support to Dean's position.

Disappointed, Teller asked to present his views on the subject at the GAC's mid-December meeting where he argued that thermonuclear research had been "unnecessarily slow."[46] For his part, Bradbury resented the "thinly veiled criticism" that Los Alamos was not doing its job.[47] Bradbury had gone so far as to offer the critics organizational change, but the GAC believed they were too little, too late. They would not suffice to relieve "pressures for a more vigorous prosecution of thermonuclear research."[48] By winter 1951, JCAE and military voices in support for a new laboratory had multiplied. Still opposed, the GAC recommended Bradbury establish a separate division at Los Alamos charged explicitly with problems of "greater variety and longer range," including thermonuclear research. This was just the proposal Teller had promoted earlier that year.[49]

McMahon met with defense representatives in February 1952 to persuade Secretary of Defense Robert A. Lovett to recommend a larger nuclear expansion program than that being considered by the Truman administration.[50] Instead, committee members concerned about tepid mili-

43. Ibid., p. 2.

44. Willard F. Libby to Gordon Dean, 13 Oct. 1951, attached to Minutes of 27th GAC Meeting, 11-13 Oct. 1951.

45. Dean, *Forging the Atomic Shield*, p. 177.

46. Minutes of 28th GAC Meeting, 12-14 Dec. 1951, Washington, D.C., p. 12. See pp. 10-14 for Teller's discussion with the committee.

47. Hewlett and Duncan, *Atomic Shield*, p. 569.

48. As cited in Barton J. Bernstein, "Teller, Lawrence, and a New Lab," prepared for the workshop on the "Decade of Innovation: Los Alamos, Livermore, and National Security Decisionmaking in the 1950s," Pleasanton, Cal., 19-21 Feb. 1992, p. 18, n. 39.

49. Minutes of 28th GAC Meeting, 12-14 Dec. 1951, pp. 27-28; Hewlett and Duncan, *Atomic Shield*, p. 570.

50. Hewlett and Duncan, *Atomic Shield*, p. 578.

tary support for nuclear weapons heard testimony that seemed to confirm their worst fears. Joint Chiefs of Staff Chief Omar N. Bradley, for example, refused to recommend speeding the H-bomb program. More fundamentally, he did not believe the H-bomb would "ever supplant our present stockpile of A-bombs, [although] . . . it would be an important supplement."[51]

Secretary of Defense Lovett requested MLC chairman LeBaron prepare an outline of DOD's historical spending on nuclear weapons. This appears to have been a defensive move against McMahon's criticisms. LeBaron's response emphasized that DOD has spent "large sums of money" in developing carrier vehicles and delivery methods for atomic weapons, sums many times larger than those the AEC spent. Furthermore, the MLC had required no prodding for its interest in atomic weapons. DOD had begun to study the atomic expansion program long prior to Senator McMahon's request to do so.[52] But McMahon continued to argue that it was military attitudes and policies, not Los Alamos, that posed the major stumbling blocks to an aggressive nuclear policy. DOD thinking and planning for long-lead items "promises to be more badly managed than the lead time in weapon development." Los Alamos thermonuclear tests scheduled to start later that year were expected to succeed. Although some time might be saved in the test program, the most urgent task was up to the military: making definite plans to exploit the new weapon.[53]

President Truman approved the second major expansion of AEC atomic production facilities in February 1952.[54] Truman had effectively excluded McMahon from the process, asking the AEC Chairman to withhold the expansion study until the administration own plan was ready. The JCS and Secretary of Defense likewise withheld plans from the Joint Committee, arguing these fell outside the JCAE's purview. The administration had thus "neatly shunted" the JCAE aside until its own proposal was ready.[55]

Preempted by the president and facing military support for what he considered inadequate expansion and complacency on H-bomb development, McMahon had two choices. He could accept the status quo and lose the initiative. Or he could become an even more vocal proponent of the H-bomb program, and its corollary, the second laboratory. For McMahon, the choice was obvious. In preparing for hearings with DOD in February 1952, the JCAE staff warned commit-

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51. JCAE, "Policy and Progress in the H-bomb Program," entry for 6 Feb. 1952.

52. Robert LeBaron to Robert A. Lovett, untitled memorandum, 4 Feb. 1952.

53. John S. Walker, untitled memorandum to the files, 5 March 1952.

54. Hewlett and Duncan, *Atomic Shield*, pp. 528-29. Truman approved the first expansion in Oct. 1950.

55. Ibid, pp. 576-578 (quote on p. 578).

tee members the Soviets might achieve an H-bomb capability before the United States. In their words, the Los Alamos hydrogen program was "too little . . . too late." They dismissed the creation of a special division at Los Alamos as "inadequate" and urged the establishment of a new laboratory as "the greatest single step . . . to hasten the H-bomb effort."[56]

McMahon found no support from the AEC for a more aggressive expansion program.[57] Dean remained adamantly opposed. A new laboratory "would [not] help this [H-bomb] program one bit." Furthermore, Dean was concerned about the organizational disruption that could result. A laboratory was "not built with bricks alone. [It] . . . comes about by illusionary process and not by edict." Los Alamos, as he told the JCAE, was a "sensitive place."[58] Dean favored a more gradual approach, strengthening the atomic weapons program by taking advantage of programs already in place. Rocky Flats had growth potential. The small laboratory of metallurgists at the Rocky Flats laboratory working on plutonium and uranium fabrication techniques might gradually grow into a larger effort.[59] Rocky Flats itself was being constructed to relieve Los Alamos of production, modification, and inspection work and would be available within the year. High explosives would be produced at Burlington, Amarillo and a third high explosive facility was in the planning stages. These and other facilities were scheduled to take over routine tasks from Los Alamos, freeing the laboratory to concentrate on leading edge work.[60]

Senator Bourke B. Hickenlooper (R-Iowa) took issue with Dean. He objected to the Commission having "all [its] . . . eggs in one basket." Allowing a monopoly in nuclear weapons design was dangerous. Under such conditions,

> where you have one outfit like that, they can be leisurely or not as they choose. . . . The competitive spirit is not existent. . . . There is a tendency also . . . on the part of a great many people to keep as much command under their jurisdiction as they can . . . and if there is to be an expansion of the program, they want to run it. . . . It may be that there is a certain limited hierarchy of nuclear physicists where it is a kind of closed lodge.

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56. "Status of the Hydrogen Project," JCAE hearing, 21 Feb. 1952, staff memo attached.

57. Hewlett and Duncan, *Atomic Shield*, p. 578.

58. "Status of the Hydrogen Project," JCAE hearing. Attached to transcript is 9 Jan. 1952 letter from the AEC rejecting the proposal for a second laboratory.

59. "Status of the Hydrogen Project," JCAE hearing.

60. Gordon Dean to Brien McMahon, 9 Jan. 1952, attached to "Status of the Hydrogen Project," JCAE hearing. Commissioner Thomas Murray dissented from Dean's letter; see Murray to Dean, 10 Jan. 1952.

McMahon seconded Hickenlooper's comments, noting that "General Electric got the lead out of its tail when we got DuPont back in the business. They really made some progress . . . when they got some competition." Also testifying before the JCAE, AEC Commissioner Murray agreed the AEC "should make every effort . . . to break the monopoly" of Los Alamos and that "the possibilities of competition" ought to be explored.[61] Dean disagreed. Unlike private sector competition, laboratory competition would dilute and jeopardize the effort, not speed it.[62]

Military Force

DOD supported the founding of a second laboratory in principle, but there was little sense of urgency about it in early March 1952. Secretary of Defense Lovett agreed with McMahon that nuclear weapons research and development should be expanded, and that plans for a second laboratory could be prepared.[63] Lovett followed the advice of his senior military representatives on the MLC, however, in warning against transferring the thermonuclear program from Los Alamos to another site. As he described it, this would be "move in the wrong direction."[64]

DOD's position changed substantially by the end of March. A letter from the three service secretaries to Deputy Secretary of Defense William C. Foster was the catalyst. Voicing alarm about possible advances in the Soviet H-bomb program, they recommended acceleration of the H-bomb program. They wanted Foster to convene a session of the Special Committee on Atomic Energy of the National Security Council on which he served to consider the proposal. The secretaries also urged the Secretary of Defense to enlist NSC support in urging rapid development of a second thermonuclear weapons laboratory.[65]

Foster promptly forwarded the letter to Secretary of State Dean G. Acheson and AEC Chairman Dean, his two colleagues on the NSC Special Committee. In his cover letter he tried to reconcile this new sense of urgency with DOD's earlier complacency. He explained that while the

61. "Status of the Hydrogen Project," JCAE hearing.

62. Gordon Dean to Brien McMahon, 9 Jan. 1952, attached to "Status of the Hydrogen Project," JCAE hearing.

63. MLC, "Report on the Rate and Scale of Effort on Thermonuclear Weapon," 28 Feb. 1952, as cited in Robert LeBaron to William C. Foster, "Suggested Outline for Discussion of Thermonuclear Program Status at Joint Secretaries' Meeting 14 August 1952," 13 August 1952.

64. JCAE, "Chronology of Second Laboratory Events," 16 June 1952 Draft, entry for 9 March 1952. See also JCAE, "Policy and Progress in the H-bomb Program," entry for 9 March 1952. The three services representatives were Adm. Frederic S. Withington, USAF Maj. Gen. Howard G. Bunker, and Army Gen. Herbert B. Loper. See their 28 Feb. 1952 memo to Robert LeBaron as cited in Dean, *Forging the Atomic Shield*, p. 209, n. 7.

65. Bunker, Loper, and Withington to Secretary of Defense, 27 March.

thermonuclear program satisfied the president's 31 January 1950 directive, there was "a growing feeling in the Department of Defense" that the program should be intensified "now" given there were no guarantees of success. Failure to do so was potentially disastrous if the Russians developed an H-bomb before the United States.[66] For his part, MLC Chairman LeBaron warned the Secretary of Defense the H-bomb program could no longer simply be treated narrowly as an "internal [AEC] management" problem. Retreating from earlier statements of support for the Los Alamos program, LeBaron now asked why the United States had been so slow to move on H-bomb. He also turned the Los Alamos case against a second laboratory upon its maker: How could the new H-bomb technology be exploited if, as Los Alamos claimed, present facilities and personnel were stretched to the limit?[67]

What explains DOD's turnaround on the H-bomb and the second laboratory? In a sense, it had not really changed. Behind the unified and moderate facade presented by DOD officials were long-standing conflicting military views on the priority of the H-bomb program. During the early 1950s, proposals to develop atomic weapons for tactical use—and increasingly this came to mean "tactical" or "small" atomic weapons—vied with the H-bomb for priority.[68]

The Korean war led American war planners to reassess priorities. Until then, war plans had called for destroying Soviet industrial targets in strategic bombing campaigns. This strategy was supposed to buy time for American and allied conventional forces to prepare for a World War II style campaign in Europe. The loss of the U.S. monopoly on atomic weapons raised the possibility that Soviet aggression could no longer be deterred, as was surmised to be the case in Korea, with the Chinese serving as surrogates for the Soviets. Such arguments lent support to proponents of tactical atomic weapons. The Air Force had "reluctantly consented" to the production of tactical atomic weapons for Army and Navy use, expecting the JCS to limit their production. The services differed strongly in their opinion about the allocation of fissionable material among the various types of weapons. Conflicts arose even within the Air Force, with the Strategic Air Command opposing tactical air atomic weapon development.[69] In short, the question was: how should resources be allocated between H-bomb and tactical fission weapons?

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66. Secretary of Defense to Secretary of State and AEC Chairman, "Intensification of the Thermonuclear Weapons Program," 28 March 1952.

67. Robert LeBaron to Secretary of Defense, "Remarks on the H-Bomb Program," 31 March 1952.

68. Tactical uses of atomic weapons and tactical atomic weapons should be distinguished. Tactical uses can involve weapons in a range of yields, including high yields. Tactical weapons usually refers to relatively small, low-yield weapons.

69. Bowen, *The Development of Weapons*, pp. 471-472.

Los Alamos had begun development of the Navy's Mk 8 bomb for aircraft delivery in 1948 and the first atomic artillery shell for the Army in 1950.[70] Displeased as it was, the Air Force perceived no frontal assault on the primacy of its strategic bombing role. By summer 1950, the Air Force itself requested the development of the TX-5 to be carried by the B-45 against tactical targets, and the TX-12 for external carriage on fighter bombers. The Air Force, however, "did not abandon its position" that the principal mission of atomic weapons should be "to insure a national capability for strategic air offensives." Because it had accepted the development of tactical weapons did not mean the Air Force endorsed producing them in numbers which would "undermine strategic power."[71]

Interservice rivalries were intensified by Project Vista, which challenged the dominance of the Air Force's strategic bombing role.[72] Led by Lee A. DuBridge of the California Institute of Technology (CalTech), it addressed the problems of tactical warfare, especially those confronting NATO in the event of Soviet aggression. When the panel issued a preliminary draft of its report in the fall 1951, interservice conflicts over the development of tactical weapons intensified. As an Air Force history described it, the Army and Navy on the one hand, and the Air Force on the other had "strongly conflicting opinions" about small tactical weapons."[73]

According to *Fortune*, the draft Vista report presented at CalTech in fall 1951 produced "an explosion" in the Air Force, while *Aviation Week* declared it "another attempt to undermine the strength of the USAF."[74] Especially offensive was a key chapter on atomic warfare which implied a secondary role for strategic air offense suggesting tactical atomic weapons could be the "decisive factor" in the defense of Europe.[75] The scientists who drafted the report were persuaded by the Commander of U.S. Air Forces in Europe to modify the language to state only

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70. Los Alamos began development of the Mk-8 for the Navy in 1948 and the W-9 nuclear warhead for the Army's 280-mm gun in 1950. The Mk 8 was for delivery by the AJ, and AD-4B aircraft. See Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), pp. 106-107. For a good overview of Navy nuclear weapon development, see Vincent Davis, *Postwar Defense Policy and the U.S. Navy, 1943-1946* (Chapel Hill: Univ. of North Carolina Press, 1966), pp. 190-199 and 240-259.

71. Bowen, *The Development of Weapons*, p. 378.

72. Hewlett and Duncan, *Atomic Shield*, p. 580; and David C. Eliot, "Project Vista and Nuclear Weapons in Europe," *IS* 11 (Summer 1986): 163-183. Project Vista was conducted from April-Dec. 1951.

73. Bowen, *The Development of Weapons*, p. 430.

74. *Fortune*, May 1953, p. 110; *Aviation Week*, July 1952, p. 4, as quoted in Eliot, "Project Vista," p. 174, nn. 41 and 42.

75. "Vista Report," vol. 1, p. 93, as cited in Eliot, "Project Vista," p. 170, n. 22.

that tactical employment of atomic weapons held "outstanding promise."[76] The final report eliminated the implication that strategic air and tactical applications of atomic bombs were mutually exclusive. Bad feelings remained nevertheless, particularly regarding Robert Oppenheimer. Though not a formal member of the Vista study, Oppenheimer had been invited to participate, as had numerous others, by the leader of the subgroup analyzing the tactical role of atomic weapons.[77]

Oppenheimer's role in helping draft the chapter on atomic weapons only increased already hostile Air Force views about the scientist. Some recalled that he had recommended thermonuclear weapons be given lower development priority than atomic weapons in 1950.[78] Prior to that, Oppenheimer had in 1949 led the General Advisory Committee in recommending against an accelerated H-bomb program, earning him the suspicion of the Air Force. These events probably contributed to his removal from the Air Force's secret access list around fall 1951.[79] The timing of his removal suggests, however, it was the Vista study and his role in it that most concerned the Air Force. Further evidence is provided by the comments of Air Force Chief Scientist David Griggs, who asked JCAE staff in April 1952 what was being done to "get Oppenheimer off the GAC."[80] The strength of Air Force sentiments was further revealed in a July 1952 JCAE staff memo which observed that the Air Force's "total passion . . . seems to be oriented toward the GAC members and the Vista report."[81]

The Air Force's real battle was with the Army, but Oppenheimer's pronouncements and activities lent credibility to Army ambitions. Formerly led by Oppenheimer, Los Alamos suffered the antipathy of the Air Force, or at least its senior civilian leadership, by association. Air Force support for a second laboratory must therefore be viewed in this context. Also to be considered in this context was the continuing struggle between the military and the AEC over control of nuclear weapons development, custody, and policy. Each successive expansion of the AEC's pro-

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76. David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960," *IS* 7 (Spring 1983): 113-181, at 140.

77. Eliot, "Project Vista," pp. 175-176.

78. Hewlett and Duncan, *Atomic Shield*, p. 580.

79. John S. Walker to JCAE Files, "Dr. Oppenheimer and the Vista Report," 2 July 1952. Removal of Oppenheimer from the Air Force access list was taken at the request of Air Force Chief of Staff Hoyt S. Vandenberg.

80. Walker to Borden, "Thermo Nuclear Program."

81. Walker to JCAE Files, "Dr. Oppenheimer," 2 July 1952.

duction capabilities had raised the custody issue anew.[82] Expansions intensified conflicts, raising questions about resource allocation, the priority of atomic and thermonuclear weapons development, and the role the services in nuclear strategy.

In January 1952, Air Force Chief Scientist David Griggs, a strong proponent of the H-bomb, arranged for Teller to meet with Air Force Science Advisory Board Chairman James A. Doolittle to discuss the thermonuclear program and the second laboratory.[83] Doolittle subsequently discussed the issues with Air Force Secretary Thomas K. Finletter, and Griggs arranged, at Finletter's request, for Teller to brief the secretary.[84] Finletter later spent two days at Los Alamos in early March where Doolittle and Deputy Secretary of Defense Foster also spent a few days that month.[85] All three may have attended the conference attended also by MLC Chairman LeBaron.[86] Foster was apparently displeased by his cool reception at Los Alamos, getting the impression Bradbury resented discussing the thermonuclear program with him. Dean later observed that a different reception "might have had quite a different effect" on Foster's opinion of the H-bomb and the second laboratory.[87] The problem had deeper roots, however, in interservice rivalries.

Finletter's visit to Los Alamos set the stage for Teller and his RAND colleagues to brief the Secretary of Defense, the three service secretaries, and other DOD representatives on 19 March 1952.[88] Teller repeated his concerns about the Los Alamos thermonuclear program. He warned

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82. Hewlett and Duncan, *Atomic Shield*, pp. 580-581.

83. For an informative history of the board during this period, see Thomas A. Sturm, *The USAF Scientific Advisory Board: Its First Twenty Years, 1944-1964* (Washington, D.C.: OAFH, 1986). The role of Air Force Chief Scientist David T. Griggs in this period is recalled by Ivan Getting and Bernard Shriever in *Reflections on Research and Development in the United States Air Force, an Interview with General Bernard A. Schriever and Generals Samuel C. Phillips, Robert T. Marsh, and James H. Doolittle, and Dr. Ivan A. Getting, conducted by Dr. Richard H. Kohn*, ed. Jacob Neufeld (Washington, D.C.: CAFH, 1993), pp. 49-50.

84. Edward Teller, with Allen Brown, *The Legacy of Hiroshima* (Garden City, N.Y.: Doubleday, 1962), pp. 60-61.

85. On Finletter's visit, see Hewlett and Duncan, *Atomic Shield*, p. 582; and Robert LeBaron to Secretary of Defense, "Remarks on the H-Bomb Program," 31 March 1952. On Doolittle's visit, which was scheduled for 11-14 March 1952, see "Itinerary" in The Papers of James H. Doolittle, Container 12, Folder 83, 1951-58, Library of Congress. On Foster's visit see Dean, *Forging the Atomic Shield*, p. 208.

86. LeBaron's 31 March 1952 memo states that there was a two-day conference at Los Alamos sometime "recent[ly]." See Hewlett and Duncan, *Atomic Shield*, p. 582, n. 17, for the difficulties in piecing together the chronology of events regarding the second laboratory in the late 1951-early 1952 period.

87. Dean, *Forging the Atomic Shield*, p. 208.

88. AEC, *In the Matter of J. Robert Oppenheimer* (Washington, D.C., 12 April-6 May 1954), reprinted in AEC, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board and*

that the Soviet thermonuclear program—based in part on information obtained from Klaus Fuchs, the confessed spy—was likely to be quite advanced. The briefing proved a turning point in military thinking on the H-bomb program and the second laboratory. The Air Force soon persuaded the Army and the Navy to join in the letter to Foster asking for a meeting of the NSC Special Committee on Atomic Weapons to urge acceleration of the H-bomb program and establishment of a second laboratory.[89]

Los Alamos was responsible for the development of nuclear weapons for all three services. Tensions and conflicts over the appropriate allocation of resources for fission and thermonuclear weapons development within the laboratory mirrored those occurring among the services. Los Alamos might have avoided Air Force criticism had laboratory leaders been more demonstrably responsive to Air Force officials. But Los Alamos' monopoly on nuclear weapons design and development provided little incentive for the laboratory to be overly solicitous. Los Alamos delayed making organizational or programmatic changes suggested by the AEC that might have defused military concerns. Air Force and DOD inquiries about the H-bomb program were viewed as intrusive by laboratory leaders, fueling Air Force interest in a second laboratory.

Powers of Persuasion

The letter from the three service secretaries prompted Deputy Secretary of Defense Foster to request a meeting with his colleagues on the NSC Special Committee on Atomic Energy.[90] Foster, Gordon Dean, and Secretary of State Dean Acheson discussed the H-bomb program and the second laboratory proposal on 1 April. But first they were briefed by Teller and his RAND colleagues. Dean's primary objective was to avoid getting the president involved, which a full discussion in the NSC would ultimately entail. Military expressions of concern about the H-bomb program could damage the credibility of the AEC and Dean did not want to take that risk.

Texts of Principal Documents and Letters (Cambridge: MIT Press, 1970), p. 758. The Teller-Rand briefing was also given to the JCS and the MLC on 31 March 1952. See JCAE, "Chronology of Second Laboratory Events," entry for 15 April 1952.

89. For Teller briefing, see William L. Borden and John S. Walker to Brien McMahon, untitled memo on thermonuclear program, 4 April 1952; Dean, *Forging the Atomic Shield*, pp. 206-211. Some argued that Fuchs could not have helped the Soviet H-bomb program because the key breakthroughs in the U.S. program were not made until after he was arrested. The Russians have acknowledged that information gained from intelligence gathering was critical to the Soviet atomic weapons program, but they also deny that significant information was gained from the U.S. program for their H-bomb design. See, e.g., Dean, *Forging the Atomic Shield*, pp. 204-205; Bethe, "Memorandum on the History of the Thermonuclear Program," pp. 14-15. See Hewlett and Duncan, *Atomic Shield*, p. 583 on service secretaries request to Secretary of Defense for NSC meeting.

90. Dean, *Forging the Atomic Shield*, p. 206; Howard G. Bunker, Herbert B. Loper, and Frederic S. Withington to Secretary of Defense, 27 March 1952.

He asked his colleagues not to make any decisions before they had heard "the other side of the question" from Bradbury and Los Alamos. Arguing the issue did not belong in the NSC, he reminded his colleagues that DOD and the AEC, not the NSC, "were [under presidential directive] to jointly determine the rate and scale of the thermonuclear effort." Dean also argued "it would be unfair to ask the President to make a decision on a question which involved numerous technical considerations and numerous personality items."[91]

According to Foster, the Department of Defense harbored no ambitions to sponsor its own separate laboratory. He had "never visualized the building of a completely separate lab out from under the jurisdiction of Los Alamos."[92] It was clear that Foster had done some investigating of his own in search of a solution. One option was to build on already established groups like the University of California Radiation Laboratory's. Dean responded that several groups were already contributing to the weapons program outside of Los Alamos, including American Car and Foundry, the Wheeler team at Princeton, the National Bureau of Standards at Boulder, and the Cambridge Corporation.[93] Agreeing that more could be done, Dean undertook to explore the possibilities at UCRL and confer with the members of the Military Liaison Committee about their preferences.[94]

Dean thereby persuaded his colleagues that the second laboratory and H-bomb matters should not be referred to the full National Security Committee. So on 3 April the NSC referred the issue to the Department of Defense for resolution.[95] Dean had now committed the AEC to exploring ways to expand the thermonuclear research program. Much remained to be determined, including the scope and scale of the effort.

Despite the NSC Special Committee's agreement that the H-bomb program was "an AEC-DOD problem," and not one that belonged on the agenda of the NSC, pressure on the AEC continued. Dean could still not be sure the matter would not reach the president. In fact, the NSC's Executive Secretary, James S. Lay, soon called to ask if the president should be given the Teller-Rand briefing. Dean argued that questions relating to the thermonuclear program, and by as-

91. Dean, *Forging the Atomic Shield*, p. 208.

92. Ibid, p. 208.

93. According to Dean, *Forging the Atomic Shield*, n. 6 on p. 209, AC&F built parts, while the NBS and Cambridge Corp. performed cryogenics work for the thermonuclear program. The thermonuclear device tested in fall 1952 by Los Alamos utilized liquid deuterium, which required extensive cryogenic equipment. The group at Princeton University, Project Matterhorn, conducted theoretical calculations under the direction of John A. Wheeler.

94. Dean, *Forging the Atomic Shield*, p. 208.

95. JCAE, "Chronology of Second Laboratory Events," 16 June 1952 draft, entry for 3 April 1952.

sociation the second laboratory, should not be presented to the president until the AEC and the DOD had an opportunity to consider the options jointly, as required by presidential directive. Dean rehearsed the familiar arguments: the issue should not go to the President because it was a joint AEC-DOD responsibility to determine the rate and scale of the atomic weapons program and that a second laboratory was a bad idea. Dean did mention the possibility of strengthening the University of California Radiation Laboratory weapons related work as a possible solution. Lay agreed, but still pressed Dean. The issue needed resolution.[96]

Meanwhile, the Air Force still wanted action. Air Force Chief Scientist David Griggs, who had arranged Teller's meetings Doolittle and Finletter, told a JCAE staff member that the H-bomb issue was "boiling over." Referring to "almost . . . criminal negligence" in the H-bomb program—the five year delay, the failure to establish a second laboratory—Griggs warned the Air Force was prepared to take action if the AEC did not.[97] The Air Force legal counsel had already investigated the legal questions involved and concluded the Air Force had the legal authority to proceed in sponsoring its own laboratory.[98] The Chicago Midway Laboratories could manage the new laboratory for the Air Force.[99] The recently established Air Research and Development Command (ARDC) would have been the sponsoring agency. Created in 1951, it was tasked to consolidate Air Force research programs under its command. Another factor for the Air Force was its desire to engage university research scientists in support of Air Force goals. The Air Force proposal to establish a center of physics research in support of thermonuclear weapon development can thus also be seen as part of its larger effort to expand the Air Force science base.[100]

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96. Dean, *Forging the Atomic Shield*, pp. 209-211 (quote on p. 209).

97. Walker to Borden, "Thermo Nuclear Program." Walker refers to "Mr. X," who feared losing his job if it were known he had met with Walker, according to the memo. Subsequent events strongly suggest that "Mr. X" is David Griggs. Griggs was a key actor within the Air Force on the second laboratory issue, and the government "agency" for which Mr. X worked, according to the memo, had contacted E. O. Lawrence at UCRL. This is consistent with both Griggs' and the Air Force's active involvement in the second laboratory issue.

98. Walter A. Hamilton to John S. Borden, "Thermonuclear Requirements and Second Laboratory," 9 May 1952. The AEC had legal authority to manage the U.S. nuclear weapon R&D program and is likely to have contested an Air Force effort to establish a nuclear weapons laboratory.

99. The Air Force received a second laboratory proposal on 25 March 1952, according to JCAE, "Chronology of Second Laboratory Events," 16 June 1952 draft. The proposal's author is not specified, but Edward Teller later sent a "revised" proposal to ARDC, making him the likely author of the original. See Teller to E. E. Partridge, "Revised 'Program' for Proposal of Chicago Midway Laboratories for Work in the Field of Thermonuclear Weapons," 29 April 1952. Partridge was ARDC Commanding General.

100. See Nick A. Komons, *Science and the Air Force: A History of the Air Force Office of Scientific Research*, Report OAR 66-7 (Arlington, Va.: USAF, 1966), for an informative history of the Air Force's early organization and sponsorship of scientific research.

The JCAE learned in early April 1952 of Air Force ambitions.[101] McMahon used the opportunity to renew his own advocacy. He wrote to the Joint Chiefs of Staff, the AEC, and the Department of State to inquire again about their views on the H-bomb program and the second laboratory proposal.[102] Teller's dissatisfaction continued, as he let Dean and the JCAE staff know.[103] He disliked the AEC solution, endorsed by DOD, of building up the H-bomb program through ongoing work in established groups. Groups working "piecemeal," would be worse than no second laboratory at all. It might be a way for the AEC of "saving face and pleasing everyone," but it was no an adequate solution for those advocating the "most vigorous sort of competitive and unified second laboratory."[104]

Dean also learned of the Air Force's continuing interest in establishing its own nuclear research laboratory. The chairman of the Executive Committee, Associated Universities, Incorporated (AUI), told Dean he had discussed with the Air Force a possible AUI-managed Air Force center of nuclear physics in Chicago. The Air Force had also had discussions with representatives of the Brookhaven laboratory in New York. This despite the 1 April agreement among DOD, State, and the AEC to hold off. Dean hoped no scientists would join a possible Air Force effort until the AEC resolved the second laboratory issue, which he expected to be soon. He was sending Kenneth E. Fields, the director of the AEC's Division of Military Application, to confer with Bradbury at Los Alamos and with Lawrence in Berkeley. Fields would also stop in Chicago, where he hoped to learn more about Air Force intentions, perhaps from Teller, the Air Force, or Midway Laboratory representatives.[105]

The California Solution

UCRL director E. O. Lawrence was personally interested in national defense. The Nobel Prize-winning physicist and founder of the Radiation Laboratory had been instrumental at the close of World War II in persuading the reluctant regents of the University of California to continue their management of Los Alamos. He has also sought to expand UCRL's role in national

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101. William L. Borden and John S. Walker to Brien McMahon, 4 April 1952.

102. See, e.g., Brien McMahon to Dean, 14 April 1952, and John S. Walker, "Thermonuclear Agenda," 8 April 1952.

103. Dean, *Forging the Atomic Shield*, p. 212; John S. Walker, "Conversation with Dr. Edward Teller on the evening of Tuesday, April 15, 1952," 17 April 1952.

104. Walker, "Conversation with Dr. Edward Teller," 17 April 1952.

105. Dean, *Forging the Atomic Shield*, pp. 212-213.

defense after the 1949 Soviet atomic bomb test.[106] Lawrence's enthusiasm resulted in AEC funding for the so-called Materials Test Accelerator (MTA), a giant linear accelerator intended to supply tritium for the thermonuclear program and produce plutonium from depleted uranium. Operated by the California Research & Development Corporation for UCRL, MTA was located in Livermore, forty miles southeast of Berkeley.

Also in Livermore, in the former naval infirmary at the Livermore Naval Air Station, scientists and engineers had been designing and building the diagnostic experiments for the Los Alamos George nuclear test conducted in Operation Greenhouse in spring of 1951.[107] One goal of the George shot had been to show that a fission explosion could ignite thermonuclear fuel. Work on the George diagnostics started in Berkeley but had outgrown the UCRL facilities. The CR&D Corporation in Livermore had provided building alterations and services required by the MP, or Measurements Project, as the George diagnostics work was called. Although the work went well, the Greenhouse team had disbanded by February 1952. Team co-leader Herbert F. York, Lawrence's former student and colleague, returned to Berkeley from Enewetak, where the latter stages of the work had been based.[108]

AEC Commissioner Murray broached the second laboratory proposal to Lawrence in December 1951. In January, Lawrence sent York to Los Alamos, Princeton, Chicago, and Washington for discussions.[109] York spoke with Los Alamos scientists about establishing a permanent UCRL installation in Livermore to support Los Alamos nuclear test diagnostics along the lines of the earlier Greenhouse work.[110] And early in February 1952, Teller toured the Livermore site with Lawrence.[111] Soon after, Teller began his series of briefings for the service secretaries,

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106. Barton C. Hacker, "A Modest Proposal: Berkeley and the Second Laboratory, 1949-1954," presented in the Symposium on Postwar Big Science and Technology, XIXth International Congress of the History of Science, Zaragoza, 22-29 August 1993, pp. 2-3.

107. For information regarding UCRL participation in Greenhouse, see Kaman Tempo. *Operation Greenhouse: 1951*, by L. H. Berkhouse et al., Report DNA 6034F (Santa Barbara, Cal., 15 June 1983), pp. 147-150, 228.

108. See Chester G. Brinck to Leonard Jacobvitz, "Participation of the University of California Radiation Laboratory in Weapons Development Program," 29 April 1954, for a chronology of how UCRL became involved in the nuclear weapons program from the perspective of the AEC Office of the General Counsel. For George diagnostic work at Livermore, see Hacker, "A Modest Proposal," pp. 5-6; York, *The Advisors*, p. 127; and for the Livermore site before nuclear research, see Stephen C. Wofford, "Livermore Naval Air Station History," n.d., unpublished mss., 14 pages.

109. Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), pp. 62-63.

110. Herbert F. York to E. O. Lawrence, "Conversation between A. C. Graves (LASL-J Division) and H. F. York (UCRL) February 1952," 26 Feb. 1952.

111. Hewlett and Duncan, *Atomic Shield*, p. 582.

DOD, and ultimately the NSC Special Committee on Atomic Weapons. Pressure on the AEC, particularly from the Department of Defense, grew too strong to repel. To Oppenheimer it seemed a decision "forced by high pressure methods." But the Air Force had its own agenda.

Princeton had been among the sites considered, but prominent scientists there were reluctant to see the university extend its involvement beyond Project Matterhorn, the name of the group performing thermonuclear calculations at Princeton for Los Alamos.[112] The Radiation Laboratory in Berkeley, however, held great appeal for the AEC. Many of its young scientists harkening from the Greenhouse diagnostic experiments remained. Lawrence himself was a familiar, reliable, and staunch partner. He was respected by the military and second laboratory advocates. Lawrence's own interest may have been enhanced by the prospect that the AEC was preparing to terminate the MTA project, rendered superfluous by uranium discoveries on the Colorado plateau. Lawrence, too, had lost interest in the project in favor of new cyclotrons.[113]

Other GAC members agreed with mathematician John von Neumann that "it made very good sense to ask Berkeley to help out with the test program." They recommended the AEC establish an experimental test program in Livermore, directed by York, backed by Lawrence, and reporting to Los Alamos. They encouraged the Radiation Laboratory to "go ahead on a broader front as long as it could do so without pirating Los Alamos people."[114]

The pressure was still on Dean in early May who received another telephone phone call from NSC Executive Director Lay. Lay had informed the president about DOD concerns regarding the thermonuclear program while assuring him they were being resolved jointly by DOD and the AEC. Dean assured Lay the AEC had "a program that makes sense," and told him the GAC had addressed itself to it recently. Dean would contact Secretary of Defense Lovett. And a briefing would be prepared for the president as soon as the AEC commissioners had agreed on a course of action.[115]

One week later, after receiving the formal GAC recommendation for bringing UCRL into the thermonuclear program, Dean outlined the options for his fellow commissioners. They included: transferring fission work elsewhere, allowing Los Alamos to concentrate on the thermo-

112. Ibid., pp. 536, 543-544.

113. Minutes of 30th GAC Meeting, 27-29 April 1952, Washington, D.C., pp. 23, 32-33, on pressures on AEC for second laboratory, and pp. 33-34 on MTA. For more on MTA, see Hacker, "A Modest Proposal," p. 10 and n. 24; Hewlett and Duncan, *Atomic Shield*, p. 583.

114. Minutes of 30th GAC Meeting, quotes from p. 33. See GAC to Gordon Dean, 30 April 1952, attached, pp. 4-5, for recommendations on second laboratory.

115. Dean, *Forging the Atomic Shield*, pp. 214-215.

nuclear program; establishing a large-scale across-the-board laboratory to compete with Los Alamos; establishing special-purpose laboratories at new or existing sites such as Rocky Flats; or transferring all thermonuclear work to a new site, supplementing the work of Los Alamos.[116] Insisting on taking no steps which would impair or destroy the effectiveness of Los Alamos, Dean supported Bradbury's plan to recruit additional Los Alamos scientific and technical staff. He also favored continuing support of John Wheeler's Matterhorn group at Princeton. UCRL would be asked to provide diagnostic support for Los Alamos thermonuclear tests. Dean also urged that UCRL's interest in thermonuclear research be encouraged. The commissioners accepted Dean's recommendation at their executive session on 27 May 1952.[117] Authority to approve UCRL's participation in the thermonuclear program resided with the Regents of the University of California, and Dean requested and secured their approval soon after.[118]

Dean made sure to inform the military, including MLC Chairman LeBaron, of AEC actions. He promised a "round-up letter" to Secretary of Defense Lovett. Under Secretary of the Air Force Roswell L. Gilpatric told Dean the Air Force was "looking forward to a new regime." Finally, on 9 June Dean informed Deputy Secretary of Defense Foster of the steps taken by the AEC.[119] A few weeks later, Los Alamos director Bradbury was joined by the commissioners and DOD and State Department officials to brief President Truman on the H-bomb program. The briefing, which Dean had sought to put off while the AEC and the DOD were at loggerheads over the pace and scope of the program, was conducted without incident and revealed that Los Alamos had made substantial progress. In the planning stages for over one year, a full-scale thermonuclear test was scheduled for the fall. There is no indication the second laboratory was discussed at the president's briefing.[120]

President Truman once quipped, "I sit here all day trying to persuade people to do the things they ought to have sense enough to do without my persuading them. . . . That's all the powers of the President amount to."[121] Although Truman was probably not referring to the H-bomb decision, his comment serves as a reminder that a presidential decision marks only one step in the

116. Gordon E. Dean to AEC commissioners, untitled memo, 12 May 1952, circulated 29 May 1952.

117. Dean to AEC, 12 May 1952.

118. Gordon E. Dean to Univ. of California Regents, 9 June 1952.

119. Dean, *Forging the Atomic Shield*, p. 215, n. 3.

120. Ibid., p. 218.

121. Richard E. Neustadt, *Presidential Power: The Politics of Leadership, with Reflections on Johnson and Nixon* (New York: John Wiley & Sons, 1976), pp. 78-79.

policy process. The implementation stage involves bureaucratic actors who help determine organizational structures and resource allocations that will give the new policy concrete form.

Truman's January 1950 announcement that the United States would pursue the development of the H-bomb did not settle the question of the relative allocation of resources between fission and thermonuclear weapons, the strategies contemplated for its use, nor which of the services would benefit. These questions were not only of interest to the Atomic Energy Commission, the Department of Defense, and the armed services, but also to the Joint Committee on Atomic Energy. This was the broader political context in which the competing organizational interests that led to the founding of the laboratory at Livermore were considered. At stake for the military and the AEC were questions about military vs. civilian control of nuclear weapons. Likewise, the JCAE's advocacy of a second laboratory was just one piece of its broader strategy for expanding the atomic weapons program.

Ultimately, as this chapter has shown, the Los Alamos H-bomb program was not the core issue in the debate over a second laboratory. Los Alamos' subsequent success in developing the H-bomb—though this outcome could not have been known with absolute certainty at the time—suggests that to the extent Bradbury failed it was in the political arena, not in the technical program. Rather, at stake was the allocation of resources among the armed services and the relative priority of H-bomb and fission weapon development. These were issues related to the Los Alamos program to the extent that military priorities or the lack thereof were reflected in the Los Alamos weapons program. This was precisely the root of the problem. The success of the Los Alamos H-bomb program, and the Air Force's subsequent productive relationship with Los Alamos over the next forty years, suggest the Los Alamos H-bomb program per se was not the principal issue even if it was hotly debated.

The turning point on the second laboratory came when the Air Force gained the backing of the Department of Defense for the proposal. Teller until then been largely unsuccessful in his efforts. His criticism of Los Alamos prompted the Air Force to enroll him in its cause: competition with the Army and Navy over shares of nuclear weapons missions and resources. Discussing these issues with the president was a risky proposition for the AEC, forcing a retreat. Justified or not, DOD criticism of the H-bomb program and the suggestion that Los Alamos was not up to its responsibilities would damage AEC credibility and threatened to erode its control of the nuclear weapons program. An Air Force sponsored nuclear laboratory might have run into legal problems, but it was a risk the AEC did not want to take.

DOD had no formal authority over the AEC, but the AEC's credibility and funding depended on satisfying DOD requirements and maintaining the goodwill of DOD officials. Despite the special status of the nuclear weapons program, the AEC was subordinate to the Department of Defense, as indicated by the fact that the chairman of the AEC interacted with the Deputy Secretary of Defense on matters of business between the two organizations. The AEC would not emerge from a meeting with the president without damage to its credibility and legitimacy. DOD leverage over the AEC was mitigated by DOD's unwillingness to acquire the responsibility and the costs of developing nuclear warheads, a cost borne exclusively the AEC. For its part, DOD seems to have had little real interest in getting involved in a military-sponsored nuclear weapons research and development laboratory. The idea was dropped readily when the AEC offered to establish a second nuclear weapons research effort.

This chapter provides the framework for understanding Livermore's subsequent development, and the dynamics of the two-laboratory system. We have seen how the competing interests of different bureaucratic actors led to the founding of the laboratory at Livermore. Dean devised a solution for the AEC that appeased the military, while addressing concerns about the detrimental impact of a second laboratory on Los Alamos. The UCRL solution was acceptable to DOD, particularly since UCRL director E. O. Lawrence was trusted. We shall see in the next chapter that DOD nevertheless continued to monitor AEC actions.

3. GROUND RULES FOR LIVERMORE
Incentives and Constraints That Shaped the Laboratory
1952-1953

The factors that lead to the creation of a new organization establish important parameters for its future development, including its form and functions. These parameters serve as the organization's starting point and have a lasting impact on its development.[1] The political and organizational environment in which Livermore was formed determined its initial resources, physical structure, location, and personnel. The institutional framework provided by the University of California Radiation Laboratory—the staff, resources, and organizational framework—shaped the new laboratory at Livermore. Concerns about the detrimental impact of a second laboratory on Los Alamos continued to shape AEC policies. The Department of Defense, which had been critical in prompting a reluctant AEC to act, continued to play an important role. This chapter considers the impact of UCRL, DOD, AEC, and the laboratories in shaping the two-laboratory system.

Livermore is Not <u>the</u> Second Lab

AEC Chairman Gordon Dean informed JCAE Chairman Brien McMahon in early June 1952 that the Berkeley Radiation Laboratory would assist the Los Alamos H-bomb program, providing

1. W. Richard Scott, *Organizations: Rational, Natural, and Open Systems*, 3rd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1991), p. 171, and chap. 7, "Creating Organizations."

diagnostic support. The move did not create a full-fledged laboratory, according to Dean, nor was it intended to. Instead, it constituted just one of a number of AEC actions to strengthen the H-bomb program. Dean's reasoning was consistent with what he and other opponents had argued throughout the debate over the second weapons. A new laboratory would "dilute" Los Alamos efforts with no compensating advantages. Instead, UCRL-Livermore would perform diagnostics for Los Alamos thermonuclear tests, much as the Livermore Measurements Project group had done in 1951 the Greenhouse tests. Livermore scientists would be encouraged to submit their own proposals for thermonuclear research, but the diagnostic work took precedence, at least for the present.[2]

McMahon forwarded relevant excerpts from Dean's letter to Military Liaison Committee Chairman Robert LeBaron. Did the MLC object to the AEC's formulation of the new laboratory's mission?[3] LeBaron declined to invoke MLC privileges under a statutory appeal procedure, preferring to consider the "substance" of the Commission's effort, not its expressed policy position, in ultimately judging AEC actions. AEC actions would be monitored, however, for proof of commitment.[4] Dean had in the meantime also outlined for LeBaron AEC plans for the second laboratory and thermonuclear program.[5]

An early June 1952 planning document prepared by Herbert York reflected the AEC desire that Livermore focus on diagnostics, at least initially. Yet by September, the AEC had approved plans for the laboratory to conduct its own nuclear tests.[6] What happened? The answer lies in the military's continued watchfulness and Teller's agreement to join the laboratory.

York wanted to build towards a laboratory with the full complement of nuclear weapon design, development, and testing capabilities. Los Alamos director Norris Bradbury had early on noted Livermore ambitions, observing its scientists were unwilling to regard diagnostic activities as an "adequate goal for a laboratory of the scope . . . [they] contemplate[d]."[7] As early as May

2. JCAE, "Chronology of Second Laboratory Events," 16 June 1952 draft, entry for 10 June 1952. Dean's letter to McMahon on this date was in response to McMahon's 14 April 1952 inquiry, as noted in the chronology.

3. JCAE, "Policy and Progress in the H-bomb Program: A Chronology of Leading Events," Jan. 1, 1953, entry for 11 June 1952.

4. Ibid., entry for 30 June 1952.

5. According to Robert LeBaron to William C. Foster, "Suggested Outline for Discussion of Thermonuclear Program Status at Joint Secretaries' Meeting 14 August 1952," 13 August 1952.

6. AEC Meeting No. 744, 8 Sept. 1952, pp. 461, 463.

7. Norris E. Bradbury, "Observations of the Livermore Laboratory Proposal," 21 May 1952. The dates of the visit were 11-12 May 1952.

1952, York had projected the need for bomb manufacture and assembly facilities by fall 1952.[8] The AEC emphasized to UCRL director Lawrence the importance of Livermore's diagnostic work. Uncertain though the budget for Livermore's fiscal year 1953 program was, the urgency of the Los Alamos H-bomb program meant funding would "somehow be made available." The prospect of the possible future Livermore nuclear tests, however, was downplayed.[9] By early June Livermore plans were less obviously ambitious.[10]

By late June 1952, York's description of the Livermore program had shifted again. The design, test, and development of thermonuclear weapons would be the "primary objective" of the laboratory though its first order of business remained diagnostics. The planning document had a new section, entitled "Weapons Operations," which included provisions for design and development of thermonuclear weapons, fabrication and assembly facilities, as well as a test program.[11]

Although Teller recalls his early support for establishing the laboratory in Livermore, contemporary documentation suggests otherwise.[12] Shortly after his February 1952 visit with Lawrence at UCRL Teller wrote York expressing his concerns about prospects for a Livermore laboratory. He told York, "I do not believe . . . you will have an easy time and I wish you the very best of luck. . . . For the moment, the assistance I can offer must be limited but . . . I should like to help as best I can."[13] Teller continued to pursue the possibility of an Air Force sponsored laboratory even after the AEC's early April agreement to expand the thermonuclear program at Livermore. This solution was inadequate, from Teller's perspective, and he feared would merely lead to a series of "Wheeler groups," working piecemeal on small problems. Teller was referring to the Princeton group established under John Wheeler to assist Los Alamos thermonuclear calculations. Teller instead had urged that the new laboratory be set up at the AEC Rocky Flats plant in Colorado.[14] And in late April, almost one month after the NSC Spe

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8. Herbert F. York, "Conversations with Norris E. Bradbury 5/11/52-5/12/52 at Berkeley," 20 May 1952.

9. Kenneth E. Fields to E. O.. Lawrence, 13 June 1952.

10. York, "Proposed Program for Project Whitney," 3 June 1952.

11. Herbert F. York to W. B. Reynolds, "Project Whitney, UCRL: Preliminary Operations Plant Personnel Requirements," 25 June 1952.

12. For Teller's view, see Edward Teller with Allen Brown, *The Legacy of Hiroshima* (Garden City, N.Y.: Doubleday, 1962), p. 60.

13. Edward Teller to Herbert F. York, 7 Feb. 1952.

14. John S. Walker, "Conversation with Dr. Edward Teller on the Evening of Tuesday, April 15, 1952," 17 April 1952.

cial Committee meeting where the AEC had agreed to set up a second weapon research center, Teller had resubmitted a proposal to the Air Force for a weapons laboratory at the Midway Laboratories in Chicago.[15] By late June, Teller had not yet agreed to go to Livermore. Part of his reluctance stemmed from personal considerations. He had strong personal and professional ties in Chicago, including a faculty position at the University of Chicago and an emigre community in which he felt comfortable.[16]

York recalls Teller as "extremely dissatisfied" with what he considered the AEC's vague plans for Livermore. At an early July 1952 Berkeley reception celebrating the new enterprise Teller announced he would have nothing to do with it. Lawrence was prepared to proceed without Teller, even suggesting the laboratory would fare better without him.[17] Memories are notoriously faulty on such matters, but Lawrence had more important matters on his mind than Teller's plans. For Lawrence, the health of his Radiation Laboratory in Berkeley came first, and that depended primarily on maintaining good relations with the AEC.

Why and when did Teller join Livermore? By summer 1952 Teller's influence on the second laboratory question had declined. It derived in the first instance chiefly from Air Force backing, bolstered by DOD support, which had caused the AEC to act. The UCRL proposal had eased military concerns, though DOD remained watchful of AEC actions. Teller could still generate pressure on the AEC, but it had grown harder to do so given the AEC-DOD agreement. The Air Force was still interested in a laboratory of its own, but DOD had grown lukewarm to the proposal.

Military Meddling

Despite Teller's waning influence, Dean still hoped to recruit Teller to Livermore. Not only would Teller bring his formidable intellect, his participation might help persuade military skeptics the AEC was serious in its intentions. Failure to do so would continue to expose the AEC to criticism, if not outright threats of a DOD-backed second laboratory. Accordingly, Dean told Deputy Secretary of Defense William C. Foster in early June 1952 of his hopes that

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15. Edward Teller to E. E. Partridge, "Revised 'Program' for Proposal of Chicago Midway Laboratories for Work in the Field of Thermonuclear Weapons," 29 April 1952.

16. Teller was looking forward to moving into a home he had recently purchased next to Enrico Fermi, whom he greatly admired. This contributed to his reluctance to move from Chicago, as did the absence of an emigre community with which he could feel at home in California. My discussion with Judy Shoolery, who is assisting Edward Teller in writing his autobiography, 10 Nov. 1994.

17. Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), pp. 67-68.

Lawrence could induce Teller to join the UCRL-Livermore team.[18] Precisely when he agreed to do so is not clear. Teller recalls arriving in mid-July.[19] Teller's participation in a series of program planning meetings with York, Lawrence, Bradbury, and Los Alamos nuclear test director Alvin C. Graves, Livermore helped solidify Livermore nuclear test plans.[20] DOD's continued watchfulness no dount contributed.

AEC Deputy Director of Military Application Admiral John T. (Chick) Hayward chaired the early program planning meetings in Berkeley convened to discuss UCRL's entry into the weapons business. Participants differed widely about appropriate activities for the new laboratory. Lawrence, a team player, saw the objective as "the furtherance of the over-all program," although he also believed Livermore should operate independently from Los Alamos. Bradbury was clear that Los Alamos "needed help" on diagnostics. Teller objected to the supporting role intended for Livermore. He thought the new laboratory "would have great difficulty in building up . . . if emphasis was kept on present help to LASL." Teller wanted a broad mandate, and wanted to avoid Livermore's becoming a mere "service organization" for Los Alamos. He was "insistent on a specific charter broad in scope and less emphasis on diagnostic help." Those who counseled a go-slow approach to testing nuclear devices and emphasis on diagnostics prevailed, at least initially.[21]

Hayward asked all the questions that remained unanswered about the Livermore effort. What would be the scope of the new laboratory? Was it the second permanent site? Was there to be a second permanent site? What was the new laboratory's budget? Its program? In Hayward's words,

> How far will we go with this effort? Is it our intention to have it grow to the ultimate size of LASL without getting into facilities that are peculiar to that place . . . ? What will be the relative effort budgetwise between the two places? . . . Is Livermore the location of our permanent second effort? Do we intend to make it a permanent installation? What system will be set up for the meshing of the second

18. As cited in Barton J. Bernstein, "Teller, Lawrence, and a New Lab: The Establishment of Livermore," prepared for the workshop on the "Decade of Innovation: Los Alamos, Livermore, and National Security Decisionmaking in the 1950s," Pleasanton, Cal., 19-21 Feb. 1992, p. 28, n. 60.

19. Stanley A. Blumberg and Louis G. Panos, *Edward Teller: Giant of the Golden Age of Physics* (New York: Charles Scribner's Sons, 1990), p. 138. In their authorized biography, Blumberg and Panos state that Teller's family joined him on 14 July 1952.

20. John T. Hayward to AEC, "Trip Report—Berkeley Situation," 23 July 1952.

21. Ibid.

program? I do not believe we can rely solely on E. O. Lawrence and Bradbury to agree or settle it. . . . How do we intend to set up the AEC organization?[22]

Hayward was not idly musing. These were real issues whose resolution, by some counts, would take years. They also greatly interested both DOD and the JCAE. Had the AEC seriously intended to expand the thermonuclear program, or simply sought to avoid the unpleasant consequences of standing pat, as it had for so long in refusing to create a second laboratory? Hayward warned that DOD would watch AEC actions "closely as an indication of whether we really meant this step or it was an expedient brought about by political considerations and not based on sound technical judgment. The clamor will rise to large proportions if we don't keep faith with our statements."[23]

Hayward was right. The military was watching. Deputy Secretary of Defense Foster invited AEC Commissioner T. Keith Glennan to a meeting with LeBaron and the three service secretaries to discuss the thermonuclear program and the second laboratory in August 1952, even as initial plans for Livermore were being formulated. The meeting was instigated by the JCAE's earlier inquiry regarding military views on the adequacy of AEC actions.[24] As LeBaron had urged, they discussed whether "the adequacy of the present program for a second weapons laboratory at Livermore . . . in terms of 'rate and scale' of the thermonuclear effort" was satisfactory. Raising concerns that Teller had earlier voiced, LeBaron sought confirmation of Livermore's independence from Los Alamos. Such confirmation "should remove any doubt about subservience of this activity to Los Alamos direction and should make it clear that the Radiation Laboratory is the source of actual laboratory direction."[25]

Accordingly, the DOD officials asked Glennan to clarify the commission's policy on the second laboratory. After consulting with his colleagues, Glennan was able to assure Foster the commission was "distinctly in favor of the thermonuclear laboratory; there is no murkiness in the situation . . . they would do all that they could to further the project." Whatever lack of clarity may have existed regarding AEC intentions were eliminated, and Foster passed the information to the service secretaries. The MLC Chairman subsequently assured the Joint Committee the

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22. Ibid.

23. Ibid.

24. JCAE, "Policy and Progress in the H-bomb Program," entry for 13 Sept. 1953. The meeting took place at an unspecified earlier date, probably in August.

25. LeBaron to Foster, "Suggested Outline for Discussion," 13 August 1952.

AEC effort was responsive to military concerns. At the same time, however, the MLC would monitor Livermore activities periodically.[26]

Only one thing had been certain in the July AEC-UCRL staff meetings in Berkeley: Livermore would perform diagnostics for Los Alamos thermonuclear tests. They had vetoed Teller's proposed September 1953 thermonuclear test, agreeing spring 1954 was the earliest possible date for such a test.[27] But under the watchful eye of the military, the AEC approved the fall tests at its September 1952 meeting.[28]

The Move to Livermore

Staff badges for gaining entry to the security-controlled Livermore site were laminated over Labor Day weekend. Work began officially on 2 September 1952. The grand ambitions of Teller, York, and others contrasted with conditions. Telephone lines had yet to be connected. Staff members planning to sleep at the laboratory were warned that "plush accommodations" were not available. Lack of plumbing and other facilities delayed use of the old Navy buildings that were to serve as housing and offices. Staff would bring their desks from Berkeley. Desk lamps were in short supply. Mail delivery still needed to be arranged and there was no on-site cafeteria. Commuting presented its own problems. Individuals were warned that they would be responsible for their own speeding tickets![29] How to provide coffee for the staff was a frequent topic of discussion at early meetings.[30] And as one participant recalls, working without air conditioning in the hot summer desert climate of Livermore was a challenge.[31]

Administrative staff for Project Whitney—as the UCRL weapons effort was called—were based in Berkeley as well as in Livermore. Close association with UCRL-Berkeley shaped the organization and culture of Livermore, which functioned as an administrative branch of UCRL until 1973. Much of the scientific staff came from Berkeley, from the university's academic de-

26. JCAE, "Policy and Progress in the H-bomb Program," entry for 13 Sept. 1952. The DOD meeting probably took place in August.

27. Hayward to AEC, "Trip Report," 23 July 1952; Minutes of PW Administrative Meeting, 25 August 1952. Operation Castle was initially scheduled for Sept. 1953, although ultimately conducted in spring 1954. See Barton C. Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley: Univ. of California, 1994), p. 131.

28. AEC Meeting No. 744, 8 Sept. 1952, pp. 461, 463.

29. See Conference, 15 August 1952; Conference, 18 August 1952; and Minutes of PW Administrative Meeting, 4 Sept. 1952.

30. See, e.g., Minutes of PW Administrative Meeting, 26 Sept. 1952.

31. George Bing comment on May 1995 dissertation draft.

partments, as well as UCRL. Many had worked on weapons-related projects at UCRL, on the Materials Test Accelerator and Greenhouse diagnostics, independently or in conjunction with Los Alamos.[32] Most in administrative positions, including Livermore's first director, Herbert F. York, who got his Ph.D. in 1949, were Lawrence's former students. Other important administrative positions were held by Berkeley-based staff.

Recruiting had started early, and on opening day 123 scientific and technical staff were on board.[33] "Research Groups" accounted for 70, mechanical and electrical engineering the remainder. Livermore's ties to UCRL were evident in the makeup of the staff. Of 35 scientists, 23 had moved to Livermore from UCRL or were new Berkeley Ph.D.'s.[34] Nine had previous weapons experience, probably in Greenhouse.[35] Berkeley academic departments served as home bases for many. The permeable barrier between Livermore and Berkeley allowed an easy flow from UCRL to Livermore and back, easing the task of recruiting. UCRL's organizational framework allowed Livermore a more relaxed and unstructured atmosphere. The relatively small Livermore staff contributed to informality and overlapping organizational identities, and there were few titles. York had the duties of director, working daily at Livermore in his office in the lead-lined x-ray room of the former naval station hospital building. It would be two years before he would sign his letters as director. Lawrence meanwhile, as UCRL director, co-signed official letters.

The scientific background and training of Livermore's recruits tell more than do numbers about the kind of scientific and technical problems they found interesting. Livermore staff were particularly strong in experimental physics, a consequence of the UCRL association.[36] By contrast, strength in theoretical physics was a distinguishing feature of much of the Los Alamos staff. This was a legacy from the Manhattan Project era that began with Oppenheimer, the premier theoretical physicist who had left Berkeley to head the wartime project. Hans Bethe and

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32. Herbert F. York to E. O. Lawrence, "Personnel and Jobs Now Underway on New Weapons Work," 26 Feb. 1952.

33. AEC, "Detailed Justification of Operating Costs," for the Univ. of California Radiation Laboratory, 3 Sept. 1952. The figures do not include guards or administrative and service staff.

34. AEC, "Detailed Justification of Operating Costs," for UCRL, 3 Sept. 1952. See pp. 3-4 for breakdown.

35. For a discussion of the UCRL Greenhouse work see, Barton C. Hacker, "A Modest Proposal: Berkeley and the Second Laboratory, 1949-1954," presented at the Symposium on Postwar Big Science and Technology, XIXth International Congress of the History of Science, Zaragoza, 22-29 August 1993, pp. 5-8; "20 Years in Livermore," *Newsline* [Lawrence Livermore Laboratory], Sept. 1972, p. 5.

36. Discussions at LLNL.

Richard Feynman, who later received Nobel Prizes for their work in theoretical physics, were two prominent theoreticians who worked or consulted at Los Alamos.

Although administratively based on UCRL/Berkeley disciplinary departments, the Livermore scientific and engineering staff was organized around laboratory programs. Livermore's matrix organizational structure, though not initially called by this term, was rooted in the practice of grouping staff with diverse disciplinary backgrounds together to form programs. The group responsible for overseeing the design, development, and test of a particular nuclear weapon, for example, would include physicists, engineers, chemists, in short, all the necessary specialists.[37] Los Alamos was organized in a university-like structure more strictly along disciplinary lines.

Divergent organizational philosophies and practices fed differences in the design, test, and development philosophy of each laboratory. Los Alamos' compartmentalized approach was foreign to Livermore scientists, who preferred the program groups of the matrix structure. The astonishment of one Livermore scientist at the Los Alamos way of doing business is captured in the following:

> some folks would work on one part [of a bomb] . . . others on another part. . . . They would write memos to each other. This was incomprehensible to me as a way of doing business . . . The effort at Livermore was much more integrated. . . . I recall . . . the term 'separate fiefdoms' [used to describe Los Alamos] . . . There were no fiefdoms at Livermore. We were all working together.[38]

Different practices dominated at Los Alamos, where scientists favored designs amenable to theoretical calculations. This was at least in part a consequence of the influence of the dominance of the laboratory's theoreticians. Calculations helped predict the behavior of nuclear devices and thereby reduced the need for tests. Livermore scientists were comfortable pursuing designs for which it was "hard to make advanced calculations of expected results," a consequence of their experimentally-oriented background. As a UCRL budget document explained, experiment provided Livermore scientists the means of studying designs "that might go unused because of difficulties in making calculations."[39] Another useful technique was computer modelling, in which Livermore invested substantial effort. Designs difficult to calculate meant Livermore developed empirically based computer models, resulting in a more incremental approach to weapons de-

37. See, e.g., Minutes of PW Administrative Meeting, 25 August 1952; AEC, Note by the Secretary, "UCRL Testing Program," 30 Dec. 1952; "Briefing for DMA Personnel by LRL on Their Research and Development Work, November 4, 1959," 27 Nov. 1959 draft, John Foster comments, p. 1.

38. Interview with Peter Moulthrop, LLNL, 6 March 1991.

39. AEC, "Detailed Justification of Operating Costs," for UCRL, 3 Sept. 1952.

sign: computer modeling, tests, then more modelling in preparation for the next test.[40] Livermore thus made its earliest contributions to the weapons program in areas especially difficult to calculate theoretically.[41] Livermore's roots in the Berkeley Radiation Laboratory—a premier experimental research organization from which Project Whitney's first scientists were recruited—thus profoundly shaped the laboratory and its programs. One could say that the UCRL connection is reflected in the physical characteristics of the weapons developed by the laboratory.

Shaping the Program

Lawrence and Teller outlined laboratory plans for the AEC at its 8 September 1952 meeting in Washington. Work at Livermore had officially started only the week before, but much had changed since the previous spring when the AEC first agreed to make UCRL the site of additional thermonuclear research. Teller had agreed to join the effort, and concrete planning had commenced for a Livermore test program. Lawrence informed the AEC commissioners that although the laboratory's most valuable contribution had initially been to assist Los Alamos with diagnostic measurements, the more "ambitions" program emerging would include a major new project.[42]

The commissioners were willing to encourage the growth of the new laboratory, but were not persuaded it should become the permanent site for a second laboratory. They also wanted to protect the Los Alamos H-bomb program.[43] Once the subject of abstract discussions, these dual interests now began shaping Livermore's institutional and programmatic development, with long-term as well as immediate consequences. In concrete terms, AEC concerns meant that Livermore should not compete with Los Alamos for staff and scarce material resources. The commissioners also wanted the laboratories to avoid program duplication. Since Livermore was the new laboratory and lacked an established track record, it bore the burden to comply. As one scientist recalls the informal mandate to Livermore: "If you're going to build bombs don't build them like Los Alamos bombs."[44] Although Teller and the MLC had wanted a well-defined charter for the new laboratory, Dean still judged that the laboratory's informal charter to explore "new ideas" would make a clear program definition "impossible."[45]

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40. Norris E. Bradbury to Carroll L. Tyler, "Future Full-Scale Weapons Tests," 6 May 1953.

41. Discussions at LLNL and LASL.

42. AEC Meeting No. 744, 8 Sept. 1952, p. 460.

43. Gordon Dean to AEC Commissioners, untitled memo, 12 May 1952, circulated 29 May 1952.

44. Interview with Frank Eby, LLNL, 12 March 1991.

45. AEC Meeting No. 744, 8 Sept. 1952, p. 463.

Lawrence and Teller had answers to such concerns. Teller assured the commissioners that Livermore's planned thermonuclear test would "not entail competition with Los Alamos for materials." And Lawrence told them that "[e]xcellent cooperation between Los Alamos and Livermore precludes the possibility of overlapping efforts."[46] Lawrence also underscored the incremental nature of the step taken by the AEC. Noting the close ties between Livermore and UCRL, he stressed Livermore as an extension of UCRL. Maximizing exchange and common services between the two groups would keep additional administrative costs to a minimum.[47] Downplaying the distinction between Livermore and Berkeley was a deliberate decision. UCRL staff members agreed that while "Project Whitney" might conveniently be used to identify the Livermore group its use should not indicate "any separateness" from UCRL-Berkeley.[48]

Initially, no new facilities were planned. The new laboratory relied on existing buildings, mainly structures from the old naval air station. In June York was told he could buy equipment and make minor structural modifications. Funding for new buildings, laboratories, or any other major facilities should not be expected.[49] Dean repeated these strictures in September and the UCRL business manager assured the AEC Chairman the laboratory would comply. Any construction would thus be "simple and strictly functional."[50]

Los Alamos also enjoyed well-established relationships with contractors and procurement firms. The newcomer to the weapons field, Livermore had to overcome this disadvantage in developing its own relationships with these same firms. The AEC's San Francisco Operations Office (SFOO), for example, observed that procurement problems had arisen since many items required were available only from firms Los Alamos. The SFOO manager had sometimes acted as a "referee" between the two laboratories.[51] Livermore also encountered problems when competing for the time and resources of government contractors. As a result, the laboratory asked the AEC to explicitly authorize the Oak Ridge facility to assign personnel and work capacity specifically to Livermore to eliminate the problem of "our work taking the back seat [to Los Alamos] all the time."[52]

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46. Ibid., p. 461.

47. Ibid., p. 462.

48. Minutes of PW Administrative Meeting, 4 Sept. 1952.

49. Kenneth E. Fields to E. O. Lawrence, 13 June 1952.

50. AEC Meeting No. 744, 8 Sept. 1952, p. 462.

51. AEC Meeting No. 852, 14 April 1953, p. 237.

52. Minutes of PW Technical Program Meeting, 9 Dec. 1953.

Although the laboratory grew rapidly, it long felt the impact of AEC-imposed limits. Nearly a decade passed before Livermore acquired the full complement of facilities required for nuclear weapons design and testing. The laboratory added to its facilities and programs piecemeal, relying on Los Alamos for testing and manufacturing. It also relied occasionally on Los Alamos for key weapons components, including high explosive assemblies, casings, and Los Alamos-designed primaries for Livermore's first thermonuclear devices.[53]

John von Neumann, chairman of the GAC's Weapon Subcommittee, declared the Los Alamos weapons program in good shape following the subcommittee's October 1952 visit to the laboratories. Fission weapon development was going well, as was the H-bomb test program. Livermore was pursuing different projects with a different approach. The new laboratory worked on "bolder" designs, less certain of success than those of Los Alamos.[54]

Teller's campaign for a second laboratory had focused on alleged deficiencies of the Los Alamos H-bomb program. His interests, however, were not so narrow, nor did he want the new laboratory limited to thermonuclear research. In December 1951, for example, Teller had told the General Advisory Committee the new laboratory should be encouraged to "plan and explore all kinds of new developments in the field of bomb physics."[55] As it turned out, Teller proposed that it be two fission weapon devices that Livermore test in the 1953 Operation Upshot-Knothole nuclear series. For classification purposes, this study will refer to these as the "Basilisk" design or devices. Although they were fission devices, their principal justification was their relevance to the thermonuclear device Teller hoped the laboratory would test the following year in the 1954 Operation Castle series. Livermore's Basilisk program did not interfere with Los Alamos, duplicate areas of Los Alamos investigation, or draw on scarce material resources, all consistent with AEC expectations of the new laboratory.

Basilisks had a history almost as long as the nuclear program. During the wartime Manhattan Project, Los Alamos scientists had investigated the possible advantages of Basilisks in fission weapon design. Calculations in 1944, however, had showed them inefficient users of nuclear fuel.[56] Hopes of making smaller, more efficient bombs rekindled interest. Ultimately, however, their inefficiency was considered too great a penalty, despite their promise of smaller weapons.

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53. AEC, "Detailed Justification of Operating Costs," for UCRL, 3 Sept. 1952, pp. 6-7.

54. Minutes of 33rd GAC Meeting, Washington, D.C., 5-7 Feb. 1953, p. 2.

55. Edward Teller, "Statement to the General Advisory Committee on Need of Second Weapons Laboratory," 7 Dec. 1951, p. 3; Minutes of 28th GAC Meeting, Washington, D.C., 12-14 Dec. 1951, p. 11.

56. David Hawkins, "Towards Trinity," in *Project Y: The Los Alamos Story*, vol. 2 of "History of Modern Physics, 1800-1950" (Los Angeles and San Francisco: Tomash, 1983), p. 70.

The Military Liaison Committee Chairman thus requested Los Alamos give lower priority to its work on Basilisks and focus on smaller conventional implosion weapons and boosters.[57] Los Alamos dropped planned Basilisk tests and boosting became the method of choice for efficient fissioning and small weapons design.

In 1952 Livermore argued that "new ideas [had] . . . entered into the picture," again stimulating interest in Basilisks.[58] In particular, aspects of the design were now of interest in learning more about certain thermonuclear reactions. Measurements of these reactions could contribute to better understanding of the processes involved in the two Livermore H-bomb tests planned for 1954.[59] Because Basilisks could in principle be made into small size and yield weapons, they were also attractive for their potential application as warheads in air-to-air missiles, which made these small fission devices doubly interesting.[60]

Two Livermore Basilisk devices were tested in spring 1953, just six months after the laboratory's founding. Basilisk I, tested on 31 March 1953, resulted in a much lower yield than expected. One important criterion of a successful test was to ensure that everything associated with the shot, including the tower, was destroyed. According to the Deputy Test Director's report, however, the steel tower for the Basilisk I device was "only one-third vaporized and more than half of it remained standing."[61] It was an inauspicious start for the laboratory. Livermore legend has it that it was an observer from Los Alamos who recorded in a notorious photograph the evidence of the test fizzle—the term used for a device that gives a far lower yield than expected. The photograph hangs in many Livermore offices as a reminder of the laboratory's rough beginnings, as well as a symbol of its role as the "new ideas" laboratory and all the risks entailed. Lore has it that the next tower was made shorter in order to avoid the possibility that it would be incompletely incinerated by Basilisk II, tested on 11 April 1953. This test too, however, ended in a fizzle. Herb York's preliminary report, only eight lines long, which estimated various findings of technical interest, noted the Basilisk II shot tower was "big enough."[62]

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57. See William Webster to AEC, 2 Dec. 1948, cited in Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), p. 39, n. 25.

58. Quoted from "Preoperational Report, October 1952," excerpted in James E. Carothers, Wallace D. Decker, and W. J. (Jim) Frank, "Thirty Years Ago: The [Basilisk Designs]," *Research Monthly* [LLNL] (March 1982), p. 27.

59. Herbert F. York, "Weapons Program Planning, University of California Radiation Laboratory, Livermore, California, December 8, 1952," 8 Dec. 1952.

60. Minutes of PW administrative meeting, 3 March 1953.

61. Carothers, Decker, and Frank, "Thirty Years Ago," pp. 31-32.

62. Herbert F. York, "Early Summary of Information as of April 11, 1953," 13 April 1953.

Meanwhile, success had characterized the Los Alamos test program. On 31 October 1952, less than two months after Livermore opened its gates, Los Alamos set off the world's first large scale thermonuclear explosion, dubbed "Mike."[63] With a yield of more than ten megatons, the explosion from Mike was so powerful it destroyed the small island of Elugelab. The size of a small factory, Mike was not a deliverable weapon, though it proved the concept.[64] The next step was developing deliverable bombs for the Air Force. Liquid-fueled devices like Mike required cryogenic equipment for keeping the deuterium fuel in liquid form, making them bulky for aircraft. "Dry" H-bomb designs were not yet certain and the Air Force wanted deliverable bombs as soon as possible.

Prior to the spring 1953 tests, Bradbury had described Livermore's Basilisk devices as worthy of investigation.[65] Seizing on the test fizzles he used the opportunity to raise questions about the new laboratory. Bradbury charged that Livermore was constrained by the limitations of its staff, which seemed to confirm earlier concerns that staffing a new laboratory would be problematic. He also objected to assumptions contained in an AEC document which implied Livermore might require a more intensive test program than Los Alamos due to the nature of its designs. The document also seemed to imply that Livermore's program was more basic and fundamental and therefore held more promise than Los Alamos'. This might mean shifting nuclear test resources to Livermore, not a happy prospect for Los Alamos. Bradbury thus sought to minimize Livermore's potential contribution to the weapons program, asserting that the laboratory's recent tests were not indicative of a more fundamental research program. Rather, the aim of the experiments was to determine nuclear cross sections of only "problematical interest."[66]

Bradbury argued that Livermore's weapons ideas were not "new" or very different from those pursued by Los Alamos. The laboratories explored different H-bombs designs, but their ultimate objectives were identical: to design narrow diameter weapons with relatively high yields. And the geometry of Los Alamos' approach was more amenable to prior calculation than

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63. See Frank Shelton, *Reflections of a Nuclear Weaponeer* (Colorado Springs: Shelton Enterprise, 1988); based on the author's participation in the nuclear weapons program, the book is especially rich in material on atmospheric testing.

64. DOE History Division, "The United States Nuclear Weapon Testing Program: A Summary History," draft document number DOE/ES-0005, August 1984. For yield, see NVO, *United States Nuclear Tests: July 1945 through September 1992*, Report DOE/NVO-209 (Rev. 14) (Las Vegas, Nev., Dec. 1994), p. 2.

65. Norris E. Bradbury to Carroll L. Tyler, "Nevada Briefings," 26 Feb. 1953.

66. Norris E. Bradbury to Carroll L. Tyler, "Future Full-Scale Weapons Tests," 6 May 1953. Bradbury was probably referring to AEC, Note by Secretary, "UCRL Testing Program," 30 Dec. 1952. This is a cover note for Herbert F. York, "Weapons Program Planning, University of California Radiation Laboratory, Livermore, California, December 8, 1952," 8 Dec. 1952.

Livermore's. Bradbury even challenged the premise that Livermore was working on thermonuclear weapons: "in every device known to us in which they are seriously interested, the major source of energy release is ordinary fissionable material."[67]

From Bradbury's perspective, Livermore had wasted scarce fissionable material on a pointless experiment based on an uninteresting principle previously discarded by Los Alamos. He questioned Livermore's expenditure of fission fuel given that the information might have been gained through calculations. During a period in which fissionable material was still relatively scarce—the expansion of production approved by the president had not yet increased availability of material—this was a serious charge.[68] Livermore's tests had thus been wasteful, duplicative, and irrelevant. Rounding out his critique of the new laboratory Bradbury touched on another AEC concern: Los Alamos' ability to meet military requirements. Bradbury charged that Livermore's proposed Castle tests were likely to interfere with Los Alamos' success in providing large yield emergency capability weapons in the shortest possible time.[69]

There is little evidence the AEC heeded Bradbury's warnings. The commissioners did not reduce their commitment to Livermore, although they did question closely Livermore ambitions to expand with every new step proposed. Growing military demand for nuclear weapons thus made it easier for the AEC to approve Livermore expansions and ignore Los Alamos complaints. In their effort to inhibit the development of a potential rival, Los Alamos leaders continued to argue the new laboratory was redundant and unnecessary. In any case, most of the AEC commissioners did not consider Livermore to be the second laboratory, which they continued to believe would be detrimental to Los Alamos. AEC concerns and DOD interests thus gave form to the early two-laboratory system, as we have seen in this chapter. Livermore and Los Alamos were expected to play different roles and perform different functions, a consequence of the AEC's desire to ensure the success of the Los Alamos program while Livermore explored new ideas. The AEC thus shielded the Los Alamos H-bomb program from failure, less concerned as it was about possible Livermore failure. Los Alamos had its share of test fizzles, but these were obscured in the laboratory's many tests. There is little evidence the AEC was overly concerned by Livermore's 1953 test fizzles. These confirmed Livermore's role as the "new ideas" laboratory, helping the AEC fulfill its commitment to the Department of Defense.

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67. Bradbury to Tyler, "Future Full-Scale Weapons Tests," 6 May 1953. Despite their designation as thermonuclear weapons, both Livermore and Los Alamos H-bombs nominally derive 50 percent of their yield from fission.

68. Bradbury to Tyler, "Future Full-Scale Weapons Tests," 6 May 1953.

69. Ibid.

4. SMALL IS BEAUTIFUL
Livermore, The Army, and Atomic Artillery
1953-1954

An organization's domain comprises its activities, roles, and functions. Determining the nature and scope of an organization's domain is a critical concern of its leaders. Domain definition is a negotiated process among the members of the organization's organizational set, that group of organizations with which it interacts, on which it depends for resources, and whose acquiescence can confer legitimacy.[1] The greater the consensus regarding the organization's domain among the members of its organization set, the easier it will be to perform its functions, and the greater its chances of survival and growth. Domain definition is not concerned exclusively with determining the general area of activity in which an organization will be involved. Also at stake is the question of the particular roles and functions it performs. As W. Richard Scott argues, a manufacturer will not simply decide to produce toys, for example, but must determine the kinds of toys and who will be the customers.[2]

Los Alamos had exploded the world's first large scale thermonuclear device less than two months after Livermore opened its gates, and was on course to develop the large H-bombs the Air Force desired. Tests of thermonuclear devices of its own design were still just under two

1. See discussion of organization set in W. Richard Scott, *Organizations: Rational, Natural, and Open Systems*, 3rd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1991), pp. 126-127.

2. Ibid., p. 193.

years away when Livermore was founded. Meanwhile, the newly minted Ph.D.'s from Berkeley working in former navy barracks had ambitions to occupy themselves with more than diagnostic work for Los Alamos. Laboratory leaders confronted problems faced by all organizations in establishing themselves: acquiring resources and dealing with the expectations of external actors. The small group in Livermore would have to proceed under constraints resulting from AEC concerns outlined in the previous chapter. Building Livermore into a full scale nuclear research, test, and development laboratory would require facilities, personnel, and resources. Laboratory leaders needed to create a weapon research and development program that would be scientifically challenging, responsive to AEC concerns, and, most critically, attract the support and interest of military sponsors. Livermore was effectively barred from pursuing lines of research already being investigated by Los Alamos. The AEC was reluctant to provide the laboratory with a specific charter, although it was clear that Livermore was supposed to be a place where "new ideas" would be explored. What "toys" would the laboratory develop, and who would be its "customers"? This chapter traces Livermore after its 1953 test fizzles and the efforts of laboratory leaders to expand the laboratory's domain beyond weapons research into weapons development.

Tactical Atomic Weapons Revisited

Lewis L. Strauss, a supporter of the second laboratory, replaced Gordon Dean as Chairman of the AEC on 2 July 1953.[3] Teller wrote Strauss in July 1953, a little more than three months after Livermore's two tests fizzles, expressing his frank concerns about laboratory morale. Teller was thinking ahead to the laboratory's first thermonuclear tests scheduled for the spring 1954 Castle series. He was confident of their success although all were "worried . . . about the question of what would happen if [the Castle devices] did not function as expected." Teller underscored Livermore's role as risk taker and hoped the laboratory would be "encouraged to proceed even in case our first gamble is not fully successful."

Teller told Strauss that in addition to its "high priority" thermonuclear research a small fraction of Livermore's work was devoted to small fission weapon research. Teller had recently become interested in small weapons research, especially for their possible application in both artillery and anti-aircraft weapons. His "personal feeling" was that the small weapons program was an important one. It was a fascinating puzzle. As he told Strauss, "the field of really small weapons (between 8" and 12" diameters) has received little attention, the problem is difficult, and there seems to me to be quite a bit of room for invention in this field." Teller minimized the financial implications of his suggestion that the laboratory expand its work in this area. He hoped

3. Richard G. Hewlett and Jack M. Holl, *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission*, vol. 3 of "A History of the United States Atomic Energy Commission" (Berkeley: Univ. of California Press, 1989), p. 569.

additional tests could be performed "without undue fanfare, [and] with little organizational expense."[4]

How did Teller come to promote this new role for the laboratory? Perhaps he advocated work on small diameter atomic weapons out of "personal" interest. Teller, however, had a remarkable ability to find convergence between his own interests and those of potential sponsors. Developments outside the laboratory pointed to growing, though contested, support for small atomic weapons for tactical use, particularly in the Army.

Proponents of high yield nuclear weapons for strategic bombing still dominated the Air Force in the early 1950s, although the development of small fission weapons for air defense was gaining support. The Air Force favored strategic weapons, to the exclusion of tactical weapons, especially if these were intended for Army or Navy use. Since these could not be excluded, the Air Force hoped to limit their number through the allocation of fissionable material to keep the stockpile "overwhelmingly strategic."[5] The Air Force Chief of Staff thus argued atomic artillery, should have a "secondary place" in the stockpile.[6]

Los Alamos weapon development program priorities were aligned with military priorities as transmitted by the Joint Chiefs of Staff through the AEC. The laboratory thus placed high priority on Air Force requirements. The Air Force could not suppress completely Army claims, however, and Los Alamos had begun work on the Army's first nuclear artillery shell around 1950. Atomic artillery was attractive to the Army as it offered greater reliability and accuracy than did rockets; besides, the Army knew a lot more about artillery than rockets.[7] At 280 millimeters, the Los Alamos-designed atomic shell had the smallest diameter of any atomic weapon then developed. Los Alamos almost immediately began working on an improved version after the first was fielded in 1952.[8] The Army was most interested in an even smaller nuclear warhead for its 8-inch howitzers. There were fewer than one-hundred 280-millimeter guns, but as many

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4. Edward Teller to Lewis L. Strauss, "Present Status at Livermore," enclosure to 13 July 1953 letter.

5. Lee Bowen, *The Development of Weapons*, vol. 4 of "A History of the Air Force Atomic Energy Program, 1943-1953, in Five Volumes" (AFHD, n.d., c. 1959), p. 374.

6. Ibid., p. 432.

7. For the Army's early postwar efforts to gain an atomic weapons role, see John J. Midgley, Jr., *Deadly Illusions: Army Policy for the Nuclear Battlefield* (Boulder: Westview Press, 1986), especially pp. 5-9; on the Army's buildup of tactical atomic weapons in the 1950s, pp. 31-85. See Barton J. Bernstein, "Eclipsed by Hiroshima and Nagasaki: Early Thinking About Tactical Nuclear Weapons," *IS* 15 (Spring 1991), for the earliest military thinking about tactical nuclear war and tactical nuclear weapons.

8. Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), p. 106.

as sixteen hundred 8-inch howitzers. An 8-inch atomic shell would thus give the Army "a formidable atomic capability," as one Army spokesman observed.[9]

The 1952 Project Vista report had given supporters of tactical atomic weapons a boost by providing a military rationale for the role of tactical weapons in the defense of Europe. The report also downplayed the contribution of strategic bombing, much to the consternation of the Air Force.[10] Intensified interservice conflict resulted, as the Army and Navy pressed for the development of tactical atomic weapons. An important argument against tactical atomic weapons was that they utilized nuclear fuel inefficiently, as discussed at greater length below. Inefficiency mattered most for applications involving large numbers, like air defense and atomic artillery. By 1953, however, Los Alamos had made substantial progress in improving the efficiency of tactical weapons, reducing their size while maintaining comparable yields. These relatively small weapons were intended largely for tactical use, and would fit into most existing and planned Air Force fighter bombers and Navy attack planes. They also had Army applications: the Honest John surface-to-surface guided missiles, shells for long-range artillery, and atomic demolition land mines.[11]

Despite improvements in small fission weapon design, differences among the services over allocation of nuclear material became even more pronounced with the advent of thermonuclear weapons. The division of resources and nuclear material production between fission weapon and thermonuclear weapon developments became the subject of heated debate.[12] This controversial topic had been central to the debate over the founding of Livermore. It was also to play a role in shaping Livermore's nuclear weapon development program.

The Simon Committee

In March 1953 the Joint Chiefs of Staff asked the AEC to develop an 8-inch atomic shell though they recognized that an efficient, low-cost weapon was not yet feasible.[13] The AEC de

9. Leslie E. Simon to ATSD(AE), "The 8-inch Atomic Artillery Shell," 25 August 1953.

10. See David C. Eliot, "Project Vista and Nuclear Weapons in Europe," *IS* 11 (Summer 1986): 163-183.

11. The 10,000-pound implosion fission bombs of the 1945-1949 period were being replaced by the 3,000-pound Mark 5 and the 2,700-pound Mark 7. The 1,000-pound Mark 12 bombs would become available in 1954. See David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy, 1945-1960," *IS* 7 (Spring 1983): 113-181, at 140.

12. Bowen, *The Development of Weapons*, pp. 472-473.

13. Robert LeBaron to Gordon E. Dean, 14 April 1953; LeBaron to Chairman JCS, 9 June 1953.

layed action, concerned the shell was too costly in nuclear material.[14] The Military Liaison Committee established a joint service group to study issues surrounding the development of tactical nuclear weapons. Commonly referred to as the Simon committee after its chairman Leslie E. Simon, Maj. Gen., USA, the joint committee was chartered to examine possibilities for small diameter atomic weapons. The ultimate goal was to provide relatively efficient, low-cost weapons suitable for air defense, close support, and other tactical roles.[15] By mid-May the AEC had agreed to participate. The outcome of the Simon Committee deliberations could help the AEC determine weapons development priorities.[16]

By early July 1953, Los Alamos had completed a feasibility study of small diameter weapons, just in time for the Simon Committee's consideration. The study included warheads with diameters of up to seventeen inches, suitable for air-to-air missiles, but focused primarily on diameters below twelve inches, more typical of artillery systems. The tone of Bradbury's letter to the AEC reflected his views on the 8-inch shell. He nevertheless left it for the Commission and the DOD to decide

> whether the possible operational advantages of a weapon of this sort justify proceeding with its development despite its very high cost per kiloton. If an actual development program is authorized . . . the amount of effort which will be required from . . . LASL . . . and the DOD will be of the same order of magnitude as that required for the development of a new Mark number free fall bomb.[17]

The Simon committee held its first meeting on 20 July 1953. The Army wanted small diameter atomic warheads to substitute for conventional in some of its weapon systems. The 280-mm gun firing the Mark 9 nuclear shell was the Army's only atomic weapon system, although the Honest John rocket and Corporal guided missile systems, both using the W-7 warhead, were soon to come. The Army considered most atomic weapons developed so far of little use for its purposes. The emphasis on nuclear efficiency resulted in the development of warheads too large for Army application. Navy delivery systems offered more flexibility. Its guided missiles and bomber aircraft could use relatively inexpensive, large diameter implosion weapons. Only if the nuclear efficiency of small weapons improved enough to compare favorably with the larger

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14. Dean to LeBaron, 15 May 1952.

15. The DOD representatives were Maj. Gen. L. E. Simon, USA (Ord.). Brig. Gen. K. F. Hertford, USA (MLC), Capt. J. M. P. Wright, USN (BuOrd), Col. Taylor Drysdale, USAF (AFOAT). Capt. R. S. Riddell, USN, was the AFSWP observer. See LeBaron to Chairman JCS, 9 June 1953. Gordon Dean named Col. Vincent G. Huston as AEC representative to the group. See Dean to LeBaron, 15 May 1952.

16. Dean to LeBaron, 15 May 1952.

17. Norris E. Bradbury to Kenneth E. Fields, 9 July 1953.

weapons would the Navy establish requirements for warheads for ship guns, torpedoes, and other small tactical weapons. The Air Force staunchly opposed small atomic weapons as impractical and unnecessary, although a pending study of air defense needs might alter that position.[18]

Strauss' interest in atomic artillery was piqued by Teller's inquiry about the possibility of expanding the Livermore program into this area.[19] At a regularly scheduled joint AEC-MLC meeting in July, Strauss asked MLC representatives if they believed the AEC's small weapons endeavor worthwhile. The MLC agreed the program had merit, but would not endorse small weapons at the expense of large. The MLC could provide no firm guidance because the services had not yet come to consensus on weapon development priorities.[20] The Simon Committee, headed by an Army general, had no such compunctions, and recommended development of an atomic shell for the Army's 8-inch howitzer.[21] Simon explained to the Assistant to the Secretary of Defense for Atomic Energy that development of the shell would create "a formidable atomic capability" given the Army's large store of 8-inch howitzers. He refuted claims the shell was an extravagant expense: its 2.5 million dollar development cost and manufacture of non-nuclear parts was extremely modest. A new weapon would impose far greater costs for procurement, training, and manpower. The 8-inch atomic howitzer, Simon argued, was "probably the most economic atomic energy measure ever proposed."[22] JCS Chairman Arthur Radford agreed.[23] So did the Secretary of Defense.[24] The overall efficiency of atomic artillery was high, especially compared with conventional artillery.

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18. Joint AEC-DOD Technical Survey Group, "Record of Meeting 20 July 1953," 20 July 1953.

19. Lewis L. Strauss to Edward Teller, 20 July 1953.

20. Minutes of 83rd AEC-MLC Conference, 23 July 1953.

21. G. H. Drewry, Jr., "Joint AEC-DOD Technical Survey Group on Small Diameter Atomic Weapons Record of Meeting 19 August 1953," 27 August 1953; Leslie E. Simon to MLC and AEC, "Recommendations of the Joint AEC-DOD Technical Survey Group on Small Diameter Atomic Weapons, proposed 19 August 1953," 19 August 1953.

22. Simon to ATSD(AE), "The 8-inch Atomic Artillery Shell," 25 August 1953.

23. Arthur Radford to Secretary of Defense, 27 August 1953.

24. On the Secretary of Defense's support, see Robert LeBaron to Leslie E. Simon, 14 August 1953; and LeBaron to Secretary of Defense, "8-inch Atomic Artillery Shell Development," 5 Oct. 1953, attached chronology.

Livermore's Small Weapons Program

At first, it had appeared possible, even necessary, to build the Livermore laboratory program on the strength of "new ideas." The spring 1953 tests had been justified on this basis. As noted in the pre-test report, the immediate aim of these tests was not to develop a stockpile weapon, but to test the properties of the nuclear material in the device for possible future use.[25] Similarly, Livermore's planned H-bomb tests were "not directed toward early design of a deliverable weapon."[26] Livermore, however, was unlikely to grow and expand into an independent full scale laboratory if confined to supporting Los Alamos. Bradbury's questions following Livermore's spring test fizzles were indicative of the laboratory's vulnerability as long as its programs did not seem to meet some direct and useful purpose. How would Livermore justify its existence, on what basis would resources be obtained, and whom would the laboratory serve? Army interest in atomic artillery provided Livermore the opening it needed.

Livermore scientists and leaders learned about Army interest in atomic shells from a variety of sources. John von Neumann, for example, who chaired the GAC Subcommittee on Weapons, was an important link between the MLC, the Simon Committee, and Livermore.[27] The GAC visited Livermore a few weeks after its May 1953 meeting when their possible development was discussed with military representatives.[28] Knowledge of military interest in small diameter weapons and the ongoing Simon Committee discussions might also have reached Livermore via Admiral Russell of the MLC who travelled to the AEC weapons laboratories in summer 1953.[29]

The Simon Committee members' interest was piqued by Livermore's investigation of "novel" small weapon designs.[30] Two days following its July meeting York asked the Armed Forces Special Weapons Project (AFSWP) in Albuquerque for information about allowable weights and dimensions for atomic projectiles between 8 and 11 inches, the calibers of Army atomic artille-

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25. James E. Carothers, Wallace D. Decker, and W. J. (Jim) Frank, "Thirty Years Ago: The [Basilisk Designs]," *Research Monthly* [LLNL] (March 1982): 27.

26. AEC, Note by the Secretary, "UCRL Testing Program," 30 Dec. 1952.

27. Von Neumann received the Simon Committee recommendations on the same day as the MLC chairman; Mark H. Terrel, "Simon Ad Hoc Group Meeting 19 August 1953," 20 August 1953. Von Neumann's contribution to Livermore's earliest investigations in the small weapons field is acknowledged in John S. Foster, Livermore Report No. UCRL-4771 NA145730, 20 Nov. 1956, pp. 27, 30.

28. Minutes of 35th Meeting of the GAC to the AEC, Washington, D.C., 14-16 May 1953. See pp. 22-25 for GAC discussions with military representatives. The GAC visited AEC research centers, including Los Alamos and Livermore, between 28 May and 10 June 1953. See Minutes of 36th GAC Meeting, 17-19 August, 1953, p. 1.

29. Minutes of 83rd AEC-MLC Conference, 23 July 1953.

30. Drewry, "Record of Meeting," 20 July 1953.

ry.[31] The Simon Committee also submitted a formal request to the AEC to investigate efficient low cost weapons for air defense, close support, and other tactical roles.[32] Around this same time, Livermore asked AEC approval for a site that would enable laboratory scientists to conduct high explosive tests.[33] Small weapons development meant large numbers of tests, though not all nuclear. A significant fraction would involve tests of the non-nuclear high explosive required to study the mechanism for imploding the nuclear material in an atomic bomb. Without a dedicated site for doing so, Livermore would be hard-pressed to develop small atomic weapons.[34] York also submitted a formal proposal for Livermore's expansion into small fission weapons in late August 1953. Fission weapons research had not been excluded from Livermore's initial program, but neither had it been explicitly included. The two principal goals of the program outlined by York would be the development of small, light-weight nuclear warheads for air-to-air defense missiles and improved atomic artillery shells. The design objective in each case were to develop reasonably efficient fission weapons of relatively small size, weight, and yield.[35]

The proposed development of a warhead for air-to-air missiles grew out of informal discussions with Air Force representatives. No formal requirement for atomic air-to-air missiles yet existed, although Air Force policy was under review. Air Force proponents of atomic air-to-air missiles saw them as a potentially cheaper means of destroying enemy bombers than conventional weapons, provided better designs could raise nuclear efficiency, lowering costs. As for atomic artillery, Livermore had been "informed" of the requirements for such weapons. Improved efficiency ought to result in shells of smaller diameter and lower weight. Yet another reason for investigating small diameter fission weapons was the development of small primaries. The purpose of the primary is to initiate the thermonuclear reaction which creates the nuclear explosion. The small thermonuclear weapons in which the military was becoming interested would require smaller primaries than then available.[36]

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31. See R. M. Blanchard, Jr., to LASL Director, attn: Donald P. MacMillan, 22 July 1953, for reference to Livermore request and about forwarding same information to Los Alamos.

32. Leslie E. Simon to MLC and AEC, "Recommendations of the Joint AEC-DOD Technical Survey Group on Small Diameter Atomic Weapons," 27 August 1953; Terrel, "Simon Ad Hoc Group Meeting 19 August 1953," 20 August 1953.

33. See Minutes of PW Administrative Meeting, 28 July 1953, for reference to Livermore request.

34. See LLNL, "Livermore Capability in High Explosive Technology and Related Disciplines," UCRL-TB 108623, n.d., for high explosive testing.

35. Herbert F. York, with Arthur Biehl, John S. Foster, and Edward Teller, to Kenneth E. Fields, "Interim Report, Small Weapons Program," 24 August 1953.

36. Ibid.

Livermore needed an active development program in order to justify its expansion. Atomic artillery provided the rationale. In contrast, Los Alamos was eager to rid itself of its responsibilities in this area. Before considering the reasons why, I briefly discuss atomic weapons design, which will help illuminate the different laboratory perspectives.

One nuclear bomb is not like another. There were two principal means of assembling the critical mass for a nuclear explosion. The first, the implosion method, descended from the wartime Fat Man design. In this approach, a sphere of conventional explosive compresses a spherical ball or hollow shell of nuclear fuel, initiating the fission reaction that results in the nuclear explosion. The second approach descends from the wartime Little Boy. Generally referred to as the gun-type design or gun method of assembly, it brings two or more subcritical pieces of nuclear material together to form a supercritical mass, usually in an elongated tube, by, for example, accelerating one piece into another, or the pieces into each other.[37] Implosion assembly was faster than gun assembly, making implosion a more efficient approach to burning nuclear fuel.[38] Implosion and gun-type designs were made in various sizes, weights, and yields, although larger weapons were usually of the implosion type. In the early 1950s, the Air Force preferred the biggest, heaviest, highest yield weapons its strategic bombers could carry. Accordingly, Los Alamos saw its task as developing the largest fission implosion weapons of highest yield possible. The largest fission weapons developed were based on King, a 500- kiloton device tested in the Ivy nuclear test series in 1952.

Artillery represented the other end of the size and yield spectrum from Air Force bombs and thus posed different physical constraints on the nuclear warheads they carried. While Air Force bombs were generally of the implosion type, gun assembled atomic warheads were best suited for deployment with artillery. Their design geometry permitted the development of relatively long and small diameter weapons. Available propellants, however, could not assemble the nuclear pieces of the gun assembled weapons rapidly enough to burn the nuclear fuel completely. This less-than-complete burning of the nuclear fuel made atomic shells relatively inefficient in comparison with implosion weapons. The inefficiency of gun-assembled designs shaded into near impossibility when plutonium was considered as a potential fuel. Fission might begin even before assembly was complete, the reason Los Alamos dropped plans to use plutonium in gun-type weapons in 1944.[39]

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37. Robert W. Selden, "An Introduction to Fission Explosives," Report UCID-15554, Livermore, July 1969, pp. 8-10, Figures 4-6; Samuel Glasstone and Philip J. Dolan, eds., *The Effects of Nuclear Weapons* (Washington, D.C.: DOD and ERDA, 1977), pp. 15-16.

38. David Hawkins, "Towards Trinity," in *Project Y: The Los Alamos Story*, vol. 2 of "History of Modern Physics, 1800-1950" (Los Angeles and San Francisco: Tomash, 1983), p. 68.

39. Ibid., pp. 96, 117, 161, 247.

While Livermore sought to enter the small diameter atomic warhead field, Los Alamos was seeking to limit its own work in this area. Los Alamos leaders viewed it as a diversion from the laboratory's main task. Laboratory weapons designers deemed atomic artillery design a dead end technically, neither challenging nor rewarding, a view shared by the laboratory's principal military customer the Air Force. The laboratory preferred focusing its investigation on implosion systems, which promised greater advancements, in nuclear efficiency and in other ways, and were better suited to the large size and yield weapons carried on aircraft.

Because Army work had lower priority (although this would soon change), it also seemed less interesting. Los Alamos director Norris Bradbury described the task of improving atomic artillery as "straightforward," and "not particularly exciting." Los Alamos had "many more important devices to occupy [its] . . . attention."[40] An AEC memo described the position of Los Alamos on gun-assembled devices: "improvements that might be accomplished . . . [did] not justify the expenditure of developmental effort" by the laboratory.[41] Livermore had no such compunctions. Without military customers, and essentially barred from treading on Los Alamos turf, Livermore aggressively pursued Army work.

Livermore's main thrust in this area was a hybrid of previous fission weapon designs which for classification purposes will be referred to in this study as the "Manticore" nuclear design. Like their mythical counterparts, Manticores combined disparate elements of previously developed systems to create something new.[42] They promised faster assembly, use of plutonium, improved efficiency, and increased yields over systems considered by Los Alamos, which had never been tested. The laboratory's Manticore investigations offered the perfect solution to AEC strictures and constraints. No laboratory duplication was involved since the technique was not under active investigation at Los Alamos. Its intended application and customer—atomic artillery for the Army—were low priority for Los Alamos. By exploring the Manticore systems Livermore would thus not be competing head-to-head with Los Alamos for military customers.

The Commission approved Livermore's proposal to investigate small size low-yield fission weapons at its 9 September 1953 meeting. Some commissioners felt such a statement necessary for AEC presentations to the Bureau of the Budget for fiscal year 1955 because the laboratory

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40. Norris E. Bradbury to Kenneth E. Fields, 22 Sept. 1954.

41. Vincent G. Huston to Donald J. Leehey, "Feasibility Study for Improvement of the 8" Shell," 1 June 1955.

42. Bowen, *The Development of Weapons*, pp. 485-486. See also Herbert F. York to Kenneth E. Fields, "Interim Report, Small Weapons Program," 24 August 1953; JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, pp. 44-49.

had until then largely been occupied with the development of thermonuclear weapons.[43] The AEC was unable to provide Livermore guidance on the range of yields to be investigated, especially minimum yields, since the military had not yet established formal requirements. There was no officially-stated military interest in very low yields (below 1/2 kt), but the AEC expected the Department of Defense to decide on the merits of such weapons within six months when Livermore should be prepared to report on the feasibility of small diameter and yield atomic weapons.[44]

The broadened scope of the Livermore program would increase Livermore's participation in the over-all weapons research and development program. The AEC had requested a total of $20 million for fiscal year 1955 operating costs including $3 million for the small weapons program if developments warranted. Livermore would have to carry out the small weapons program within its current personnel ceiling of 1600. Nor was the Commission yet ready to approve the acquisition of a Livermore site for hydrodynamic testing, asking for a feasibility report before any off-site experimental programs began.[45]

The Secretary of Defense supported development of the 8-inch shell, urging the MLC not to delay.[46] But when the JCS Joint Strategic Plans Committee submitted its nuclear weapons development requirements to the AEC, "serious divergencies [sic] of opinion developed." The three services differed about the proper mix of tactical and strategic weapons. Especially in contention was the development of atomic artillery. The Air Force wanted to structure the stockpile to meet its strategic air offensive objectives while the Army and Navy argued for the greatest possible number of weapons. Specifically at issue was the continued stockpiling of Army and Navy Mark 8 and 9 weapons. The broader question involved allocation of development and production resources between thermonuclear and fission weapons.[47] Atomic artillery development "continue[d] to drag," MLC Chairman LeBaron told the Secretary of Defense in early October 1953.[48] As late as October 1953 the MLC could still not provide a unified DOD position on the development of low yield fission weapons. LeBaron could provide the AEC no "simple state-

43. AEC Meeting No. 912, 9 Sept. 1953, pp. 597-598.

44. Vincent G. Huston to Herbert F. York, thru John J. Flaherty, 9 Sept. 1953.

45. Ibid.

46. LeBaron to Secretary of Defense, "8-inch Atomic Artillery Shell Development," 5 Oct. 1953. See attached chronology.

47. Bowen, *The Development of Weapons*, pp. 436-439, 451-452.

48. LeBaron to Secretary of Defense, "8-inch Atomic Artillery," 5 Oct. 1953.

ment" about priorities. Any effort to converge all the "energetic" activities of the services (i.e. interservice rivalries) was bound to produce "a picture with elements of controversy."[49] These unclear signals continued to make the AEC reluctant to forge ahead on the 8-inch shell. In order to solidify support, LeBaron sent AEC Chairman Strauss a copy of the JCS August 1953 memo endorsing atomic artillery development.[50] Finally, in late October, the Army requested formal DOD authorization for development of the shell.[51] The Air Force objected, charging it would cost more fissionable material per unit yield relative to larger diameter weapons and asked the JCS to reconsider its earlier support.[52] The MLC sent a split opinion to the Assistant to the Secretary of Defense for Atomic Energy (ATSD(AE)).[53] But the Under Secretary of the Army asked for and got from DOD $1 million for the program.[54] The Army was on its way.

To become a full scale weapons design, test, and development laboratory, Livermore would need facilities and specific weapon development responsibilities. York had thus renewed his effort to obtain approval of a convenient site for doing hydrodynamic experiments.[55] Without it York warned researchers might be inclined to skip such tests.[56] Livermore scientists and engineers were gaining experience conducting high explosive tests in the course of their work with the Los Alamos GMX (high explosives) division. And Los Alamos had promised to fabricate high explosive parts for Livermore and would perform about 30 test shots through summer 1954.[57] Sentiment was strong at Livermore that it needed its own site. The Director of Military Application informed York in February 1954 the AEC was supportive of Livermore's acquisition of a high explosive test site. In the meantime the laboratory should accept Bradbury's offer to

49. Robert LeBaron to AEC Chairman, 14 Oct. 1953.

50. Other sizes had also been discussed, but the MLC settled on the 8-inch shell; Robert LeBaron to Lewis L. Strauss, 7 Oct. 1953.

51. Robert LeBaron to Lewis L. Strauss, 29 Oct. 1953, Earl D. Johnson to Secretary of Defense, 21 Oct. 1953, attached.

52. USAF to MLC, "Simon Committee Recommendations 8" U-235 Shell," proposed for entry in MLC minutes, n.d. (c. Oct. 1953).

53. Mark H. Terrel to Chairman MLC, 15 Jan. 1954.

54. Mark H. Terrel to Chairman MLC, 1 Jan. 1954.

55. Herbert F. York to Kenneth E. Fields, "UCRL High Explosive Support Requirements," 11 March 1954.

56. Minutes of PW Technical Program, 5 May 1954.

57. Minutes of PW Technical Program Meeting, 17 Feb. 1954.

use the "R" test site at Los Alamos, and the "S" site for fabrication of high explosive charges.[58] York and his Livermore colleagues were concerned the offer might impede the speedy acquisition and approval of its new site, rendering Livermore arguments for a high explosive test facility "less effective."[59] Their fears were unfounded. Selecting the site and getting it took only another year and a half.

Army Patronage

Although Army interest had sparked Livermore's small weapons program, the laboratory still had a long way to go. The AEC's approval of the Livermore small fission weapon program did not include a specific weapons development assignment and both laboratories continued to study ways to improve the efficiency of small-diameter weapons.[60] Given the role of the Air Force and the H-bomb in the events that led to the founding of Livermore, the Army's role in Livermore's institutional development might on superficial consideration seem surprising. As we have seen, however, the Army-Livermore union grew directly out of the system of incentives and constraints created by the two-laboratory system. The Army and Livermore had much in common in the early- to mid-1950s as each sought to expand. Army interest in a larger nuclear role challenged Air Force dominance. And it was in the shadow of Los Alamos that Livermore sought to become a full scale nuclear research, test, and development laboratory. As interlopers of sorts, Livermore and the Army, together, might build what each would have had difficulty accomplishing independently.

To become a full-scale laboratory Livermore would need stockpile responsibilities. The challenge for Livermore leaders was to find ways to match scientifically interesting and challenging research and development programs with products potential military customers might want. Army interest got the laboratory started on a concrete program, helping Livermore create a niche for itself in the world of nuclear weapons development. Laboratory leaders were also careful to ensure the laboratory worked within the general guidelines established by the AEC even as they sought to expand the laboratory's program, as illustrated in this chapter. Its technical program was thus shaped by the incentive and constraint structure of the two-laboratory system.

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58. Kenneth E. Fields to Herbert F. York, 12 Feb. 1954. See LRL, *Status: Fiscal Year 1958*, "Status Report, July 1, 1957-June 30, 1958, of the Ernest O. Lawrence Radiation Laboratory," pp. 175-178, for a brief history of Livermore's Site 300. This report also provides informative descriptions of other Livermore functions, facilities, and personnel.

59. Minutes of PW Technical Program, 5 May 1954.

60. Lewis L. Strauss to MLC Chairman, 24 May 1954.

5. MUTUAL ACCOMMODATION
Foundation of the Two-Laboratory System
1954-1955

Livermore's success in achieving full-laboratory status would depend on the outlook of its sponsors, the Joint Committee on Atomic Energy, Atomic Energy Commission, and Department of Defense, as well as on the success of its technical program. Also important was how the laboratory managed its relationship to Los Alamos and how this relationship was perceived by sponsors. Los Alamos faced different issues. For the first time since its founding, its leaders felt compelled to justify laboratory programs and priorities in light of another's. This chapter examines laboratory strategies under these circumstances.

Livermore and the H-Bomb

Elected in November 1953, President Dwight D. Eisenhower's administration enunciated a new nuclear policy the following year. The New Look called for greater reliance on atomic weapons in national defense, emphasizing the development of H-bombs and fission weapons, while cutting back on manpower. The policy did not fundamentally alter the recommended mix of higher yield to lower yield nuclear weapons. The highest priority still remained the deployment of massive nuclear retaliatory striking power and emphasized the Air Force's strategic bombing role. The New Look did call, however, for the development of light weight nuclear missile warheads. Small fission weapons for tactical, close support, and air defense missions were to be phased in over time. The rate at which the latter occurred would depend on resources.[1]

Military priorities were paramount considerations in structuring the Livermore and Los Alamos nuclear weapons development programs. Laboratory directors learned details of how the New Look policy translated into military requirements and about its expected impact on the nuclear weapons program at the General Advisory Committee's January 1954 meeting. Los Alamos would continue to focus on urgent high priority developments, including the Air Force's high yield H-bombs.[2] Having tested the world's first thermonuclear device in 1952, the laboratory would use the Castle test series scheduled for the Pacific in spring 1954 to develop the first deliverable H-bombs, including the "emergency capability" bombs the Air Force wanted.

Livermore's mandate to avoid program duplication with Los Alamos and its role as the "new ideas" laboratory pointed to a different ordering of priorities which focused on longer-term and riskier weapons development projects, including "small" H-bombs. Interest in small H-bombs had preceded the new policy. York told the GAC in February 1953 that it was Livermore's "philosophy" to concentrate on small external diameter high energy weapons.[3] The laboratory's interest had less to do with philosophy, however, and everything to do with military interests. Military Liaison Committee Chairman Robert LeBaron had informed the AEC's Gordon Dean in April 1953 that the development of "small" high yield H-bombs—one quarter to one or two megatons for delivery by high performance fighter-bomber, pilotless aircraft, and guided missiles —was a "major objective" of the Department of Defense.[4] The laboratory's were well aware of DOD interests, and LeBaron's communication to the AEC would have been preceded by months, if not years of deliberation and included informal discussions with the laboratories.

The Castle test series would offer Livermore its first opportunity to test a thermonuclear device. York told a gathering of laboratory staff in January 1954 that since the Los Alamos high yield H-bomb program was in good shape Livermore would investigate smaller size and yield H-bombs.[5] In the planning stages since summer 1952, the devices tested in the Koon and Echo events would not be weapon prototypes but would investigate certain design features of interest.

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1. For a discussion of the New Look from the perspective of the GAC, see Minutes of 38th GAC Meeting, 6-8 Jan. 1954, Washington, D.C., pp. 2, 5, 23-25, 31-33, 40; I. I. Rabi to Lewis L. Strauss, 9 Jan. 1954. More generally, see John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy* (Oxford: Oxford Univ. Press, 1982), chaps. 5-6.

2. Minutes of 38th GAC Meeting, pp. 23-25.

3. Ibid., p. 13.

4. Robert LeBaron to Gordon E. Dean, 23 April 1953.

5. Minutes of PW Technical Program Meeting, 27 Jan. 1954, p. 4.

On the assumption of success in Castle, however, Livermore scientists had begun investigating modified versions of Koon and Echo devices by summer 1953. Their narrow diameters would be suited for delivery by fighter bomber or missile, although, as we shall see, they were never tested or fielded.[6] The smallest would have had a yield of about one megaton.[7] The larger, yields in the range of three to five megatons.[8] Despite differences in yields, the Livermore designs were fundamentally similar, although it was not a simple matter to modify important design parameters.

The AEC approved Livermore's work on the larger of its two thermonuclear devices in February 1954. Requirements for a weapon of this type would be large. The Air Force wanted weapons in this size and weight range for its new B-47 jet bomber. AEC Director of Military Application Kenneth E. Fields urged the laboratory to investigate its compatibility with this and other suitable aircraft. For its part, Los Alamos hoped to put the B-47 "in business" with its Castle-Bravo device to be tested in 1954. B-47s were more numerous than the B-36s, the only aircraft that could then carry large H-bombs intercontinentally. H-bombs capable of carriage on the B-47 were thus an attractive proposition for the Air Force.[9]

Livermore ambitions for competing with Los Alamos to develop the nuclear bombs for the B-47 aircraft were evident at the laboratory's February 1954 technical steering group meeting. Their first thermonuclear tests less than two months away, Livermore scientists were full of hope and expectation. Based on design features particular to the Livermore design, they believed their device would have a "slight inside track" over the Los Alamos counterpart, assuming the Livermore Koon and Echo shots went well.[10] A careful comparison of the merits of the laboratories' two designs would be made by the AEC and the Air Force after the Castle tests, and Livermore hoped its design would be selected for development.

Government studies on the merits and feasibility of strategic ballistic missiles motivated Livermore's investigation of H-bomb designs in the one megaton range. The physical constraints

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6. Minutes of PW Technical Program Meeting, 21 August 1953. The new designs had no names at this early date. See discussion in Minutes of PW Technical Program Meeting, 11 Sept. 1953; 25 Nov. 1953; and 17 Feb. 1954. Herbert York to Kenneth E. Fields, 21 Sept. 1953.

7. Herbert F. York with Harold Brown, to Kenneth E. Fields, 27 Nov. 1953, attachment, n.d.

8. Minutes of PW Technical Program Meeting, 4 Nov. 1953.

9. Minutes of PW Technical Program Meeting, 17 Feb. 1954. For a discussion of the thermonuclear weapons considered for possible delivery by the B-47, see Lee Bowen, *The Development of Weapons*, vol. 4 of "A History of the Air Force Atomic Energy Program, 1943-1953 in Five Volumes" (AFHD, n.d.), p. 462.

10. Minutes of PW Technical Program Meeting, 17 Feb. 1954.

of missiles—more stringent than the Air Force's strategic aircraft—pointed to the development of such small size and weight warheads.[11] No delivery vehicle was known in February 1954 to "need" Livermore's one megaton device, the smallest yield H-bomb then under active study, but such delivery systems were believed to be "coming along." Growing interest in ballistic missiles thus led York to conclude it was a "good idea to cut the weight [of H-bombs] way down," and the AEC agreed.[12] Los Alamos was also investigating a small thermonuclear device, eventually tested in the Castle Nectar event. Although not as ambitious in terms of size and weight as Livermore's, the two designs would compete for this smallest H-bomb class.[13]

Los Alamos tested a broad range of H-bomb in Castle.[14] The Castle-Bravo device yielded 15 megatons, roughly three times the predicted yield. A variation tested in the Romeo shot gave 11 megatons, while the device tested in the Union event came in at about 7.[15] The Castle-Bravo device was particularly significant because it demonstrated the feasibility of lithium deuteride, a dry fuel. Prior to Castle, "all bets" had been on cryogenic, or liquid, systems. These would have involved complex logistic problems associated with keeping deuterium at the cold temperatures required to maintain it in liquid form. As Bradbury told the JCAE, however, the Castle-Bravo shot "x-ed out" liquid-fueled systems, "chang[ing] the whole concept of weapon development from that point on."[16]

York thought Livermore's first H-bomb test, whose expected yield was about one megaton, would "look a little small" sandwiched in among the high yield Los Alamos tests.[17] The outcome was worse than York expected. Livermore's Koon event on 6 April 1954 was a fizzle. The device yielded only 100 kilotons.[18] Livermore's Deputy Director Duane Sewell later told a Liver-

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11. For an interesting history and discussion of the development of ballistic missiles, including the various study groups, see Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York: Columbia Univ. Press, 1976).

12. Minutes of PW Technical Program Meeting, 17 Feb. 1954.

13. Kenneth E. Fields to Herbert F. York, 12 Feb. 1954.

14. Minutes of 40th GAC Meeting, 27-29 May 1954, Washington, D.C., pp. 17, 28, and 32.

15. Initial estimates put the yield to 12.8 MT. See Minutes of PW Technical Program Meeting, 10 March 1954. For yields of Castle tests, see NVO, *United States Nuclear Tests: July 1945 through September 1992*, Report DOE/NV-209 (Rev. 14) (Las Vegas, Nev., Dec. 1994), p. 3.

16. JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, p. 36.

17. Minutes of PW Technical Program Meeting, 10 March 1954.

18. Minutes of PW Technical Program Meeting, 5 May 1954.

more staff gathering it was pointless to fire the second device based as it was on the same design. The laboratory's second H-bomb test was thus cancelled.[19] Not conceptually unsound, the device's failure, as York later remarked, "was not . . . particularly educational. It had resulted from a simple design flaw."[20] It was a disappointment to the laboratory nevertheless.

The AEC informed York and Bradbury the Los Alamos Castle-Bravo design, not Livermore's, had been selected to meet the "exceedingly important and urgent requirements" for the B-47 aircraft.[21] Los Alamos' success thus led to cancellation of the competing Livermore H-bomb program in this yield and weight class.[22] Los Alamos' small thermonuclear device test (the Nectar shot) was also a success, achieving a yield of 1.69 megatons, within laboratory predictions. There was still time to investigate the feasibility of small H-bomb designs, however, since delivery systems were not yet available. The AEC therefore asked both laboratories to continue their small H-bomb programs. Livermore then set its sights on weapons for guided missiles and fighter-bombers.[23]

The ultimate aim of the nuclear weapons design, test, and development program was not to explore every conceivable weapons design, but rather to develop weapons for the stockpile. Although Livermore's disappointing Castle test was not caused by a fundamental design flaw, Los Alamos shots had gone well, Livermore's had not. Los Alamos' success had rendered Livermore's H-bomb design "unnecessary," at least in the B-47 class of weapons, as Bradbury later remarked.[24] This was not to say that Livermore's design might not ultimately have been made to work. But as one Livermore participant recalls, everybody just "lost interest" in it.[25] The press to develop weapons, including the "emergency capability" H-bombs the Air Force wanted in 1954, created momentum for choosing the Los Alamos design. Los Alamos H-bombs, ranging from the largest, in excess of 40,000 pounds and 25 megatons, down to 3,000 pounds and 1 megaton, were thus stockpile bound.

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19. Minutes of PW Administrative Meeting, 1 June 1954.

20. Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), p. 78.

21. Kenneth E. Fields to Donald J. Leehey, "Cancellation," 28 April 1954.

22. Minutes of PW Administrative Meeting, 1 June 1954.

23. Norris E. Bradbury to Kenneth E. Fields, 22 Sept. 1954.

24. Bradbury to Fields, 22 Sept. 1954.

25. Interview with W. J. (Jim) Frank, 13 Dec. 1991.

Livermore's H-bomb test fizzle prompted AEC questions about Livermore's origins, role, and functions. The AEC's General Counsel Office was asked about how the University of California Radiation Laboratory had become involved in the weapons program.[26] The GAC also discussed Livermore's problems, speculating that its difficulties might relate to the informal mandate for the laboratory to "do something more risky than Los Alamos." This left Livermore scientists in the frustrating position of not having "a real program of their own," as John von Neumann put it. And the chairman observed that Livermore did not have responsibility for any "necessary" part of the weapons program. Another committee member agreed the laboratory "lacked a clear job." Establishing a clearer division between the objectives and responsibilities of the two laboratories might help. So would getting a full-time director, on which the GAC urged the AEC to insist. Although the de facto director, York's role was not yet official.[27]

Discussion at an early May 1954 Livermore staff gathering reveals the laboratory's uneasiness. York explained to his colleagues in the technical program steering group that the laboratory program had been in a "state of flux."[28] Livermore faced difficult choices, given limited resources, between emphasizing basic research to advance fundamental knowledge related to weapons design, or getting weapons into the stockpile. The laboratory had no weapon development responsibilities, but hoped to gain some. Weapon development, within limits, could be pursued without deepening fundamental scientific knowledge. This could be done by testing a variety of designs and selecting those that worked without complete knowledge of the reasons for their success.

York attributed Livermore's difficulties, and its three-for-three test fizzles, to the rush to develop weapons for the stockpile. The laboratory had proceeded with "too much haste." The yield of the Livermore device was approximately an order of magnitude less than intended, the second time yield estimates had been off by significant amounts. Although the design was not conceptually flawed, York felt the problem could have been caused by "any one of many . . . small things." By working too fast, small things were being overlooked, jeopardizing the laboratory's overall success. And although the new laboratory had been founded on the expectation that fundamental research would be performed in connection with the weapons program, little such research had been done. Instead, most of Livermore's effort had been programmatic and toward the development of specific devices. Los Alamos' approach exemplified the benefits of research.

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26. See Chester G. Brinck to Leonard Jacobvitz, "Participation of the University of California Radiation Laboratory in Weapons Development Program," 29 April 1954, for reference to the initial request and the AEC General Counsel's perspective on how UCRL became involved in the weapons program.

27. Minutes of 41st GAC Meeting, 12-15 July 1954, Albuquerque and Los Alamos, pp. 55-56.

28. Minutes of PW Technical Program Meeting, 5 May 1954.

Its discoveries about basic characteristics of lithium explained why the Castle-Bravo shot "went so big" while Livermore had been diverted by the drive "get the current bomb made."[29]

Livermore needed a more balanced and deliberate program, and the technical steering group decided to put the laboratory on a "Research Program time scale."[30] The AEC agreed with this approach.[31] Within days, however, the best of intentions evaporated. The proposed test schedule for the 1955 Teapot series would strain the ability of the laboratory to do both fundamental research and field testing.[32] Ambitions for a more deliberate research program were thwarted by the press to develop weapons.

The Los Alamos Dilemma

Los Alamos faced different problems than did Livermore, responsible as it then was for all weapons development. Despite its substantial responsibilities, however, Los Alamos for the first time felt it necessary to explain and justify its resource allocation decisions in light of another laboratory's. Were Los Alamos' priorities in order? These issues were particularly salient as they pertained to small fission weapon work. As discussed in Chapter 4, small diameter fission weapons were costly in nuclear material and their applications low priority. Livermore's Manticore[33] work attracted the attention of Duncan P. MacDougall, leader of Los Alamos' high explosive division (GMX) where fission weapon work was centered. MacDougall described Livermore's Manticore designs as "ingenious (?)," while the question-mark following his comment indicates ambivalence about their value. He inquired of Theoretical Division leader Carson Mark in fall 1953 if they were of any interest. Should GMX be thinking about them, or were they "mostly nonsense?"[34]

MacDougall raised similar questions the following year. Although the New Look emphasized the development of nuclear weapons across a broad range of sizes and yields, high yield weapons remained the priority. Livermore's small fission weapon devices were most obviously applicable as atomic artillery. Weapons requiring large quantities of plutonium to achieve rela-

29. Ibid.

30. Ibid.

31. Minutes of PW Technical Program Meeting, 12 May 1954.

32. Minutes of PW Technical Program Meeting, 12 May 1954.

33. As noted in the previous chapter, for classification purposes the design of the two fission weapons tested by Livermore in 1955 is referred to in this study as "Manticore."

34. Duncan P. MacDougall to Norris E. Bradbury, "Topics Discussed with T. C. Merkle, UCRL, Livermore," 9 Oct. 1953.

tively small yields made little sense. Times were changing, however, and MacDougall thought material constraints would not be as great by 1958 or 1960. Under these circumstances it would be "less exciting and important" to make efficiency the high priority it now was. Furthermore atomic artillery was unlikely to remain a low priority forever and MacDougall wanted Los Alamos to be ready.

> [I]f (and this is certainly a big 'if') it is (or will become) militarily and economically sound to have and use very small weapons which produce yields of fractions of kilotons at a [low cost in] . . . active material, then I doubt that it is in the best interests of the country for us to leave this whole area to Livermore/NOL [Naval Ordnance Laboratory] or Livermore/Picatinny and simply look down our aristocratic noses at the entire business.[35]

MacDougall's comments reflect Los Alamos concerns about forfeiting the atomic artillery field to Livermore. While his GMX division had "no preconceived wish to jump in and share the [Manticore atomic artillery design] . . . puddle with Livermore," Los Alamos could not afford the political costs of appearing to neglect important military interests. This concern was compounded by the informal mandate to avoid overlapping research programs: Livermore claimed the Manticore approach to small warhead design and MacDougall had no other "bright ideas" on how to design nuclear shells in the kiloton or subkiloton range using small amounts of nuclear fuel. He proposed a review, from both the "technical and the political standpoint" of the Los Alamos small weapons program, warning Bradbury it demanded a careful balancing act. On the one hand, it would be . . . awkward to appear to be taking over a 'Livermore' class of weapon, simply because we could do the job better and faster, unless we had some good idea which would make the 'LASL' small weapons demonstrably different from the 'Livermore' small weapons. On the other hand, MacDougall asked if it would be "politically desirable" to allow the Department of Defense to believe there were important requirements Livermore was working to meet, "but which LASL was simply sneering at?"[36]

Livermore's existence forced Los Alamos to articulate its views in areas where it might not otherwise have bothered. The alternative was to risk appearing to neglect potentially important areas of research. Bradbury's response was to go on the offensive, raising anew questions about Livermore's role. The objectives of the Livermore program should be "carefully explored" after Teapot, marking another shift in outlook. Bradbury had first opposed founding a new laboratory arguing it would hurt Los Alamos. After Livermore's 1953 test fizzles, he dismissed Livermore's Basilisk designs as "old ideas." He now argued Livermore's work was redundant and in-

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35. Duncan P. MacDougall to Norris E. Bradbury, 3 Sept. 1954.

36. Duncan P. MacDougall to Norris E. Bradbury, 3 Sept. 1954.

terfered with Los Alamos'. Livermore's Manticore systems were "not new," having originated at Los Alamos as early as 1944. They were "an obvious interpolation" between designs that were already well-characterized. Applicable to tactical atomic weapons, but their high plutonium costs would make their use prohibitive given the large numbers required to make them of significant military value. At the same time, the Livermore systems were neither technically interesting nor challenging, and Los Alamos had "many more important devices" on which to focus. Bradbury proposed postponing for several years Livermore's Manticore work. Should demand for large numbers of atomic artillery shells materialize and Manticores achieve appropriate development priority, Los Alamos could undertake the task after 1957. By then, Los Alamos would have surveyed a range of possibilities, and Livermore's approach would be just one among a number of possible designs considered.[37]

Also at stake in the discussion was the design of the so-called Class D thermonuclear weapons, the smallest under active study, weighing a few thousand pounds and having yields in the low megatons.[38] Such weapons could be used in small size and yield gravity bombs or as warheads for guided missiles. Gravity bombs imposed somewhat more stringent constraints on size and weight than did guided missiles.[39] Livermore planned to test its lightweight thermonuclear weapon design in the 1955 Teapot series; Los Alamos planned two such tests. The physical limitations imposed on Class D missile warheads differed from those imposed on weapons for free fall bombs. Bradbury agreed that two optimized systems might thus be preferable to imposing the limitations of one delivery method on the other, but he thought this unnecessary. In any case, the designs emerging from laboratory competition would not differ markedly, although he acknowledged competing approaches might make Class D warheads "slightly better." They would not come any sooner, however, nor open up "any new vistas of warfare not originally foreseen in the two-stage [thermonuclear] weapons area." Bradbury's perspective on Livermore's small H-bomb program thus differed from his outlook on small fission weapons. In Bradbury's words, "Livermore designs for small two-stage systems are similar in size, weight, cost, and yield to Los Alamos systems. They differ in detail . . . but it is possible that Nature is reasonably tolerant. Rather than the cornucopia of technical options provided by laboratory competition, Bradbury thought the outcome would resemble competition between firms in technically based indus-

37. Norris E. Bradbury to Kenneth E. Fields, 22 Sept. 1954.

38. For a more precise definition of weapon classes see JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, pp. 3-4; see also Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), p. 13.

39. The constraints imposed on the nuclear warhead design of strategic missiles, particularly those that carried multiple independently targetable reentry vehicles (MIRVs), were much more severe than those of both gravity bombs and the early guided missiles.

tries. Under these conditions, science would dictate. Competition between Livermore and Los Alamos would be

> identical with the 'competitions' set up for aircraft manufacturers in the design of a new plane . . . All the designs submitted are basically the same because aerodynamics is a science and not an art, and no manufacturer will propose to produce an airplane which will fly twice as far, twice as fast, for half the weight his competitor will propose.[40]

After Teapot, Bradbury favored letting Los Alamos take over. He urged that the best features of the Los Alamos and Livermore systems be combined, or that the best system be selected. Los Alamos would proceed with ordnance development with Sandia in the "normal fashion." The objectives of the Livermore program could then be reevaluated after Teapot.

> The original concept for the Livermore Laboratory was that it should explore ideas or systems not receiving attention at Los Alamos. It was also believed in some quarters that brilliant new ideas would flow from the establishment of competition. Both the [Basilisk] .. and [Manticore designs] . . . are old ideas which had not yet seemed sufficiently urgent or promising for the LASL to exploit in competition with the urgency of other weapon demands. The two-stage area is now one in which Livermore and Los Alamos differ primarily in details of . . . engineering. The brilliant new ideas have not appeared."[41]

In short, Bradbury believed a second full scale weapons design and development laboratory unnecessary. He did not believe Livermore should be developing weapons for the stockpile. Instead, an appropriate role for Livermore would be to

> supply . . . Los Alamos and the AEC data and information on the capabilities of various nuclear systems . . . Such data may certainly involve such things as full-scale nuclear tests. However . . . the final design of any resultant weapon should be the responsibility of Los Alamos. . . . Livermore [could] supply . . . informational material relevant to nuclear explosions without . . . becoming involved in . . . ordnance engineering of specific weapons. Unless and until some really new ideas appear, present AEC facilities utilized in this manner appear to us to be adequate.[42]

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40. Norris E. Bradbury to Kenneth E. Fields, 22 Sept. 1954.

41. Ibid.

42. Ibid.

Meanwhile, both laboratory directors sought to avoid the appearance of program duplication. Bradbury's argument about similarity of objectives in small H-bomb development notwithstanding, he sought accord with Livermore. Also concerned lest the AEC view the laboratories' H-bomb tests as redundant, York told a Livermore staff gathering he would strive for close cooperation with Los Alamos.[43] For his part, Bradbury authorized his assistant Jane Hall to coordinate with Livermore to persuade the AEC the laboratory approaches to small H-bombs differed. Hall argued Livermore's approach to the Class D weapon was more difficult to characterize theoretically than was Los Alamos'.[44] And Teller told AEC Chairman Strauss Livermore's planned Teapot H-bomb test device had "quite a few differences" from the Los Alamos device.[45]

Testing Competition

The GAC met in December 1954 to discuss the laboratories' proposed Teapot tests. The members of the committee were familiar with Bradbury's concerns regarding Livermore, but these only partially informed their outlook on the new laboratory. On the one hand, they believed a second laboratory could bring independence to the weapons field. On the other, they felt the laboratory had not met expectations. GAC Chairman I. I. Rabi, for example, felt Livermore "had [not] been an effective organization in the 2 1/2 years of its existence." He wondered aloud if Livermore would ever "really be an important laboratory." Bradbury's critique of the Manticore program seem to have penetrated. One committee member questioned if Livermore's Manticore systems were bold, daring, or unconventional. Others defended Livermore, arguing the new laboratory deserved more time, and that "it was good to have a place where unconventional things get tried." Another argued a second laboratory with independent leadership was a "definite asset to the program." Others questioned whether Livermore was meeting such expectations.[46]

Because Teapot would be conducted in Nevada, tests were limited to relatively small yields. That meant small fission weapons and "mock-ups" of H-bombs, devices designed to explore only limited features of an H-bomb. Los Alamos would test small fission devices for possible application in air-to-air missile warheads and other tactical systems. Livermore proposed test firing one lightweight mockup as part of the Class D small H-bomb program, Los Alamos two. Livermore

43. Minutes of PW Technical Program Meeting, 9 June 1954.

44. Jane H. Hall to Herbert F. York, 29 Nov. 1954.

45. Edward Teller to Lewis L. Strauss, 10 Dec. 1954.

46. Minutes of 43rd GAC Meeting, 20-22 Dec. 1954, Washington, D.C., pp. 6-8.

also proposed two Manticore tests.[47] The Director Military Application told the GAC at its December 1954 meeting that the Los Alamos program was responsive to military requirements and that Livermore's proposed small H-bomb device also met this criterion. The AEC also agreed with Bradbury and York that the laboratories' H-bomb devices were "sufficiently dissimilar" to merit investigation, and each promised useful data.[48] In short, Los Alamos' proposed program, and Livermore's H-bomb test were "essential" to the development of air defense, tactical, and thermonuclear weapons.[49] The GAC agreed Los Alamos' tests were important. The Livermore and Los Alamos tests were "directed in different ways toward the development of light thermonuclear weapons."[50] Livermore's proposed Manticore tests received a less enthusiastic nod as an "interesting but somewhat problematical line of weapon development." Deemed less urgent than the other Teapot tests, the GAC recommended the AEC approve only one of the proposed two, leaving the second contingent on results of the first.[51] The AEC approved.[52] AEC concern to limit radioactive fallout was a factor. An easy way to do so was to reduce nuclear tests and the first to go were the lower priority ones.

The AEC continued to hold back on committing development funds for atomic artillery. Low efficiencies of the gun-type Los Alamos systems, and other uncertainties surrounding atomic shell development, made it unwise to commit to definite weapon designs. The AEC was no more committed to the Livermore approach to atomic artillery, informing the MLC that the feasibility of the Manticore devices could not be confirmed until they had been proved in Operation Teapot.[53] The pressure was on for the Livermore devices to perform, and the laboratory experienced success at last. A Manticore device, tested in the Tesla event, performed as expected on 1 March 1955. This led to approval of the second shot fired on 9 April, also a success.[54] A tre-

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47. Ibid. See Herbert F. York to Kenneth E. Fields, 7 Dec. 1954, for Livermore test program.

48. Minutes of 43rd GAC Meeting, pp. 15-16.

49. DMA, "Proposed Program for Operation Teapot," Report to the General Manager , 30 Dec. 1954.

50. I. I. Rabi to Lewis L. Strauss, 22 Dec. 1954.

51. Ibid.

52. AEC, "Proposed Program for Teapot," 30 Dec. 1954; Minutes of AEC Meeting No. 1051, 4 Jan. 1955.

53. K. D. Nichols to Herbert B. Loper, 7 Jan. 1955.

54. Minutes of the PW Technical Program Meeting, 23 March 1955; Minutes of 44th GAC Meeting, 2-4 May 1955, Washington, D.C., p. 6; AEC, WT-1153, "Operation Teapot, Technical Summary of Military Effects, Programs 1-9, Feb.-May 1955," p. 91. For yields, see NVO, *United States Nuclear Tests: July 1945 through September 1992*, Report DOE/NV-209 (Rev. 14) (Las Vegas, Nev., Dec. 1994), p. 3.

mendous sense of relief permeated the laboratory. A post-shot report summed up the feelings of the Livermore staff: "the shot knocked the tower down, completely. The bottom portion was melted. The results look good."[55] The Livermore light thermonuclear weapon device was also successfully tested shortly thereafter. Teller expressed his own sense of relief and satisfaction to Strauss, writing that, "all of us here are proud and happy and grateful that we had the chance to work to the point where our work needs no further elaborate justification."[56]

The Joint Committee on Atomic Energy probed the relationship of the two nuclear weapons design laboratories at a May 1955 hearing. Did the laboratories compete directly, or did they agree to divide nuclear weapons research and development responsibility between them?

> Exactly what is the relationship between Los Alamos and Livermore? How is our weapons research divided between the two laboratories? . . . Is it the Commission's policy to have the laboratories in more or less direct competition on specific weapons problems? Or is there some 'sharing of the market' arrangement, wherein one laboratory concentrates on one set of problems, and the other on another? . . . In the event that the two laboratories come up with similar proposals for developing and testing some new weapons principle, how does the Commission decide who will do the job?[57]

The JCAE also wanted to know about specific weapon developments. Displaying their typical enthusiasm for things nuclear, they asked the laboratory directors to discuss the potential for very small and very large nuclear weapons. What was the outlook for weapons weighing only a few hundred pounds and with diameters in the neighborhood of six inches? What were the prospects of very high yield thermonuclear weapons, ones "which would dwarf even the hydrogen bombs now in our stockpile?" What was being done to design a nuclear warhead for the intercontinental ballistic missile? What were the prospects for the development of atomic "hand grenades?"[58]

Responding to JCAE questions about the laboratories' weapons programs, Bradbury reported that all classes of H-bombs were well in hand.[59] At the high end, military interest peaked at

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55. Minutes of PW Technical Program Meeting, 13 April 1955.

56. Edward Teller to Lewis L. Strauss, 15 April 1955.

57. JCAE Staff to JCAE committee members, "Possible Questions for Meeting with Atomic Energy Commission Weapons Laboratory Directors, Tuesday, May 3, 1955," 3 May 1955.

58. Ibid.

59. JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, discussions throughout, but see especially pp. 3-23 for discussion of Los Alamos programs, and pp. 23-27, 38-42, and 44-50 for discussion of Livermore programs.

weapons weighing tens of thousands of pounds with multi-megaton yields for carriage on the intercontinental B-52 strategic bomber. At the low end were lighter weight weapons with yields of from one to three megatons. Its small H-bomb design confirmed in Teapot, Los Alamos would continue in this area, conducting tests in the 1956 Redwing series. Livermore's Class D device had also proved workable but the laboratory terminated active investigation in light of Los Alamos' success. Los Alamos was also working on small fission weapons for air defense, as requested by the Department of Defense. Prototypes had been tested in Teapot and the weapon was expected to go into stockpile in 1957. Other Los Alamos small fission weapons projects included an anti-submarine bomb and nuclear warheads for ground-to-air defensive missiles. Finally, Los Alamos was also developing the 8-inch atomic artillery shell.[60]

Livermore's role as the new ideas laboratory and the need to stay off Los Alamos' turf led to its investigation of the smallest diameter fission weapons considered by either laboratory to date. Los Alamos' conventional implosion systems could not readily go below diameters of about ten inches. Bradbury thus deferred to York when asked about possibilities for "pocket models" of atomic weapons.[61] Livermore's approach to weapons below 12-inch diameters would utilize Manticore designs, and York judged systems on the order of 6-inches feasible. About "atomic grenades" York was skeptical. Small as were the Livermore systems under study they still "too heavy to throw."[62]

Livermore's H-bomb research further exemplified its role as the new ideas laboratory. York told committee members Livermore had recently initiated theoretical studies of thermonuclear weapons of "very small" size and yield, as small as eight inches in diameter, well below Class D weapons diameters. Designing a suitable primary would diameter be the first challenge, and Manticore-like systems might provide fruitful lines of investigation.[63] Livermore also investigated "clean" weapons in the larger Class C, as well as Class D. Clean weapons were meant to reduce radioactive fallout, and lower the risk of contamination to soldiers and equipment. York conceded the Los Alamos approach might be superior to Livermore's, but the laboratory was not working on these "by nature of the general regions that we [Los Alamos and Livermore] do work in."[64] Finally, both laboratories had learned through informal discussion of potential mili-

60 Ibid., pp. 15-20.

61. Ibid., p. 11.

62. Ibid., p. 12.

63. Ibid., pp. 38-39.

64. Ibid., p. 25.

tary interest in weapons with yields as high as 60 megatons. Both were doing calculations to explore the possibilities, although Bradbury acknowledged that the highest feasible yield attainable in the near future was probably closer to 40 megatons. As it turned out, the trend in warhead development was towards smaller yields.[65]

Although Bradbury had raised questions about Livermore in private communications with the AEC, he portrayed harmony to the JCAE. The committee members were enthusiastic about the two-laboratory system, and interested in promoting Livermore's expansion and growth. Criticism of Livermore would only have created conflict, with little hope of gain for Los Alamos. In an explicit acknowledgement of Livermore's role Bradbury deferred to York on weapons research under Livermore's purview. He also took pains to explain that Los Alamos' work was "dissimilar" from Livermore's. For his part, York also made it clear that laboratory respected the boundaries established by the informal mandate for the laboratories to be "different," explaining Livermore was not investigating certain areas because these were the purview of Los Alamos. These comments seemed to suggest the laboratories were not competing, which concerned JCAE Chairman Clinton P. Anderson (D-N.M.), an avid supporter of competition. Bradbury responded that competition was a "curious word." In comments reminiscent of his statements to the AEC regarding competition and the aircraft industry, Bradbury pointed to the Atlas missile warhead development to illustrate his point. Los Alamos was exploring one design for the Air Force missile, Livermore another. This was competition "in a sense." It also ensured that various weapons design possibilities were explored.[66]

Atomic Artillery

Bradbury remained dubious that atomic artillery provided a useful way of fighting a modern war, as he told the JCAE at their May 1955 hearing.[67] His views echoed those of the Air Force, Los Alamos' principal military customer whose mission still dominated nuclear strategy. The laboratory's long-standing views on atomic artillery were well known to the AEC: potential improvements on gun-assembled devices did not justify further laboratory expenditures at that time.[68] The New Look boosted the priority of atomic artillery. Livermore sought to move into this niche while Los Alamos preferred to focus on higher priority systems. Livermore's Teapot

65. Ibid., pp. 42-44.

66. Ibid., pp. 74-75.

67. Ibid., p. 69.

68. Vincent G. Huston to Donald J. Leehey, "Feasibility Study for Improvement of the 8" Shell," 1 June 1955.

successes were all that was needed for the AEC to transfer the lead role in atomic artillery to the laboratory in June 1955, though no specific weapons assignment was yet involved. This suited Los Alamos which had long sought to rid itself of such responsibility. The Army displayed some reluctance towards the arrangement. Livermore's lack of a proven record in weapons development played a part. The Army's Assistant Chief of Ordnance had also learned Los Alamos might have "a quicker and better approach" for the improved 8-inch shell.[69] Despite any misgivings, however, the Army now enjoyed a working relationship with a laboratory willing to place high priority on its interests. In many ways, it was a perfect match. Both the Army and Livermore were underdogs, Livermore boxed out by Los Alamos, the Army by the Air Force. Their relationship benefitted both organizations. Livermore's atomic artillery work helped justify expansion of the laboratory's facilities and programs. The Army benefitted from Livermore's undivided attention.

The transfer of responsibility was not immediately accompanied by a weapon development assignment. In 1954 Los Alamos started work on the W-33 atomic shell for the 8-inch gun. It was the last artillery shell developed by the laboratory and was long-lived in the stockpile. Deployed in 1955, it was not retired until 1989 (Table 1). Until recently W-33 shells constituted approximately half of the total atomic artillery shells in the stockpile. Los Alamos developed one more atomic shell, the W-74, although it was cancelled prior to deployment in favor of Livermore's W-79 in 1973, developed in both conventional and enhanced radiation versions. The latter, commonly referred to as the neutron bomb, was politically controversial. Livermore began development of the W-48 atomic shell in 1957 which was supposed to be have been replaced by Livermore's W-82 atomic shell in the 1990s. The latter was never deployed because this would have coincided with the removal of all European theater nuclear weapons.[70]

The downside, as well as the benefits, of Livermore's role as the new ideas laboratory were illustrated in this chapter. It left the laboratory exposed to potential critics who might charge that its programs were aimed at low priority projects of modest technical promise. These were the among the reasons, after all, that Los Alamos had abandoned them. Some Livermore designs were indeed Los Alamos discards as Bradbury charged, including Basilisks and Manticores.[71] Making these ideas their own, Livermore scientists dug deeper, seeking different applications

69. W. L. Bell, Jr., to Alfred D. Starbird, 13 March 1956.

70. Hansen, *U.S. Nuclear Weapons*, p. 107. S. T. Cohen, *The Neutron Bomb: Political, Technological and Military Issues* (Cambridge, Mass., and Washington: IFPA Special Report, 1978), for an interesting history of the neutron bomb from the perspective of a participant.

71. As noted in Chapter 3, for classification purposes the design of the two fission weapons tested by Livermore in 1953 is referred to in this study as "Basilisk."

and new technical approaches. The General Advisory Committee's approval of only one of the two proposed Livermore Teapot tests caused the laboratory no severe hardship. When the first test went well, the AEC promptly approved the second. But the incident does illustrate the consequences of being the new ideas laboratory. These drawbacks notwithstanding, the laboratory had little choice. Another imperative for Livermore's successful expansion and growth was the need to demonstrate its utility to the military. This drove the laboratory to neglect fundamental research in favor of developing weapons for the stockpile. Here too the risk was worth taking. The alternative was to forego developing weapons for the stockpile. With no weapons to its credit, the laboratory would be hard pressed to justify additional resources.

Livermore complicated life for Los Alamos, forcing laboratory leaders to articulate their position on various programs. Livermore's technical successes and growing military demand for new weapons prompted a shift. This chapter saw Bradbury move from urging Livermore become a supplier to Los Alamos to his willingness to cooperate with the laboratory. By spring 1955 the laboratory directors had moved toward a more broadly defined cooperative relationship, resulting in part from Livermore's successful Teapot tests. York was interested in better defining the relationship because the alternative would have been to be seen as superfluous. For both directors the answer to this problem was to make sure their laboratories took different approaches to solving technical problems. Laboratory cooperation thus helped define the general areas of investigation each laboratory pursued. Los Alamos focused on weapons developments of central concern and highest priority as defined and put into practice by the New Look, inheriting this job by virtue of its senior status. Livermore's success relied most on expanding military demand for nuclear weapons as well as its technical competence. These two factors came together for the laboratory in the mid-1950s.

6. EMERGING PATTERNS

Livermore's Early Nuclear Weapons Development Assignments 1955-1956

Livermore and Los Alamos were in a relationship of competitive interdependence in that they competed for the resources of another party, the AEC.[1] Jeffrey Pfeffer and Gerald R. Salancik have argued that increased coordination or cooperation are typical solutions to the uncertainties caused by interdependence.[2] Livermore and Los Alamos had begun to coordinate their activities by the mid-1950s, trying to reach agreement on how to divide weapon development responsibilities. They did not negotiate from equal strength. Los Alamos, the more experienced laboratory, had the confidence of the armed services. It had acquired these by default, being the only full-scale weapons development laboratory until the founding of Livermore. For its part, Livermore had no experience developing weapons for the stockpile. When it would gain the full complement of facilities enabling it to do so would depend on military demand for weapons, Livermore's technical success, and its bureaucratic skills.

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1. W. Richard Scott, *Organizations: Rational, Natural, and Open Systems*, 3rd (Englewood Cliffs, N.J.: Prentice-Hall, 1991), p. 198.

2. Jeffrey Pfeffer and Gerald R. Salancik, *The External Control of Organizations: A Resource Dependence Perspective* (New York: Harper & Row), 1978, pp. 40-43.

"New Role" for Livermore

Livermore's successful Teapot tests helped pave the way for the laboratory's first weapon development assignment in summer 1955. So did growing military demand for nuclear weapons. Also in play were differing Navy and Air Force needs for a Class D "small" H-bomb. The Navy wanted a small H-bomb for its Regulus missile, while the Air Force wanted one for its fighter bombers. Air Force bomb parameters were constrained by what the fighter bombers could carry. Regulus could carry a larger and higher yield warhead, which the Navy preferred. Initially, the Department of Defense had called for a common warhead for both applications. This meant the Navy warhead would be constrained by the limitations of the Air Force requirement for the smaller bomb.[3] Bradbury informed the Joint Committee on Atomic Energy at their early May 1955 hearing that Los Alamos would focus on the high priority small Class D H-bomb, designated the TX-28, around the constraints imposed by the 100 series Air Force fighter bombers.[4]

Both laboratories had tested small H-bombs in Teapot. Livermore's program was idled because Los Alamos would develop the Class D warhead. Navy interest in its own weapon boosted prospects for Livermore's first weapon development assignment. Bradbury and York met in June 1955 to discuss the weapons program in what became regular liaison meetings to coordinate laboratory activities. The directors agreed to recommend Livermore develop a nuclear warhead designated the XW-27 for Regulus.[5] Bradbury stressed the benefits of the arrangement for Los Alamos, which would be freed to devote more time to other work:

> [T]he benefits from having Livermore actually engage in practical weapon design as well as the benefits which will accrue to the LASL in having somewhat more time available to devote to the design of our other systems outweigh a delay of this order [of two months] for the XW-27 [resulting from the assignment to Livermore]. Accordingly we . . . recommend that the Commission accept the Livermore proposal.[6]

For its part, Livermore would gain necessary weaponization experience and begin to "carry its share in this kind of work."[7] The Military Liaison Committee was expected to be supportive,

3. Vincent G. Huston to Kenneth E. Fields, 13 June 1955. The Air Force would carry the Class D externally on the B-47 and F-105 aircraft, and an effort would be made to achieve a bomb bay carriage capability in the F-105.

4. JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, p. 7.

5. The 'X' in XW-27 and other warheads similarly designated stood for experimental. Once deployed in the stockpile, the warhead would be designated the W-27.

6. Norris E. Bradbury to Vincent G. Huston, received by AEC 7 June 1955.

7. Herbert F. York to Vincent G. Huston, 6 June 1955.

since it had encouraged a separate XW-27 development.[8] The Navy weighed in, informing the MLC it would "require as a matter of priority" the development of a Class D weapon with the size and yield characteristics of the Livermore's proposed design.[9]

Reassured by York the assignment would not disrupt other laboratory programs, the AEC asked Livermore to develop the Navy warhead.[10] The laboratory's first weapon development assignment, it was also "a new role for Livermore."[11] The laboratory would give top priority to the project, dropping longer-term weapon projects to accommodate it, including its investigation of the smallest H-bombs then under consideration. The small fission weapons program would continue as before.[12] The Livermore technical staff group learned officially of the news at its regularly scheduled meeting in early July. York read a message from new Director of Military Application General Alfred D. Starbird which informed him the AEC had approved Livermore's "new weaponizing role."[13] The laboratory program was not yet advanced enough to design all the components for the new weapon, at least not in a timely fashion. Los Alamos therefore provided a Los Alamos-designed primary for the XW-27 warhead.

Despite its new role, conditions at Livermore remained spartan in summer 1955, although an on-site cafeteria was finally approved on 6 June.[14] Real progress was made that summer toward making Livermore into a full-scale weapons design, test, and development laboratory. Livermore's pending development of the XW-27 warhead provided the rationale for expanding Livermore facilities and capabilities, where previously the AEC had been reluctant to do so. Discussions took place in June, between the AEC, Livermore, and Sandia laboratory representatives about possible Sandia assistance for Livermore.[15] Sandia Corporation, based in Albuquerque, had originated as Los Alamos' weaponization division in World War II and continued to assist Los

8. Huston to Fields, 13 June 1955.

9. Jack L. Armstrong to Donald J. Leehey, 17 June 1955.

10. See Vincent G. Huston to Herbert F. York, 7 June 1955, for AEC inquiry regarding impact of W-27 program on Livermore, and Herbert F. York to Vincent G. Huston, 8 June 1955, for reply.

11. Huston to Fields, 13 June 1955.

12. York to Huston, 6 June 1955.

13. Minutes of PW Technical Program Meeting, 6 July 1955.

14. Minutes of PW Technical Program Meeting, 7 Sept. 1955.

15. Minutes of PW Technical Program Meeting, 8 June 1955.

Alamos in that function after it became an independent facility in 1948.[16] The laboratory's primary responsibility was for arming, fuzing, and other nonnuclear weapons components, including the interface between the nuclear warhead and delivery system.[17] Five people from Sandia-Albuquerque were in place by September 1955 to assist Livermore's development of the Regulus warhead.[18] An old Navy barracks at Livermore housed the first arrivals. Sandia President James W. McRae promised about 120 Sandia personnel by end of 1956.[19] The AEC formally announced Sandia-Livermore as a permanent branch of the Albuquerque facility in March 1956.[20] In October 1957 the Sandia staff occupied their first permanent building in Livermore.[21] The site and staff grew rapidly, and Sandia requested new construction to house up to 1000 staff by spring 1958.[22]

Livermore also made progress during summer 1955 in another crucial area: acquisition of a high explosive test facility. The laboratory had sought such a facility for at least two years, ever since seeking to expand its small fission weapons program. The AEC had been reluctant to duplicate Los Alamos facilities. Without a convenient place to machine and test high explosives Livermore was forced to conduct experiments at the Nevada Test Site, at Los Alamos, or elsewhere. Livermore's W-27 responsibilities finally prompted the AEC to approve acquisition of the necessary land fourteen miles from Livermore and designated Site 300.[23] Until all details were finalized the laboratory continued to rely on the Nevada Test Site. One laboratory scientist recalls grueling trips to Nevada for large numbers of high explosive tests in preparation for the 1956 Redwing test series.[24]

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16. Necah Stewart Furman, *Sandia National Laboratories: The Postwar Decade* (Albuquerque: Univ. of New Mexico, 1990), pp. 251-281; SNL, *8100 Directorate: The First Thirty Years* (Livermore, n.d, c. 1986), p. 1.

17. SNL, *The First Thirty Years*, pp. v/vi.

18. Minutes of PW Technical Program Meeting, 7 Sept. 1955; Minutes of PW Technical Program Meeting, 30 Nov. 1955.

19. Minutes of PW Technical Program Meeting, 30 Nov. 1955.

20. SAN, "Chronology," n.d. (c. mid-1960s), entry for March 1956.

21. SNL, *The First Thirty Years*, p. v/vi.

22. Minutes of 51st GAC Meeting, 29-31 Oct. 1956, Washington, D.C., p. 24.

23. SAN, "Chronology," entry for June 1955.

24. Interview with Charles A. McDonald, Jr., LLNL, 7 Oct. 1991.

Administrative changes made by summer 1955 also reflected Livermore's move towards permanence. The General Advisory Committee had expressed concerns the previous year about Livermore's lack of a full-time director.[25] This prompted E. O. Lawrence to name Herb York director of the UCRL-Livermore branch.[26] Until then Lawrence has served as official director in his capacity as UCRL head, although York had overseen Livermore's day-to-day operations. UCRL-Livermore was rapidly outpacing the UCRL-Berkeley laboratory in funding and staff. In spring 1955 Livermore had 53 theoretical and 255 experimental physicists to Berkeley's 37 and 201, and 1004 support personnel to Berkeley's 978.[27] Livermore's total staff topped Berkeley, 1633 to 1541.[28] And it was still growing. The Livermore budget request for fiscal year 1955 was $13.80 million and $20.75 million for fiscal year 1956.[29] The large difference was mostly attributable to Site 300 activities.

Rounding out the laboratory's move towards permanence, Livermore also created a weapons engineering division in summer 1955, expected to grow to 250 by 1956.[30] Weaponization transforms a nuclear device—designed only to determine if a design concept can work—into a manufacturable weapon. The means for doing so was crucial to Livermore's ability to develop weapons for the stockpile. The laboratory also became a member of the Special Weapons Development Board, with York serving as its principal representative. The board, and other service sponsored committees, helped keep Livermore in closer contact with the interests of the armed services.[31]

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25. Minutes of 41st GAC Meeting, Albuquerque and Los Alamos, 12-15 July 1954, p. 55.

26. E. O. Lawrence to Harold A. Fidler, 2 Sept. 1954. The UCRL business manager also noted that York had been named director in Wallace B. Reynolds to Fidler, "UCRL Program Personnel and Funding Requirements," 8 July 1955; see item 12 in attached "Discussion Information For Staff Paper on UCRL Ceilings." Both documents provide a brief overview of UCRL's involvement with the nuclear program.

27. Reynolds to Fidler, "UCRL Program Personnel and Funding Requirements," 8 July 1955, with attached Exhibit A, "Laboratory Personnel."

28. Minutes of PW Technical Program Meeting, 7 Sept. 1955.

29. Minutes of PW Technical Program Meeting, 27 July 1955.

30. Minutes of 49th GAC Meeting, Washington, D.C., 28-30 March 1956, p. 39; Minutes of PW Technical Program Meeting, 30 Nov. 1955.

31. Minutes of PW Technical Program Meeting, 7 Sept. 1955.

Ballistic Missiles and the Weapons Laboratories

Los Alamos' growing weapon development workload contributed to its more conciliatory outlook towards Livermore even while increasing military demand helped propel Livermore's growth. The number of nuclear warheads entering stockpile between 1945 and 1960 equalled the number deployed in the subsequent thirty years (Figure 2). Most deployments were concentrated in the late 1950s and early 1960s. In 1958, five new warheads were deployed in the stockpile, four designed by Los Alamos (Figure 2). The number of nuclear weapons in engineering and production during this period was even greater, since modified versions of a warhead could be used in more than one delivery system (Figure 4). The engineering phase is the most resource intensive phase of the AEC's seven-phase weapons design, test, and development process.[32] Even feasibility studies were becoming burdensome. These joint AEC-DOD studies analyzed technical and other issues prior to DOD's formal request for a new weapon required laboratory participation.[33]

An important element of new military demand was growing interest in ballistic missiles. These had been of interest since at least World War II, but partisans of strategic bombing within the Air Force succeeded in delaying their development. By June 1953, however, the Joint Chiefs of Staff had recommended to the Secretary of Defense that the ballistic missile program be reviewed. By late fall Trevor Gardner was tasked by the Secretary of the Air Force to head a newly formed Strategic Missiles Evaluation Committee (SMEC). Commonly referred to as the von Neumann Committee after its chairman John von Neumann, SMEC held its first meeting in November 1953. A parallel study centered at the RAND Corporation was begun in fall 1953.[34] The RAND and von Neumann Committee findings issued in February 1954 came to similar conclusions: missiles, in particular, the Atlas intercontinental ballistic missile (ICBM), could be built much sooner than previously anticipated.[35] A reconstituted von Neumann Committee was established in April 1954 to determine the organizational requirements of the ICBM/Atlas program. The committee's expanded membership included laboratory representation: York of Livermore, and Darol Froman of Los Alamos.[36] By late fall 1954 the Air Force organization for ballistic

32. In phase 3, the warhead design is modified to conform with the military delivery system, the most resource intensive for the laboratories. Phase 3 is preceded by concept and design phases, and followed by production engineering, stockpile production, and retirement and disposal. See Mark Dickinson, SNL, "DOE and DOD Acquisition Interfaces," 29 March 1991, for an explication of weapon development phases.

33. Harry E. Skinner to Alfred D. Starbird, "Feasibility Studies," 27 Sept. 1955.

34. Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York: Columbia Univ. Press, 1976), pp. 154-157.

35. Ibid., pp. 159-162. The RAND study provided input to the von Neumann Committee study.

36. Ibid., p. 172; Herbert F. York, *Race to Oblivion: A Participant's View of the Arms Race* (New

missile development was in place.[37] In October, the AEC asked Livermore and Los Alamos to investigate thermonuclear warhead designs for the Air Force Atlas intercontinental ballistic missile.[38] The Titan missile would serve as a back-up to Atlas in the event of the latter's failure.[39]

Proponents of a U.S. missile development program got a jolt in May 1955, confronted by a display of Soviet air power in the May Day celebrations highlighting the new Soviet strategic bomber, Bison.[40] If the Soviets had caught up with U.S. capabilities in strategic aircraft design, so too might their missile program have progressed. The Killian Committee, appointed by President Eisenhower, warned that failure to accelerate U.S. strategic missile programs might leave the Soviets in the lead by 1960. The Joint Committee on Atomic energy weighed in, recommending the President give high priority to the U.S. ICBM program.[41]

Increased attention to the strategic missile program prompted an AEC inquiry to the laboratory directors in August 1955. AEC Director of Military Application Alfred D. Starbird was concerned the laboratories had not made sufficient progress on the Atlas nuclear warhead and asked for a status report.[42] York responded that Livermore's limited weaponizing capabilities precluded full participation, so the laboratory's work largely revolved around theoretical studies.[43] Laboratory membership on the Joint Air Force-AEC nose cone committee kept program scientists abreast of relevant details. Preliminary design studies based on the Class D small H-bomb, though not specifically aimed at the Atlas missile, would help advance thinking on the problem. Based on these studies York believed a one megaton yield could be achieved within the design limitations of the missile. In any case, no experimental data would be available until after Redwing.[44] Los Alamos planned a mockup test of the Atlas warhead in 1957, to be followed by a full scale test in 1958. Since missile characteristics would not be available prior to

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York: Simon & Schuster, 1970), p. 86.

37. Beard, *Developing the ICBM*, p. 178.

38. Harry E. Skinner to Alfred D. Starbird, 8 August 1955.

39. Beard, *Developing the ICBM*, p. 210.

40. Thomas B. Cochran et al., *Soviet Nuclear Weapons*, vol. 4 of "Nuclear Weapons Databook" (Cambridge, Mass.: Ballinger, 1989), p. 229.

41. Ibid., pp. 186-188.

42. Alfred D. Starbird to Norris E. Bradbury, James W. McRae, and Herbert F. York, 9 August 1955.

43. See Harold A. Fidler to Alfred D. Starbird, "Comments on ICBM Memo," 21 Oct. 1955; Herbert F. York to Harold A. Fidler, "Accelerated Livermore Program," 14 Oct. 1955, attached.

44. Herbert F. York to Alfred D. Starbird, 30 August 1955.

that, Bradbury argued the laboratory's schedule was "adequately correlated" with predictions for the missile's availability. Bradbury also reminded Starbird that the laboratory did not yet have formal approval for its Atlas program.[45]

ICBM development was designated high national priority by presidential directive in fall 1955. In November, national priority was also given to intermediate range ballistic missile (IRBM) development. Two separate development programs were authorized—the Air Force Thor and the joint Army-Navy Jupiter. The laboratories received their first official notification of the IRBM program in January 1956.[46] Livermore and Los Alamos intensified their efforts. Both laboratories were expected to go through a full-scale test program. Just one of the two laboratories would normally have been asked to take a warhead through the development stage, but the importance of the ICBM program called for a different approach. The summer 1956 Redwing tests would contribute to ongoing paper studies for the Atlas warhead.[47] Low-yield mock-up nuclear devices would be tested in the 1957 Plumbbob test series. Full-scale nuclear tests would await the Pacific series scheduled for 1958.[48] Only then would a decision be made about which laboratory and which warhead design would be selected for Atlas.[49]

DOD wanted to use the same warhead for both the ICBM and IRBM missiles.[50] Los Alamos and Livermore each had two preliminary warhead concepts for the Atlas, Thor, and Jupiter. Whichever of the four proposed designs were chosen would also probably be used in the first models of the larger Titan ICBM. The four concepts seemed interchangeable from the standpoint of external geometry, although a separate warhead for Jupiter was thought potentially desirable.[51] By spring 1956, the Livermore Atlas warhead design studies were grouped under the designation XW-38, and Los Alamos', the XW-35.[52]

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45. Norris E. Bradbury to Alfred D. Starbird, "Intercontinental Ballistic Missiles Program," 13 Oct. 1955.

46. Alfred D. Starbird to Harold A. Fidler, 27 Jan. 1956.

47. Herbert F. York to Alfred D. Starbird, 17 Jan. 1956; Norris E. Bradbury to Starbird, 26 Jan. 1956.

48. Alfred D. Starbird to Reuben B. Robertson, 19 March 1956. Initially code named Pilgrim, the spring 1957 test series became Plumbbob shortly before testing began. See Barton C. Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley: Univ. of California, 1994), p. 186.

49. Alfred D. Starbird to Norris E. Bradbury, 6 Jan. 1956.

50. Herbert B. Loper to AEC Chairman, 1 March 1956.

51. Kenner F. Hertford to Alfred D. Starbird, "Basic Sameness of Warheads for Jupiter, Thor, Atlas, and Titan," 8 June 1956.

52. James W. McRae to Alfred D. Starbird, 28 June 1956.

Division of Labor

Livermore and Los Alamos spent winter 1955-1956 preparing for Redwing. Livermore's proof test of the XW-27 Regulus missile warhead was a success, the laboratory's first warhead destined for the stockpile. By then the W-27 was also expected to deployed in bombs mounted on carrier-based navy aircraft. The latter turned out to be the most utilized. On the order of several dozen W-27s were deployed on Regulus, but hundreds were eventually manufactured for loading on the Navy's fighter-bombers. Air Force and Navy cruise missiles alike had a controversial and technically disappointing history, and soon lost out to ballistic missiles. Regulus warheads vanished from the stockpile by 1965, while Livermore's W-27 Navy bombs were gone by 1962. Plans to deploy the XW-27 with the Air Force Rascal, Snark, and Matador missiles never matured because the missiles themselves were never deployed.[53]

Redwing test results showed small diameter megaton weapons feasible, and Los Alamos' small H-bomb design for the Air Force was designated the TX-28. The B-28 (the stockpile name for the TX-28) was long-lived. This "workhorse" was only phased out in the early 1990s, albeit much modified from its original form. Military demand largely made the difference between the W-27 and B-28 systems. The Air Force ordered thousands of the B-28 warheads compared to the hundreds of W-27s deployed on naval aircraft.[54]

The three small diameter fission devices tested by Livermore in Redwing confirmed their utility for atomic artillery. A modified version of one of these devices was also a potential primary prototype useful for the small H-bomb Livermore hoped to develop.[55] These had evolved from the Manticore concepts tested by Livermore the previous year.[56] Lessons learned from previous experiments and advances in computer modelling had contributed to substantial design modifications and improvements. The result was an innovative small fission weapon, with improved fuel efficiency, in a compact system suitable for atomic artillery, or in modified form, as a small primary. Livermore's success prompted the frank admiration of Duncan MacDougall of Los Alamos, who had two years earlier alerted Bradbury to Livermore's small fission weapon work. He congratulated John S. Foster, director of Livermore's "B" Division in charge of fis-

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53. SNL, *The First Thirty Years*, p. 132. Hundreds of W-27 bombs were deployed, while only tens of missile warheads were ordered.

54. Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), p. 148-154.

55. Minutes of PW Technical Program Meeting, 15 June 1955.

56. As noted in chap. 4, for classification purposes, the design of the two fission weapons tested by Livermore in 1955 is referred to in this study as "Manticore."

sion weapons development. Having "sweated out the problems" connected with Los Alamos small fission weapons, MacDougall could "appreciate the magnitude of your accomplishment in solving the even more complex problems in developing [Livermore small weapons]. . . . Congratulations!"[57]

Livermore's accomplishments prompted another shift in Bradbury's outlook towards Livermore. Since both laboratories "possess[ed] the ability to provide satisfactory designs" he argued parallel weapons programs were a waste of effort and resources. Why not agree on which laboratory would develop what system?

> . . . it would seem preferable to decide . . . that one laboratory or the other will conduct necessary future tests and development of each of the specific systems . . . Such a division of effort can only result in a more effective, and possibly more rapid, nuclear weapons development program.[58]

The AEC liked the idea and asked to meet with laboratory representatives on 15 August 1956 to discuss how to divide weapons development responsibilities. But first the Director of Military Application asked the laboratories to send him initial recommendations.[59] Laboratory suggestions were based on their knowledge of military requirements gained through a variety of sources. Informal contacts between scientists and officers were one means, as were contacts through joint committees.[60] The General Advisory Committee and the Joint Committee on Atomic Energy were also important sources. The Military Liaison Committee was the formal channel through which military requirements were communicated. The AEC learned about service wants and needs through joint meetings with the MLC and passed these on to the laboratories.

Bradbury had already sketched out his proposed division of labor. He seemed reluctant to make his preferences clear regarding programs of highest national priority, or of low priority, perhaps out of concern not to appear too imperial. On the horizon, for example, was Atlas, a major new development program which Bradbury judged of "overriding national priority and therefore . . . entitled to the utmost in competitive effort. . . . This area would seem to warrant

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57. Duncan P. MacDougall to John S. Foster, 26 June 1956. For an overview of the experimental physics "A" and "B" divisions, see LRL, *Status: Fiscal Year 1958*, "Status Report, July 1, 1957-June 30, 1958 of the Ernest O. Lawrence Radiation Laboratory," n.d. (c. 1957), pp. 80-83, 87-88; Jonathan E. Medalia, "Nuclear Weapons Stockpile Stewardship: The Role of Livermore and Los Alamos Laboratories," CRS Library of Congress Report 94-418 F, p. 34.

58. Norris E. Bradbury to Herbert F. York, 16 July 1956, draft.

59. Alfred D. Starbird to Norris E. Bradbury, Herbert F. York, and others, "Discussion of Post Redwing Weapons R&D Program," 30 July 1956.

60. See, e.g., Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), chap. 5.

an extensive discussion between our two laboratories." He did not know how the field should be "split" as it seemed to him "to be a very difficult question." Bradbury also did not commit himself in advance on "very small weapons." His laboratories would continue whatever studies were already being conducted. Responsibility for specific weapon development should be decided on an ad hoc basis. Los Alamos would do "whatever was required" in the classical gun-type weapons, but would conduct no further tests unless worthwhile improvements could be made.[61]

The laboratory directors and their top staff held a pre-meeting meeting at Los Alamos on 14 August to decide on their course of action for the next day. They agreed Los Alamos would take responsibility for the Atlas warhead. A modified version of the Los Alamos TX-28 Air Force bomb would provide the basis for the design.[62] As discussed above, DOD wanted to use the same warhead in all four ballistic missiles, Atlas, Titan, Thor, and Jupiter. Nevertheless, Livermore would investigate a warhead for Titan, a back-up program against Atlas failure. Titan might be able to carry the larger higher yield Livermore XW-27 warhead being developed for the Navy. The meeting with the AEC largely ratified laboratory suggestions. They also resolved that Livermore would take the lead on improved atomic artillery and for exploring the feasibility of 155-mm and 105-mm shells, the latter for possible use in howitzer, mortar, and recoilless rifle systems. In other fission weapons development Los Alamos took responsibility for ASROC—the Navy's anti-submarine rocket warhead—while Livermore would develop a warhead for the Terrier, Little John, and Project C-ADM weapons. Livermore also took the lead on investigating possibilities for a 1 megaton warhead in a "small package."[63] Of ten weapons with firm stockpile entry or operational availability dates Livermore had one: the Regulus warhead. The future division of labor was expected to be more equal. The laboratories each took responsibility for about half of the seven or eight new projects which had not yet been assigned specific stockpile entry dates.

Laboratory cooperation was supported by the General Advisory Committee as well as the AEC. Bradbury made sure to tell GAC members about it when the laboratory directors met with them in late October. Such cooperation helped AEC management of the weapons program and helped avoid program duplication. The laboratories operated under a dual imperative, however. The Joint Committee on Atomic Energy members were enthusiastic supporters of laboratory competition. Bradbury thus hastened to add in his comments to the GAC that laboratory cooper-

61. Norris E. Bradbury to Herbert F. York, 16 July 1956, draft. It is not clear this letter was ever sent.

62. Minutes of 51st GAC Meeting, p. 21.

63. Duane Sewell, untitled notes of meeting at Los Alamos of Livermore, Los Alamos, and Sandia administration and staff, 14 August 1956.

ation did "not preclude" either one from considering the whole spectrum of potentially interesting weapon designs.[64]

Livermore Gets Short Shrift

Following the August meeting with the laboratory directors the AEC sent its proposed program for meeting military requirements to the MLC.[65] The response suggests the AEC was surprisingly out of touch with military thinking. Suggested MLC changes caught the AEC and the laboratories off guard. Many had a disproportionate impact on Livermore, much to the dismay of laboratory leaders. Livermore had accepted responsibility for programs aimed at longer-term, speculative, or non-existent military requirements, more vulnerable to cancellation or change. An important area involved the ICBM and IRBM warhead programs. The laboratories had agreed Los Alamos would develop the XW-35 for all four missiles while Livermore would design a follow-on warhead for Titan, the XW-38. Titan's greater payload capacity would allow it to take advantage of the greater weight, size, and yield Livermore warhead.[66] AEC confidence in the Los Alamos warhead was such that no back-up program was needed and the Livermore warhead would instead be considered a follow-on to it. Pleased though DOD was to learn the Los Alamos warhead no longer needed a back-up program, no technical specifications were yet available for a new missile. AEC plans for a new warhead were thus premature.[67] In short, according to DOD the Livermore XW-38 development program was unnecessary.

DOD guidance in other weapon categories also left Livermore with no specific weapons development assignments. Los Alamos would develop one warhead for the ASROC, Terrier, Little John, and Project C-ADM weapon systems. ASROC's stringent technical parameters meant the warhead would be "overdesigned" for the other applications. From the MLC's perspective, however, the logistical and operational simplicity of a single design outweighed disadvantages. DOD reported no need for a laydown bomb—designed to lie on the ground before exploding to allow delivery aircraft to escape—exceeding 1 megaton, another Livermore area of responsibility. Neither had DOD yet developed an official requirement for large clean weapons, also Livermore's purview. And while the innovative new small fission weapon derived from Livermore's early Manticore design work appeared "very attractive in the small warhead field," here, too, DOD had no known need for it. Finally, there was no firm requirement for the improved 8-inch

64. Minutes of 51st GAC Meeting, p. 21.

65. Herbert B. Loper to AEC Chairman, "AEC Post Redwing Development Program," 21 Nov. 1956.

66. Herbert F. York to Alfred D. Starbird, 18 Sept. 1956.

67. Herbert B. Loper to AEC Chairman, "AEC Post Redwing Development Program," 21 Nov. 1956.

atomic shell Livermore had expected to develop, although the Department of Defense was considering the matter.[68]

The DOD guidance "caused a considerable stir" at Livermore.[69] There was no comparable concern at Los Alamos.[70] All five weapons systems terminated or suspended were Livermore's. The MLC guidance, if taken literally, thus meant there was "no firm weaponization program" for Livermore, as the manager of the Santa Fe Operations field office observed.[71] The program represented "half of our potential capability," as York put it. He acknowledged the original program—calling for adding three weaponization programs to Livermore per year—would probably have been more than the laboratory could handle comfortably. But he argued that a certain philosophy was supposed to guide the allocation of weapon development responsibilities. Each laboratory "was, and should remain, competent in all fields of nuclear weaponry," with only two exceptions: Los Alamos had primary responsibility for classical implosion and gun-assembled bombs, while Livermore was responsible for improved atomic artillery. The work load should be "equitably divided," so that each laboratory could explore new ideas.[72]

Differences in terminology between the MLC and AEC may have contributed to the problem. The term "military requirement" as used by DOD indicated a new weapon had not only been approved for development by the Joint Chiefs of Staff, but that its technical parameters, including dimensions, weight, and yield, had been determined. A weapon could not be treated as a "military requirement" until it had gone through the JCS approval process, even if there was service interest in it. Informal discussions among service representatives, AEC, and design laboratories were not be sufficient to establish a formal military requirement.

The problem was compounded for AEC planning purposes because official military requirements tended to come at the end of DOD's extensive weapon research and testing program. Waiting for official DOD notification of the delivery system's availability, or even until official requirements were issued for a nuclear warhead, could mean delays. The laboratories thus often began studying nuclear warhead designs before DOD had issued formal military requirements.

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68. Ibid.

69. Harold A. Fidler to Alfred D. Starbird, "AEC Post-Redwing Weapon Development Program," 28 Dec. 1956.

70. Norris E. Bradbury to AEC, 18 Dec. 1956, cited in Kenner F. Hertford to Alfred D. Starbird, 21 Dec. 1956.

71. Fidler to Starbird, "AEC Post-Redwing Weapon Development Program," 28 Dec. 1956.

72. Herbert F. York to Norris E. Bradbury, 13 Dec. 1956.

Flexibility to do so was provided by the phased system of warhead development, with phase 1 allowing exploratory research.[73]

The AEC advised Livermore to assume for planning purposes that DOD would generate new requirements and that the laboratory would weaponize at least two devices per year beginning in 1959 or 1960. Livermore should thus continue its work on a follow-on ICBM warhead, but not under anything as specific as the XW-38 rubric.[74] In the meantime, Los Alamos' XW-35 would meet DOD requirements for a ballistic missile warhead.[75] The XW-38 program was redefined as a continuing research effort to support follow-on missiles. It would revert to phase 1, general research studies of an exploratory nature, since delivery system parameters, including yield and requirements were not yet available.[76] As it turned out, the AEC strategy was vindicated. By May 1957 DOD had decided on a heavier higher yield warhead.[77]

Livermore and Los Alamos were also to proceed on the assumption that two warheads, not the single one recommended by the MLC guidance, would be required for the ASROC, Little John, Terrier and Project C-ADM weapon systems. Notwithstanding MLC guidance for a single warhead for all applications the AEC had yet to receive official service-coordinated and approved military specifications for it. The laboratories would thus both continue as agreed. Other AEC recommendations to the laboratories adhered to MLC guidance. Livermore abandoned its laydown weapon program, though exploratory research would continue. The laboratory also shelved its artillery program pending a go-ahead from DOD. The laboratory would continue general studies on atomic projectiles of various calibers in coordination with the Army Ordnance's Picatinny Arsenal and the Armed Forces Special Weapons Project.[78]

The AEC continued planning new facilities and personnel for Livermore. The augmented laboratory could still handle no more than two weaponization assignments per year. Keeping development programs to a minimum would also leave the laboratory free to investigate new ideas, "a field of endeavor in which UCRL [Livermore] had always been uniquely qualified."[79]

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73. See, e.g., discussion of this problem in JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-8-MA, 18 May 1955, pp. 23-24.

74. Fidler to Starbird, "AEC Post-Redwing Weapon Development Program."

75. Norris E. Bradbury and James W. McRae to Alfred D. Starbird, 3 March 1957.

76. Fidler to Starbird, "AEC Post-Redwing Weapon Development Program."

77. J. B. Macauley to Lewis L. Strauss, 27 May 1957.

78. Fidler to Starbird, "AEC Post-Redwing Weapon Development Program."

79. Ibid.

Growing military demand for nuclear weapons prompted Los Alamos to seek coexistence with Livermore. Cooperation helped control some of the uncertainties of dealings with a rival laboratory. The alternative was to leave decisions to the AEC. Cooperation benefitted Livermore, but for different reasons. The laboratory needed weapon development responsibilities to enhance its legitimacy, continued survival, and growth. This gave Livermore an incentive to cooperate with Los Alamos. Cooperation benefitted both laboratories, but Livermore had less bargaining leverage. Livermore was the big loser when AEC and laboratory representatives met to allocate weapons program responsibilities. More experienced, and with established ties to the military, Los Alamos gained responsibility for the highest priority and most urgent military requirements. Livermore found opportunities in nuclear systems that were more speculative, that did not yet have formal JCS authorization, or were low priority. As this chapter has shown, such systems were more prone to cancellation by the military. The next chapter highlights Livermore strategies for overcoming some of the drawbacks of being the "second lab."

7. A PERFECT MATCH
Livermore, the Navy, and the Polaris Missile Warhead
1956-1957

Livermore's development of Polaris has been described as the laboratory's "coming of age." The program was a technical success, although as we shall see not without problems. Polaris carried the first modern, high-yield, low-weight nuclear warhead, and became an important element of the strategic stockpile. The highly integrated warhead and reentry vehicle required close cooperation between the laboratory and the Navy, establishing a new way of doing business for both. Livermore's bid for the Polaris warhead assignment illustrates its strategies for dealing with its "second lab" role.

Livermore Comes of Age

The atomic bomb and air power dominated U.S. postwar military planning, while missile development languished. The armed services competed over access to nuclear technology and control of the strategic bombardment mission. This competition reached a critical turning point in the late 1940s debate between the Air Force and Navy over development of the B-36 bomber and Navy Super Carrier. The point of contention was which service and which system would win the means for delivering nuclear weapons. Limited bombing accuracy of the B-36 fit the Air Force's strategy of threatening massive force against urban areas to deter Soviet aggression in times of war. The Navy challenged the strategy's effectiveness and morality, proposing instead that the more accurate carrier-based aircraft be used to deliver nuclear weapons to military targets. The Air Force won the debate, as attested by appropriations for procurement of the new B-36 bomber, although the Navy was not thereby excluded from a role in strategic warfare.[1]

Interest in intercontinental ballistic missiles (ICBM) revived in the early 1950s in the wake of intelligence reports the Soviets had an ICBM test program. Soviet ICBMs would be capable of bypassing U.S. forward defenses to attack the U.S. mainland, even Strategic Air Command bomber bases. The logic of deterrence suggested that Soviet ICBM capability would inhibit the United States from responding to aggression against its allies since this would put American cities and the U.S. bomber force at risk. The answer seemed to be to develop a U.S. ICBM capability. U.S. ballistic missile programs were revived and ultimately placed on accelerated development schedules. Both intermediate (IRBM) as well as inter-continental missile programs were put in place to ensure early deployment, while expansion of the bomber force continued.

The prospect that missiles might soon comprise the largest share of the United States strategic force stimulated interservice competition for control of one or more of the anticipated ballistic missile development programs. Although the Air Force had won the long-range bombing mission, responsibility for ballistic missiles was not yet determined. By 1955, however, the Air Force had won approval for the Atlas ICBM and its backup, Titan. The Air Force would also develop the intermediate range Thor missile, the Army the Jupiter IRBM. The Navy was the last of the three services to propose a ballistic missile program. Formulation of a united Navy position was delayed by jurisdictional disputes, conflicting technical advice, and reservations regarding nuclear weapons as instruments of mass destruction, as discussed by Harvey Sapolsky in his seminal analysis of the Polaris Fleet Ballistic Missile (FBM) program.[2] Eisenhower had entered office promising fiscal restraint, although the federal budget continued to rise due in part to increased defense spending. Administration policy emphasized nuclear weapons over conventional forces for meeting national security needs. By the mid-1950s, however, the administration sought constraints even on the former. So when the Navy proposed a fifth ballistic missile program in addition to the four already approved, the president was unwilling to approve it. The Navy with some reluctance thus made arrangements to cooperate with the Army in developing Jupiter. A joint Navy-Army group was established in November 1955, its purpose to adapt Jupiter for shipboard and possible submarine use. The Navy Fleet Ballistic Missile Office, also known as the

1. The discussion of the origins of the Navy's Polaris FBM is largely based on Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge: Harvard Univ. Press, 1972), pp. 1-13.

2. In addition to Sapolsky, see also Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York: Columbia Univ. Press, 1976); Jacob Neufeld, *Ballistic Missiles in the United States Air Force*, 1945-1960 (Washington, D.C.: OAFH, 1990); Zuoye Wang, "American Science and the Cold War: The Rise of the U.S. President's Science Advisory Committee," Ph.D. diss., Univ. of California at Santa Barbara, Dec. 1994, chap. 5, for PSAC's role in the development of ballistic missiles.

Special Projects Office (SPO) of the Navy Bureau of Ordnance, was established to manage the Navy's FBM program and Navy-Army interactions.[3]

The four missiles under development, including Jupiter, were liquid-fueled, as were all missiles, a major drawback from the Navy's perspective. Fire hazards posed by placing liquid-fueled missiles on board ship were substantial. Missile size and other features were also problematic given the constraints of submarine deployment. Senior naval officers thus informed their Army counterparts at an early Jupiter project meeting the Navy would switch to a solid-fueled missile as soon as advances in technology permitted. The Navy asked approval from the Office of the Secretary of Defense Ballistic Missile Committee, charged with coordinating military missile projects, to explore the feasibility of solid-fueled ballistic missiles. Already charged with development of the Regulus II and Triton cruise missiles, it was turned down. Failure did not end Navy interest. Senior naval officers acted on their own initiative and without formal authorization to begin a new Navy FBM program. Assisted by Aerojet-General Corporation and Lockheed Missile and Space Division the Navy began Jupiter S in early 1956. Jupiter S would cluster six solid-fueled rockets in a first stage with a single rocket in a second stage to carry a nuclear warhead. The "Six Plus One" design soon gained approval of the Secretary of the Navy as the official interim FBM objective.[4]

The Navy still needed Air Force support to make headway and by February 1956 had persuaded the Air Force its goals complemented rather than conflicted with its own. Jupiter S was soon authorized by DOD as a backup to the Army's liquid-fueled Jupiter missile, but had little funding. One question still remained: Was Jupiter S worth building? It was an 80-ton "monster," as Sapolsky dubbed it, 44 feet long with a 120-inch diameter. Four such missiles were to be carried on an 8,500-ton submarine in a housing attached to its sail. Though unwieldy, Jupiter S nevertheless appeared to offer the best prospects for initiating the Navy's FBM program. The search for a successor began almost immediately. System improvement studies showed component weight reductions would permit development of a lighter missile with performance characteristics equivalent to that of Jupiter S. But Navy officials made the political judgment that changes might jeopardize the entire program. The Special Projects Office continued cooperating with the Army, planning deployment of a liquid-fueled Jupiter on board surface ship in 1958.

3. Herbert F. York, *Race to Oblivion: A Participant's View of the Arms Race* (New York: Simon & Schuster 1970), p. 100; Sapolsky, *Polaris System Development*, pp. 22-23.

4. Ibid., pp. 22-26.

The solid-fueled Jupiter S was not expected until 1965.[5] DOD asked the AEC in April 1956 to develop a nuclear warhead for the liquid-fueled Jupiter.[6]

Project Nobska

In summer 1956 the Navy sponsored a scientific study on antisubmarine warfare which has been credited with changing the course of the Navy's Fleet Ballistic Missile program. Summer studies had become a common means for military organizations to enlist academic experts in thinking about problems related to national security.[7] Sponsored by the Office of Naval Research and organized by the Committee on Undersea Warfare of the National Academy of Sciences, the Nobska study covered topics ranging from submarine detection to navigation to continental defense.[8] Attendees at Project Nobska, named for the Nobsque lighthouse that marked the Massachusetts coast at Woods Hole where it stood, included Navy officers and civilians, industry executives and scientists, and academic scientists knowledgeable in nuclear propulsion and weapons.[9] The scientists were invited to help the Navy learn about new technologies that might contribute to Navy goals. Nobska participants learned, for example, about possibilities for a new lighter weight missile from Frank E. Bothwell, in charge of missile targeting and design work at the Naval Ordnance Test Station.[10]

Edward Teller was also invited. His statements about trends in nuclear warhead technology are reported to have electrified conference participants. Nuclear warheads in much smaller sizes and yields than previously available would soon be within the capabilities of the nuclear design laboratories. "Why," he asked, design "a 1965 weapon system with 1958 technology?"[11] Instead,

5. Ibid., pp. 26-28.

6. C. C. Furnas to Lewis L. Strauss thru Alfred D. Starbird, 6 April 1956.

7. J. R. Marvin and F. J. Weyl, "The Summer Study," *Naval Research Review* 20 (August 1966): 1-7, 24-28.

8. NAS, Comm. on Undersea Warfare, *Project Nobska: The Implications of Advanced Design and Undersea Warfare. Final Report*, vol. 1, *Assumptions, Conclusions and Recommendations*; and vol. 2, *Technical and Systems Studies* (Washington, D.C., 1 Dec. 1956-1 March 1957); NAS, Comm. on Undersea Warfare, *Review of Project Nobska, 5-9 Aug. 1959* (Washington, D.C., 1959).

9. Gary E. Weir, "The Next Time You Go for a Walk . . . Project Nobska, 1956," *Pull Together: Newsletter of the Naval Historical Foundation and the Naval Historical Center* 33 (Spring/Summer 1994): 5-8.

10. Sapolsky, *Polaris System Development*, pp. 28-29.

11. Robert A. Fuhrman, "Fleet Ballistic Missile System: Polaris to Trident," The Von Karman Lecture for 1978, AIAA 14th Annual Meeting, Washington, D.C., Feb. 1978, p. 4.

he held out the promise of a megaton thermonuclear warhead in a twelve inch diameter.[12] As of summer of 1956, the approved Navy program for Jupiter S did call for a missile based on 1958 technology for a submarine not scheduled for service until 1965. As Harvey Sapolsky put it, "there was a clear mismatch between the missile and the missile's major platform." In an effort to better match missile and platform, the Nobska panel on Strategic Use of the Underseas considered projected improvements in major FBM subsystem technologies, including the nuclear warhead. Improvement in any single subsystem would have little impact on overall performance, but taken together, the panel projected an extremely effective system by the mid-1960s. The panel recommended the Navy build a solid-fueled ballistic missile weighing between eight and fifteen tons, five to ten times lighter than Jupiter S, with a range of 1,000 to 1,500 miles. In the interim it would carry a relatively low-yield nuclear warhead, with a higher yield weapon ready by the mid-1960s.[13]

Despite concern in some quarters that such a system would interfere with other Navy programs, the Special Projects Office and the Chief of Naval Operations promoted the Nobska panel's recommendation. As Sapolsky pointed out, however, persuading the Department of Defense to approve a new Navy missile development program was no easier in 1956 it had been the previous year. By agreeing in the Joint Chiefs of Staff to Air Force control of land-based IRBMs, the Navy gained crucial Air Force support. Projected savings of $500 million over Jupiter S also helped. By early December 1956 Secretary of Defense Charles E. Wilson had authorized the Navy to initiate development of the Navy missile, now designated Polaris, and to terminate its joint program with the Army.[14]

Historical accounts of the origins of the Polaris program credit Teller's predictions at Nobska with providing the catalyst for the Navy's pursuit and DOD's ultimate approval of the Polaris program. Although advances in other technologies were also critical, including the development of solid fuels, most accounts focus on Teller's predictions at Nobska.[15] York has argued that Livermore scientists, under his tutelage, had long worked on development of light-weight H-bombs. Teller's predictions at Nobska were thus grounded in technical trends and not pulled from thin air. Such work was well under way, according to York, when the Navy "came along,"

12. Edward Teller, transcript of untitled remarks, Symposium on Nuclear Weapons and Weapons Effects, Project Nobska, Woods Hole, Mass., 20 July 1956, p. 4. Teller had said much the same thing the day before, and expressed agreement with John S. Foster of Livermore, who had discussed improvements in small size and yield fission weapons.

13. Sapolsky, *Polaris System Development*, pp. 30-31.

14. Ibid., pp. 33-34.

15. See, e.g., Fuhrman, "Fleet Ballistic Missile System: Polaris to Trident," p. 4.

implying the laboratory had initiated work in this area in the absence of clear military interest.[16] Work was well underway, but what is also clear is that it was responsive to expressions of military interest in small H-bombs. Evaluating this claim requires assessing the state of nuclear weapons development at Livermore and Los Alamos in summer 1956. That in turn requires looking back to 1953 and asking why the laboratory began work on small H-bombs.

The answer is rather straightforward. Chairman of the Military Liaison Committee Robert LeBaron told AEC Chairman Gordon Dean in April 1953 of military interest in small, lightweight thermonuclear weapons. Acquiring weapons with yields of one-quarter to two megatons capable of delivery by high performance fighter-bombers, pilotless aircraft, and guided missiles was already "a major objective" of the Department of Defense.[17] The AEC's Director of Military Application informed the laboratories of DOD interest, urging Los Alamos to continue its work in the area and Livermore to begin.[18]

Teller and John von Neumann had predicted in early 1953 that thermonuclear warheads weighing less than 1,500 pounds with yields of one megaton would soon be possible.[19] By that summer Livermore scientists were investigating H-bombs in narrow diameters and low yields, as discussed in Chapter 5.[20] Although these specific designs were never developed they served to focus Livermore small H-bomb research.[21] The AEC was encouraging even though no formal military requirement for warheads with these characteristics yet existed. Such delivery systems, however, were thought to be "coming along."[22] In fact, several studies underway all urged faster development of strategic missiles.[23]

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16. Herbert F. York, *Making Weapons, Talking Peace: A Physicist's Odyssey from Hiroshima to Geneva* (New York: Basic Books, 1987), p. 76.

17. Robert LeBaron to Gordon Dean, 23 April 1953.

18. Kenneth E. Fields to E. O. Lawrence, draft, n.d. This letter was not sent, but a cover note indicates that the same issues would be discussed in a conference with the laboratories.

19. Beard, *Developing the ICBM*, p. 154, n. 1.

20. Herbert F. York to Kenneth E. Fields, 21 Sept. 1953; Minutes of PW Technical Program Meeting, 11 Sept. 1953.

21. Herbert F. York with Harold Brown to Kenneth E. Fields, 27 Nov. 1953.

22. Minutes of PW Technical Program Meeting, 17 Feb. 1954.

23. Beard, *Developing the ICBM*. See entries for the Strategic Missiles Evaluation Committee, von Neumann Committee, and RAND study.

York told the Joint Committee on Atomic Energy in May 1955 that Livermore had initiated theoretical studies of thermonuclear weapons in "very small" sizes and yields.[24] These were even smaller than those contemplated for the Class D weapons, the light-weight H-bombs being developed for Air Force fighter-bombers and the Navy's Regulus missile. Both laboratories had conducted tests in 1955 that would contribute to the development of "small" Class D H-bombs. The Army was interested in even smaller H-bombs for tactical applications.[25] Livermore and Army representatives discussed possibilities for a small one-megaton warhead as early as September 1953. The AEC had sought to keep these discussions limited to the exchange of information since rockets capable of delivering such a warhead were not yet available. By May 1955, however, prospects for the rocket appeared more promising.[26] Army representatives again met with Livermore scientists in December 1955 to discuss using a Livermore warhead for the Army Sergeant missile, a successor to Corporal. The technical characteristics of the Livermore warhead were similar to those of what eventually became the Polaris A-1 missile.[27] By January 1956, the Livermore small H-bomb program emphasis had shifted. Modifications were introduced to make the design more relevant for tactical bombs and intercontinental ballistic missiles. These new applications called for a larger yield warhead than originally contemplated for Army purposes, though still smaller than the Class D weapons developed for the Air Force and Navy.[28]

The Redwing nuclear test series was underway by the time of the 1956 Project Nobska summer study. In the planning stages for over a year, one of its principal objectives was to test high-yield, relatively light-weight warhead prototypes for ballistic missiles. The growing priority of the ballistic missile program gave the Livermore small H-bomb program a boost. Though not initially authorized for Redwing, Livermore's small H-bomb was finally tested in July. The results showed a high-yield warhead could be built within the payload capacity of an ICBM. Los Alamos also tested a small H-bomb in Redwing.

A few days before Teller's mid-July 1956 remarks at Nobska, York had provided the AEC Livermore's estimates for the availability of a light-weight one-megaton yield warhead. York

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24. JCAE, Subcomm. on Military Application, Executive Session, Meeting No. 84-1-3-MA, 3 May 1955, pp. 38-39.

25. Ibid., pp. 24-25. This discussion centered on the potential tactical uses of small clean H-bombs.

26. Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), p. 198.

27. Jack L. Armstrong to Chief of Ordnance attn: Charles Reinoldi, 9 Dec. 1955. The Livermore device discussed with the Army was expected to lead to the design of a relatively light weight thermonuclear weapon, with a 13-14 inch diameter, and a yield of 200-300 kilotons. Los Alamos ultimately developed the warhead for the Sergeant missiles.

28. Harold Brown to distrib., "Report on Flute for the Pre-Mortem Group," 13 Jan. 1956.

thought Livermore might achieve as much as 2-3 megaton yields in the 1,500-pound class within the next few years.[29] Higher yield-to-weight or yield-to-volume improvements could be achieved in part by using different materials in both primary and secondary. Los Alamos scientists had considered such techniques, but were less liberal in proposing the use of expensive materials, underscoring the different philosophies of the two design laboratories.[30]

Were a suitable lightweight warhead available, the solid-fueled FBM could be considerably reduced in size, helping the Navy make its case for an accelerated and independent missile capability. At the conclusion of the Nobska summer study, the Navy requested AEC confirmation of the feasibility of the nuclear warhead projected by Teller.[31] York soon responded that such a warhead might be available by 1962 or 1963. Written marginalia on a copy of the letter distributed to Los Alamos reflects the more conservative outlook of Los Alamos scientists. One comment stated that "Material in newspapers like this generally carries in fine print at the bottom (Advt.)" Another says, "There is a popular song, which goes, 'Did you ever see a dream walking?' Well, York did." Another simply says "WOW!"[32] Bradbury derided DOD's appetite for optimistic technical forecasts and Livermore's propensity for making them, although he did not name the laboratory directly. As Bradbury put it, DOD greeted "almost any proposal connected with atomic weapons [with enthusiasm] . . . particularly if it might require a whole new weapon system. . . . The likelihood or imminence of success in achieving the described objective is not a real factor . . . but only the statement by someone with a Ph.D. in Physics that 'it might be possible.'" Bradbury asked if the AEC should not be more careful in introducing "gleam-in-the-eye" studies into DOD circles whereby "a legitimate research program [would be converted] into a dubious development program."[33] Livermore's technical optimism was viewed with more favor in other quarters. An AEC staff member later commented on Livermore's "typically enthusiastic

29. Minutes of 50th GAC Meeting, Washington, D.C., 16-18 July 1956, p. 2. Oralloy, or highly enriched uranium, stands for Oak Ridge alloy, after the facility in Tennessee where it was fabricated.

30. Norris E. Bradbury to Alfred D. Starbird, 26 Jan. 1956; Franck C. Di Luzio to Starbird, 3 July 1957.

31. Fuhrman, "Fleet Ballistic Missile System," p. 4.

32. Herbert F. York to Alfred D. Starbird, 5 Nov. 1956. A Navy chronology of the FBM program shows the AEC in Sept. 1956 estimating that a nuclear warhead could be available for a small ICBM by 1965, perhaps as early as 1963. See SSPO, *FBM Facts/Chronology: Polaris, Poseidon, Trident* (Washington, D.C.: Dept. of the Navy, 1986); Fuhrman, "Fleet Ballistic Missile System," p. 4.

33. Norris E. Bradbury to Willard F. Libby, 15 June 1959. Bradbury's remarks strongly suggest he is talking about Livermore, though he does not mention it by name.

approach" at a Los Alamos sponsored conference. Livermore attendees presented a "broader spectrum of ideas, and in more depth and detail," than any other.[34]

Los Alamos had less need or incentive to make optimistic technical projections. Not looking for new customers because of its large workload, Los Alamos' had different priorities. Air Force and Army strategic missiles had less severe weight and size constraints than Navy systems, allowing more flexibility in nuclear weapons design for achieving desired yields. At the same time, Los Alamos scientists felt compelled to respond to Livermore technical claims. A yield-to-weight competition of sorts was in full swing by the summer 1957, contrasting Livermore's optimism about future technical possibilities with Los Alamos' more conservative outlook. Harold Agnew of Los Alamos circulated an internal memo comparing the weapons accomplishments of the two laboratories and forecasting future technical potential. The information was supposed to enable Los Alamos scientists to speak with authority and confidence regarding Los Alamos yield-to-weight projections. As far as "sticking my own neck out," Agnew predicted yield-to-weight ratios in 1,500-pound weapons three times better than then feasible and 50 percent better than hoped for in the new "prime" weapon series.[35] A similar exercise at Livermore offered yield-to-weight ratio predictions double those of Los Alamos.[36]

Strictures against laboratory duplication meant Livermore investigated the wants and needs of military customers underserved by Los Alamos, like the Army in the 1950s. The physical parameters and technical constraints of their delivery systems differed from Los Alamos'. This meant differences in technical programs and therefore to different technical achievements. Los Alamos had tested small H-bombs comparable to Livermore's in Redwing. Their estimates of what the future held, however, were more conservative. When asked at Nobska to comment on future yield possibilities for small size and weight H-bombs, for example, Los Alamos scientists predicted yields only half of Teller's one megaton.[37] These estimates were not very different from Livermore's in terms of the scientific uncertainties involved. Furthermore, they were not enough to concern the Navy, which principally wanted assurances high-yield small size warheads were possible, caring less about whether or not they were actually be achieved. Both laboratories could probably have developed warheads with the desired characteristics. Livermore scientists were more willing to make optimistic predictions and do their best to achieve them. As one Los

34. Richard D. Wolfe, "Briefing for the Commissioners on the Results of the Weapons Symposium held at Los Alamos Scientific Laboratory Nov. 7-8, 1963," 7 Nov. 1963, p. 39.

35. Harold M. Agnew to distrib., 3 June 1957.

36. A-Division, Harold Brown, Carl A. Haussmann to distrib., 16 July 1957.

37. Edward Teller, "Edward Teller on LLL's Role in Nuclear Weapons Design," *Research Monthly* (Sept. 1977): 1-11, at 3.

Alamos scientist recalls, Livermore "did a lot more talking than we did."[38] The talk had important consequences, however. Teller's predictions at Nobska were as important for their technical content as they were for their impact on Navy thinking and their political ramifications.

"Splitting the Pie"

Sapolsky has argued that it was not so much Teller's technical predictions at Nobska that mattered most to the Navy as it was the ability to bring this information to bear in the political arena to gain support for Polaris.[39] The ultimate yield of the warhead was not as important to the Navy as was the ability to say with authority—conferred by the statements of Livermore scientists and backed by the AEC—that the desired yield was possible. The Navy's fervent desire for Polaris thus meant "love at first sight" between Livermore and the Navy as one Livermore scientist recalls.[40]

DOD approved the Polaris program in December 1956, officially requesting development of the nuclear warhead in January 1957. Because one megaton was a political necessity, not an operational requirement, the Navy was prepared to accept an interim yield on the order of several hundred kilotons. The missile would nonetheless be designed around the one-megaton warhead expected to be ready by 1965.[41] No decision had yet been made about which laboratory would develop the warhead. Bradbury put his thoughts on the subject in writing in February 1957. Though copies of most letters to the AEC were generally sent to all concerned, Bradbury sent this one confidentially, with no copy to Livermore. He followed a familiar pattern, covertly addressing his concerns about Livermore to the AEC, while seeking accommodation and cooperation with Livermore at the working level.[42]

Livermore—which had fewer programmatic responsibilities than Los Alamos—was freer to pursue innovative research. Six of the nine weapons due for stockpiling between 1958 and 1960 were assigned to Los Alamos. The disparity between responsibilities and resource allocation were evident in plans for the 1957 Plumbbob test series. There was a "vast difference" in the type of experiments proposed by the laboratories. To meet its programmatic responsibilities, Los Alamos planned "nose-to-the-grindstone" experiments related to specific weapon designs. Not

38. Interview with Max Roy, Los Alamos, 5 April 1991.

39. Sapolsky, *Polaris System Development*, pp. 244-245.

40. Interview with Jack W. Rosengren, LLNL, 4 March 1991.

41. C. C. Furnas to Lewis L. Strauss, 9 Jan. 1957.

42. Norris E. Bradbury to Alfred D. Starbird, 20 Feb. 1957.

tied down with weapons development assignments, Livermore could devote a smaller fraction of its shots to programmatic responsibilities. An even split of resources would leave Los Alamos working to meet its immediate responsibilities. Livermore, on the other hand, had considerable freedom to explore broadly because its programmatic commitments were relatively small. Livermore, in short, was "having fun."[43]

Bradbury suggested laboratory resources be allocated to reflect programmatic responsibilities. An even division between the laboratories risked "seriously curbing" exploratory effort at the laboratory with the larger programmatic commitment. Bradbury explained this was the reason for his "hanging back on Polaris and letting UCRL make what promises they wish without LASL complaint." He urged more specific weapon development responsibilities for Livermore to alleviate the problem.[44] Bradbury proposed that Livermore and the Navy "pick a size for Polaris" and "let . . . [Livermore] . . . go ahead and make the best warhead they can for it."[45]

The AEC assigned Livermore the lead in coordinating the warhead feasibility study but postponed the decision about which laboratory would develop the warhead. The Navy agreed.[46] At this point, the actual assignment became a formality. The feasibility study concluded in July 1957 that a thermonuclear warhead could be developed to meet the interim requirement for a several hundred kiloton warhead. DOD requested the AEC proceed with development engineering.[47] The AEC asked Livermore to develop the Polaris warhead.[48]

The Soviet launching of Sputnik on 4 October 1957 increased the Polaris program priority.[49] The Secretary of Defense informed the AEC that development was a high DOD priority. This meant the interim low yield warhead should be available no later than October 1960, a one-megaton warhead ready as soon thereafter as possible.[50] Livermore's accelerated Polaris program would now include tests in the 1958 Hardtack series.[51] This action only added to growing milita

43. Ibid.

44. Ibid.

45. Norris E. Bradbury to AEC, 28 Feb. 1957, as cited in ALOO to Alfred D. Starbird, 11/2045Z March 1957.

46. Alfred D. Starbird to Kenner F. Hertford, 19 March 1957.

47. J. B. Macauley to Lewis L. Strauss, 3 July 1957.

48. Alfred D. Starbird to Harold A. Fidler, 15 July 1957.

49. Sapolsky, *Polaris System Development*, p. 9.

50. Donald A. Quarles to Lewis L. Strauss, n.d. (received 12 Dec. 1957).

51. Alfred D. Starbird to Herbert F. York, "Polaris in Hardtack," 10 Dec. 1957.

ry demands placed on the nuclear design laboratories and AEC production complex. In sheer numbers, the late 1950s were a period of peak demand. Total nuclear weapons in engineering development through production for each year between 1951 and 1961 represented almost half the total for all years combined through 1992 (Figure 4). The heavy engineering production and development workload had the greatest impact on the Sandia laboratories, but the design laboratories were not immune. In 1957 it was not evident when and if the upward trend would end. The largest number of individual warhead types ever to enter stockpile was in 1958, when the laboratories provided five new designs for the armed services (Figure 1). The total designed was thirteen, but a number were cancelled before reaching stockpile.

By November 1957 Bradbury had become so concerned about the increased workload that he made a radical proposal. He sought York's cooperation in urging DOD that "missiles . . . adapt themselves to the warheads and that there not . . . be a special warhead for each missile."[52] Bradbury's suggestion might strike later observers of the laboratories as unusual. Unlike the later periods, however, in the 1950s the laboratories were deluged with work. It was nevertheless rejected, requiring as it would have a greater commitment of resources by DOD. As one Livermore scientist recalls, "commonality . . . never sold well in the services."[53]

Increased laboratory cooperation was a solution for the growing workload. York contacted Bradbury in August 1957 to suggest the laboratory directors and senior staff hold a joint meeting to resolve "who does what."[54] The AEC agreed, recommending that representatives from the Division of Military Application, Los Alamos, Livermore, and others, meet at least annually to discuss scheduling and responsibilities. One such meeting had already taken place, in August 1956, as discussed in Chapter 6. The proposal would make such meetings routine.[55] Director of Military Application Alfred D. Starbird urged a system be developed to ensure that future nuclear warhead development work be distributed "reasonably and equitably" between the laboratories.[56] Laboratory cooperation had advantages from the AEC perspective. It may have helped reduce staff workload. More importantly, it helped regularize laboratory management and communicated professionalism and control. Agreement between the laboratory directors conveyed the sense to the Joint Committee on Atomic Energy, DOD, and the armed services that

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52. Alfred D. Starbird to Kenner F. Hertford, 4 Nov. 1957.

53. Interview with Carl A. Haussmann, LLNL, 4 Oct. 1990.

54. Herbert F. York to Norris E. Bradbury, 23 August 1957.

55. Harold A. Fidler to Alfred D. Starbird, "Development Status Review," 27 Nov. 1957.

56. Alfred D. Starbird to Kenner F. Hertford, 10 April 1958.

the weapon program was proceeding in an orderly fashion and on solid footing. Planning ahead avoided starts and stops in the program. An ongoing planning process avoided program disruption should DOD decide against development of a particular system.

Starbird judged it too early to make assignments for all upcoming systems in spring 1958, but suggested dividing the list into three groups, one comprising systems primarily of interest to Los Alamos, the second to Livermore, the third to both. Grouping warheads this way would facilitate "reasonable, long-range planning of the laboratories' workloads." Warheads of interest to only one laboratory could be reserved for that laboratory, while warheads of interest to both "would be spotlighted." The laboratories could recommend "the logical assignment of nuclear development responsibility" after DOD's formal requests. Starbird asked the manager at Albuquerque to sponsor a joint Livermore-Los Alamos meeting. The goal was to determine laboratory preferences in 28 upcoming weapons projects.[57] The directors of Livermore and Los Alamos and senior staff met "on neutral ground" in Los Angeles in mid-December 1958 "to split up the pie" as one Livermore scientist wrote.[58] The division of responsibilities was made, in part, on the basis of nuclear tests already conducted, because a nuclear test moratorium had gone into effect on 1 November 1958. (The moratorium is discussed in the next chapter).

Implicitly, the proposed list divided weapon development responsibilities according to warhead weight. Los Alamos had taken the 6000-, 1500-, and 350-pound classes, Livermore the 3000-, 600-, and 200-pound classes. This breakdown "was not made explicit," though the Livermore representative at the meeting felt it was generally recognized. Livermore's development of the Subroc warhead, for example, acknowledged Livermore's "claim in the field."[59] This explanation appears to have been a post hoc rationalization, as argued below.

Military Preferences

The principal decision made at the Los Angeles meeting was to "trade" the Livermore W-51 Davy Crockett warhead for the Titan I warhead.[60] Although Livermore had continued follow-on studies for Titan, Los Alamos had kept responsibility for development of the first Titan warhead. Teller and Bradbury explained the trade was made with the design loads of the two laboratories in mind and with the objective of maintaining a broad spectrum of weapon interests and respon-

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57. Alfred D. Starbird to Kenner F. Hertford, 10 April 1958.

58. Wilson J. Frank to Harold Brown, 16 Dec. 1958.

59. Ibid.

60. See SNL, *8100 Directorate: The First Thirty Years* (Livermore, c. 1986), pp. vii/viii, for a brief discussion.

sibilities across both institutions.[61] The Air Force was not pleased. Army and Navy confidence in Livermore was not much better, as reflected in an AEC memo listing the "advantages" and "disadvantages" of various options. It found "low confidence" in virtually every Livermore program. Concerned about the effective utilization of laboratory resources, however, the AEC sympathized with Los Alamos' view that the laboratory workload was "unfairly" distributed, permitting Livermore more leeway for new development.[62] Failure to remove a major program from Los Alamos in exchange for taking over the Davy Crockett warhead from Livermore would seriously overload Los Alamos, perhaps resulting in slippage of operational availability dates for other systems. Such slippage would be made "in accordance with established DOD priorities," implying that other high priority Los Alamos weapons programs might suffer delays.[63]

Starbird endorsed the suggested division of weapon development responsibilities but realized the Titan trade could not forced on the Air Force. Instead, the AEC would need to persuade DOD of the merits of the proposal. The case could be made if the AEC got the "right answers from the Labs," and Starbird asked the laboratory directors to provide further justification for their recommendation.[64] Teller found it "rather surprising" the Air Force would find a direct advantage or disadvantage to a specific DOD program in the laboratory recommendation. Program allocations were based on considerable discussion among the senior staff members of both laboratories. The outcome represented, in their judgment, an optimal workload distribution. Either laboratory was "equally capable" of meeting the military requirements for Titan. The proposed shift was intended to balance laboratory workloads, best enabling the AEC to produce the weapons DOD desired.[65] Bradbury focused on the importance to both laboratories of keeping their skills in thermonuclear and fission weapons research. A distribution by alternating weight classes seemed a reasonable approach. Bradbury reminded Starbird Titan had initially been a Livermore responsibility, although it was transferred to Los Alamos in an effort to better balance laboratory workloads. The Titan warhead weight class, as Bradbury put it, could properly be said to "belong" to Livermore.[66]

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61. Norris E. Bradbury and Edward Teller to Alfred D. Starbird, 15/2355Z Dec. 1958.

62. Col. Brown to Alfred D. Starbird, 29 Dec. 1958.

63. Alfred D. Starbird to Edward Teller and Norris E. Bradbury, 24 Dec. 1958.

64. Brown to Starbird, 29 Dec. 1958.

65. Edward Teller to Alfred D. Starbird, 9/1617Z Jan. 1959.

66. Norris E. Bradbury to Alfred D. Starbird, 2 Jan. 1959.

Laboratory justifications of the "pie split" skirted the issue of military preferences and priorities, an important though unacknowledged factor in weapon assignments. The Air Force considered Titan more important than Davy Crockett, and trusted Los Alamos more than Livermore to produce the warhead. The Air Force was thus reluctant to accept the exchange, "since they could see no direct advantage to the 38 [W-38 Titan warhead development] program."[67] One factor in Air Force considerations was that the Livermore design would require more nuclear material, raising overall Air Force nuclear materials requirements. This was not a welcome prospect at a time when materials were still considered scarce.[68] The Army had lost interest in Livermore's Davy Crockett warhead after test results showed a disappointingly low yield in summer 1958.[69] In November, one month prior to the Los Angeles meeting, the Army requested reopening the Davy Crockett feasibility study to consider a Los Alamos warhead, which it preferred.[70]

Successful W-38 Titan warhead tests in Hardtack helped tip the balance in favor of Livermore's winning back the Titan warhead.[71] At the same time, Davy Crockett had risen in the DOD hierarchy of priorities. In June 1958 the Army requested it be given equal priority with ballistic missile development.[72] When the AEC reassigned Davy Crockett to Los Alamos in January 1959, Livermore regained responsibility for W-38 Titan I development.[73] Deployed in 1961, Titan I was retired in 1965. The Davy Crockett warhead, renamed W-54, was also carried on the Falcon missile, and as such remained in the stockpile until 1989 (Table 1).

Davy Crockett had comprised a large fraction of Livermore's weapon program and caused major staff allocation difficulties.[74] Two explanations for what happened circulated around the laboratory, reflecting the fact that it was a loss to those charged with the Davy Crockett warhead development and a gain to those charged with Titan's. The first explanation had Teller (then director of the laboratory) fighting to keep the Davy Crockett but that Los Alamos had "insisted on having it." The second explanation had Livermore desirous of developing the Titan warhead

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67. Alfred D. Starbird to Norris Bradbury, Edward Teller and others, 24 Dec. 1958.

68. Col. Brown to Alfred D. Starbird, 29 Dec. 1958.

69. Alfred D. Starbird to Herbert B. Loper, "Davy Crockett," 10 July 1958, and added typed notes.

70. Alfred D. Starbird to Jane H. Hall, 28 Nov. 1958; Starbird to John A. McCone, 1 Dec. 1958.

71. Discussion with Jack Rosengren, LLNL, 16 June 1995.

72. Herbert B. Loper to Alfred D. Starbird, 20 June 1958.

73. Alfred D. Starbird to Kenner F. Hertford, 15 Jan. 1959.

74. Wilson J. Frank to Harold Brown, 16 Dec. 1958.

and Los Alamos insisting on a trade for it.[75] Laboratory leaders lacked the freedom of choice either of these explanations implies. Davy Crockett's increased priority reflected changes resulting from the New Look. Tactical atomic weapons were gaining in priority, elevating the Army's leverage. The "decision" by the laboratories to transfer responsibility for Davy Crockett to Los Alamos might more properly be described as recognition of, and acquiescence to, the Army's preference for the Los Alamos warhead.[76]

Livermore met Teller's Nobska forecast, though not in time for the first W-47 Polaris warhead deployed on the A-1 missile in 1960. Livermore's quest for a thermonuclear warhead with a high yield-to-weight ratio had come to fruition in 1958 when Livermore tested a new class of secondary design, which for classification purposes this study will refer to as the L-3. The 1958-1961 nuclear test moratorium intervened, so warheads incorporating L-3 secondaries were not deployed until 1963, on the Minuteman II and Polaris A-2 missiles.[77] L-3 designs proved long-lasting, remaining in use for many years. Their principal advantage lies in high yield-to-weight and yield-to-volume characteristics. L-3s were particularly well suited to the size and weight constraints of multiple reentry vehicles (MRVs) and later MIRVs (multiple independently targetable reentry vehicles). Livermore's Polaris A-3 warhead was the first MRV deployed in the stockpile, and Livermore also developed the first MIRV warheads.

L-3s grew out of the structure of incentives and constraints discussed in this dissertation, which helped govern Livermore from its earliest days and structured the two-laboratory system. L-3s combined old and new, just as had the Manticore designs. One important L-3 design feature, for example, was first tested by Los Alamos in 1956. The concept involved a configuration more difficult to model than other Los Alamos designs with the computational power then available. Although it worked, Los Alamos successfully pursued the more conservative approach.[78] Pressure to get workable H-bomb designs into the stockpile encouraged Los Alamos scientists on this path despite the potential advantages of the more daring design. Livermore revived the idea two years later in L-3s, an example of what Livermore director John Foster later described as his laboratory's role in pursuing ideas initially bypassed in the "frantic race to get something in [the stockpile]."[79]

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75. Lawrence S. Germain, *A Brief History of the First Efforts of the Livermore Small-Weapons Program* (Los Alamos: CNSS, 2 Jan. 1991), p. 24.

76. Alfred D. Starbird to MLC Chairman, 15 Jan. 1959. See note typed on the back of the memo for Army interest in Los Alamos device.

77. Ibid.

78. Interview with Carson Mark, Los Alamos, 3 April 1991.

79. "Briefing for DMA Personnel by LRL on Their Research and Development Work, November 4,

L-3s achieved their high yield-to-weight ratios by using oralloy, or enriched uranium, in a new way. Greater yields in weapons of comparable weights was the outcome.[80] Scientists at both laboratories had long recognized H-bomb yields could be increased by using oralloy, but it was an expensive material. The reluctance of Los Alamos scientists to make more liberal use stemmed from the postwar need to make limited amounts of fissile material go as far as possible. Livermore scientists joined the nuclear weapons development enterprise at a time when materials were becoming more plentiful, and this shaped their outlook.[81]

Small-diameter primaries were critical for designing H-bombs that would fit the narrow confines of ballistic missiles. Livermore's experience designing small fission weapons for the Army contributed to the necessary skills and knowledge. Among these was the ability to devise computer models to calculate the physics and physical phenomenon of a nuclear explosion. The geometry of these complex phenomena can be simplified into one-, two-, or three-dimensional problems. The more dimensions, for a given resolution, the more computing power required.[82]

The computing power available in the 1940s and early 1950s confined computer code development to one dimension. The required simplification made complex problems difficult to model accurately. Conversely, it encouraged designs calculable with relatively simple codes. The extensive computing power required for multi-dimensional problems was not available to Los Alamos in its early years. The solution was to rely as much as possible on theoretical calculation, a hallmark of the early Los Alamos approach to nuclear design. Alternatively, experimental data could also help fill the gaps, but at great expensive and effort, particularly when added to the already full 1950s test program.[83] The Manticore fission weapon designs explored by Livermore had challenged computational tools available at the time. The calculational demands of these systems, in turn, helped propel Livermore's development of two-dimensional codes. At the same time, the growing availability of the codes allowed progress in weapon design.

Building on several years' work that began with exploration of small fission designs in 1955, Livermore completed a series of tests in the 1957 Plumbbob series that resulted in the Robin family of primaries. Robins incorporated important innovations. The family comprised several

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1959," 27 Nov. 1959 draft, John S. Foster comments, p. 1.

80. Samuel Glasstone and Philip J. Dolan, eds., *The Effects of Nuclear Weapons* (Washington, D.C.: DOD and ERDA, 1977), p. 22.

81. Discussions at LLNL and LANL.

82. DOE, *The Need for Supercomputers in Nuclear Weapons Design* (Washington, D.C., Jan. 1986), especially pp. 24-25.

83. Discussions at LLNL and LANL.

designs with certain features in common. They were well suited to the narrow confines of strategic missiles, having benefitted from technical advances growing out of Livermore's earlier atomic artillery work. A new Robin and L-3 could be designed together to make a thermonuclear weapon.[84] Robins soon became a standard feature of the stockpile, deployed in both Livermore and Los Alamos systems for many years. Bradbury quickly recognized the achievement they represented and considered the Livermore primaries superior to comparable Los Alamos systems.[85]

Robins, like L-3s, grew out of the structure of laboratory competition. The mandate to avoid program duplication, and the search for underserved military customers were important factors in Livermore's exploration of small fission devices. The Robin lineage can be traced to Livermore's earliest technical explorations. Robins, of course, bear little direct resemblance to earlier systems, given the twists and turns in technical developments. Their roots in older designs are evident, however, as are the traces of the two-laboratory system displayed in the design's technical characteristics.

Los Alamos did not invent secondaries with features comparable to L-3s because the laboratory was more "conservative" than Livermore, as one Los Alamos scientist put it.[86] The same might be said of Robin primaries. Los Alamos' conservatism resulted from the two-laboratory incentive structure. Each laboratory was shaped by the kinds of problems they set out to solve. They labored under different size, weight, and yield constraints and requirements, a consequence of the military systems each developed. The technical parameters of the weapons thus influenced the direction of each laboratory's research program and the character of their innovations.

Polaris Problems

L-3 was a Livermore success story. So was Livermore's development of the Polaris nuclear warhead. A complete rendering of the Polaris story must consider the problems that arose after its deployment. These were the outcome, in part, of how Livermore handled the problems posed by the nuclear test moratorium which began on 31 October 1958. The impending moratorium generated one of the most extensive nuclear test series conducted before or since. Uncertain if nuclear testing would again be allowed, laboratory scientists conducted seventy-two tests between April and October 1958. Although the Hardtack test series had several objectives, ascertaining the safety of the weapons to be deployed in the stockpile was a high priority for both laboratories.

84. The first L-3 was tested using a Los Alamos-designed primary.

85. Norris E. Bradbury to Carson Mark, Duncan MacDougall, et al., 15 August 1957, LANL Archives.

86. Interview with Del Bergen, LANL, 3 April 1991.

A nuclear weapon is considered "safe" if it does not give a nuclear yield in the event of an accident or terrorist act that detonates the high explosive surrounding the fission fuel. The high explosive is designed to initiate the fission reaction, but should do so only in a certain sequence of events. A fission weapon or primary is considered "one-point safe" if there is a very low probability of achieving nuclear yield should the high explosive in the weapon detonate at any single point.[87] A single-point detonation might occur if, for example, a bullet struck the weapon, or it were hit by a fragment from a nearby explosion. A primary detonated in this fashion might conceivably ignite a thermonuclear explosion since the primary is intended to initiate the nuclear reaction in the secondary.

Achieving nuclear safety thus involves ensuring that a fission weapon, or the primary of a thermonuclear weapon, does not detonate accidentally. There were two principal paths to one-point safety. One method employed mechanical means to prevent unintended nuclear yield. The other method involved making weapons "inherently" one-point safe through nuclear design, not always a simple matter as both laboratories discovered in 1958. Hardtack tests revealed the Polaris warhead primary was not inherently one-point safe, and the problem was not resolved before the start of the test moratorium. The laboratory considered safing the primary with a mechanical device conceived at Livermore and developed in conjunction with Sandia-Livermore. Although some earlier weapons had been mechanically safed, the new device bore little resemblance to previous systems.[88] Livermore scientists, including director Teller, did not have complete confidence in the reliability of mechanical safing. That is why Teller wrote Bradbury in December 1958 to ask that he hold open the option of using a Los Alamos primary for Polaris. Teller would cancel the request as soon as he was certain of the mechanical safing scheme.[89]

Livermore indeed cancelled the request for the Los Alamos primary, with important consequences. A faulty mechanism was later discovered that would prevent nuclear detonation in many of the Polaris missile weapons, much to Navy consternation. A substantial fraction of the Polaris warheads in the stockpile were thus determined in the 1960s to be potential "duds," as discussed in greater detail in Chapter 9.

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87. The definition has changed over time, becoming more restrictive. In its final form, one-point safety of a fission implosion system is defined as a less than one in one million probability of a nuclear yield in excess of four pounds high explosive equivalent in the event of a detonation at any point in the high explosive system. See Sidney D. Drell, John S. Foster, and Charles H. Townes, "Report of the Panel on Nuclear Weapons Safety of the House Armed Services Committee," Dec. 1990, p. 16.

88. Frank S. Eby to Richard L. Wagner, "W-47 Exercise," 25 Jan. 1979; Eby, "History of W-47 Nuclear Safety," Doc. No. COPJ 79-27, 29 Jan. 1979.

89. Edward Teller to Norris E. Bradbury, 23 Dec. 1958.

Los Alamos also tested the one-point safety of its own primaries in Hardtack, some already deployed in the stockpile. Once the nuclear test moratorium commenced, however, the laboratory's approach differed from Livermore's. Rather than use mechanical safing devices to resolve persistent questions, Los Alamos attempted further nuclear design changes to achieve inherently safe primaries. In fact, Los Alamos conducted hydronuclear tests during the moratorium, tests with very small quantities of nuclear material.[90] Even so, some Los Alamos designs were frozen before these tests. Some, including the B-43 bomb, required fixes after deployment. Tests conducted after conclusion of the test moratorium corrected the problems.[91]

Although both laboratories strove to design one-point safe fission weapons and primaries, their approaches diverged during the nuclear test moratorium. What explains their different approaches? And why did Livermore opt for its own mechanically safed primary rather than use a Los Alamos-designed primary? Answers to these questions can shed light on the workings of the two-laboratory system. Technical differences between the Livermore and Los Alamos primaries might offer a partial explanation for Livermore's choice. The weights of primaries differed, as did their compatibility with the different H-bomb designs. Yet Teller had enough confidence in the compatibility of a Los Alamos primary to retain it as a backup option. The matches could not have been extremely ill-suited.

Polaris was Livermore's opportunity to prove itself. The laboratory wanted its own primary in the weapon because it wanted full credit for it. Relying on Los Alamos would imply Livermore was not yet quite up to being a full-fledged laboratory. As one of the Polaris warhead designers recalls, "Polaris was a crucial program. Livermore did not want to share responsibility with Los Alamos [for the Polaris warhead], which it would have had to do had the laboratory taken a Los Alamos primary."[92] At the same time, the deployment of an unsafe warhead would present its own risks. Livermore's credibility and continued existence would be placed in jeopardy should a nuclear accident occur with one of its warheads, not to mention the potential destruction. So Livermore opted for its own warhead, armed with a mechanical safing device to prevent an unwanted nuclear explosion.

For its part, Los Alamos did not operate under similar constraints. Most applications of the Los Alamos primary in question were deployed by the Air Force, which may have had different

90. The yields of these one-point safety tests were not expected to exceed one pound, so were determined by the government not to violate the moratorium.

91. George H. Miller, Paul S. Brown, and Carol T. Alonso, *Report to Congress on Stockpile Reliability, Weapon Remanufacture, and the Role of Nuclear Testing* (Livermore: TID, Oct. 1987), pp. 15-23, for a discussion of the general issue, and p. 22, for B-43 reference.

92. Discussion with Jack W. Rosengren, LLNL, 16 June 1995.

views than the Navy on system safety.[93] Furthermore, Los Alamos did not operate under the same pressures as Livermore, particularly true in the period leading up to and including the nuclear test moratorium. During times of budgetary constraints, or of limited nuclear testing, questions were frequently raised about the need for two laboratories. All eyes were on the Livermore weapons program for potential cuts or elimination, as we shall see in the next chapter.

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93. For an interesting discussion of Air Force attitudes towards risk, see Barton C. Hacker and James M. Grimwood, *On the Shoulders of Titans: A History of Project Gemini* (Washington, D.C.: NASA, 1977).

8. FUTURE UNCERTAIN
From Moratorium to Partial Test Ban
1958-1963

By the late 1950s Livermore had matured as a laboratory, having acquired the facilities, staff, and experience necessary for nuclear weapons design and development. Routines had developed for interactions with Los Alamos, the AEC, and the military services. The two laboratories sought agreement on allocation of weapons development responsibilities, and the AEC largely followed their recommendations. Military preferences were implicit in the pie split if not explicit. At first, the armed services were not as confident in Livermore's abilities, but the laboratory gained substantial weapon development responsibilities nevertheless, though usually for systems that were higher risk or lower priority. By the late 1950s, Livermore had become the second laboratory, no longer the patchwork of programs it had been earlier in the decade. Despite the laboratory's growth into a facility capable of directing the design, test, and development of nuclear weapons, Livermore still faced greater uncertainty than did Los Alamos. The disproportionate threat posed to Livermore by the start of a nuclear test moratorium in 1958 and both laboratories' strategies for coping are considered.

Toward Moratorium

Radioactive fallout from nuclear tests had raised public concerns prior to 1954, but these were intensified by the Castle Bravo nuclear shot. Fallout from the 28 February test scattered over seven thousand square miles of ocean, encompassing the joint task forces supporting the test, Marshallese natives living on nearby atolls, and American weather service personnel stationed on atolls. Radiation sickening of the fishermen on the Lucky Dragon, a Japanese fishing vessel, received worldwide attention.[1]

Soon after, the Prime Minister of India called for a nuclear test moratorium. President Eisenhower did not act on the Prime Minister's suggestion. But the Department of Defense soon asked the AEC to investigate means for reducing radioactive contamination from high yield weapons. The AEC wanted more information and turned to the laboratories. Both laboratories had already thought about "clean" or reduced fallout weapons, and the AEC encouraged their investigations.[2] The AEC wanted to respond further to public opinion and released the "facts" about fallout in a February 1955 report. Nine of fourteen Teapot shots fired at the Nevada Test Site in 1955 were kept to relatively low yields, less than ten kilotons.[3] That same year the AEC designated Livermore's work on low fission content weapons "urgent," requesting the laboratory realign its program towards a major effort on clean weapons. Livermore's high yield weapons program was occupied exclusively with development of the clean device.[4]

Bravo had linked the issues of nuclear disarmament and radioactive fallout for the concerned public, infusing the disarmament talks with additional urgency. In April 1955 the Joint Committee on Atomic Energy got involved, holding hearings on radioactive fallout, as did the Senate Armed Services Committee. Public concerns about fallout also motivated scientists and politicians to portray nuclear explosive power in a more positive light. In August the United States participated in the first International Conference on the Peaceful Uses of Atomic Energy in Geneva.[5]

Eisenhower created the new post of special assistant for disarmament in March 1955 hoping it would inject new life into the London Disarmament Conference which had quickly deadlocked after convening in February 1955. Eisenhower's choice for the position was Harold Stassen, who faced the challenge of balancing the conflicting interests of the State Department, Pentagon, and the AEC, as well as the Soviet Union and America's allies in the North Atlantic Treaty Organi-

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1. Barton C. Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley: Univ. of California, 1994), p. 148; DOE, History Division, "The United States Nuclear Weapon Testing Program: A Summary History," draft document number DOE/ES-0005, August 1984, pp. 14-17.

2. Ruth R. Harris and Richard G. Hewlett, "The Lawrence Livermore National Laboratory: The Evolution of its Mission, 1952-1988," report for LLNL (Rockville, Md.: HAI, 21 March 1990), p. 5.

3. DOE, "The United States Nuclear Weapon Testing Program," pp. 18-19, 52.

4. Harris and Hewlett, "The Lawrence Livermore National Laboratory," p. 5.

5. DOE, "The United States Nuclear Weapon Testing Program," p. 52.

zation. Presidential candidate Adlai Stevenson focused attention on the test ban when he suggested during the 1956 campaign that the United States unilaterally declare a stop to nuclear testing as the first step towards reaching an agreement with the Soviet Union. The AEC preferred that a test ban and nuclear disarmament agreements be linked in a comprehensive arms control package. But a few months after Eisenhower's reelection, Stassen set the stage for a separate agreement on the test ban while internal debates continued on administration disarmament policy.[6]

Livermore's response to the political uncertainty surrounding nuclear testing was to diversify its program while building on its expertise. The laboratory began Project Plowshare—to investigate the use of nuclear explosions for peaceful purposes, including nuclear excavation—holding a symposium on the subject in February 1957.[7] The JCAE continued its oversight, holding a round of hearings in summer 1957 to look into the health effects of radiation. This political environment set the stage for E. O. Lawrence, Teller, and Mark Mills of Livermore who testified at the JCAE's 20-21 June 1957 hearings where they argued against a test ban in strong terms. Lawrence, director of the University of California Radiation Laboratory who also served on one of the task forces established by Stassen to study disarmament inspection procedures, told Joint Committee members it would be a "crime" were the United States to use "dirty" nuclear weapons in war. Fallout hazards from nuclear testing were negligible, and all three scientists agreed it would be "wrong," "misguided," and "foolish," to preclude development of clean weapons that might prevent the death of millions in a nuclear holocaust. Such weapons would require nuclear testing and six or seven years to develop. Teller added another argument to his case against the test ban, pointing out that the Soviets might conceal their activities by testing underground or in the upper atmosphere.[8]

The JCAE had been skeptical at first of AEC claims about clean bomb technology, made just four days after the president announced his disarmament policy and in the midst of the committee's fallout hearings in May 1957. Congressman Chet Holifield of California, with concurrence of committee chairman Clinton Anderson, charged the AEC was misleading the committee

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6. Richard G. Hewlett and Jack M. Holl, *Atoms for Peace and War, 1953-1961: Eisenhower and the Atomic Energy Commission*, vol. 3 of "A History of the United States Atomic Energy Commission (Berkeley: Univ. of California, 1989), p. 296.

7. DOE, "The United States Nuclear Weapon Testing Program," p. 21.

8. Hewlett and Holl, *Atoms for Peace and War*, p. 399. For a discussion of the role of scientists in nuclear test ban negotiations, see Harold Karan Jacobson and Eric Stein, *Diplomats, Scientists, and Politicians: The United States and the Nuclear Test Ban Negotiations* (Ann Arbor: Univ. of Michigan Press, 1966). On the role of scientists in national security decision making see Allen G. Greb, "Science Advice to Presidents: From Test Bans to the Strategic Defense Initiative," IGCC Research paper no. 3, San Diego, 1987.

and the American public. The scientists' testimony in June, however, persuaded at least some JCAE members the president ought to learn more, including possibilities for concealment of Soviet tests. Congressman W. Sterling Cole of New York, a former chairman of the JCAE, arranged for the Livermore leaders to see Eisenhower to whom they repeated their arguments for clean weapons, the ramifications of underground testing, and the utility of small, tactical clean weapons. Eisenhower conceded the benefits of clean weapons but also reminded the scientists of worldwide pressures for a test ban, arguing the United States could not allow itself to be "crucified on a cross of atoms." The president was unwilling to abandon hope of success in the disarmament talks, although he mentioned his interest in clean bombs and peaceful nuclear explosives at news conferences over the summer.[9]

The 1956 Redwing tests series had advanced the development of nuclear weapons with reduced fallout and had investigated smaller and lighter nuclear weapons. The aim of the 1957 Nevada Plumbbob test series was to develop relatively small yield fission and thermonuclear weapons. These were perceived as a partial answer to the problem of massive retaliation, which seemed to lock the U.S. into an all-or-nothing response in war. Henry Kissinger had already begun advocating a more "flexible response" to international crises by this time. Secretary of State John Foster Dulles acknowledged the changing outlook on nuclear weapons in an October 1957 *Foreign Affairs* article. Plumbbob built on the earlier Redwing work, and was aimed at developing nuclear warheads for intercontinental and intermediate range missiles, air defense, and anti-submarine weapons. The series also included the first fully contained underground nuclear test, a novel method for preventing fallout. A Livermore innovation, Rainier was the first fully contained underground nuclear test.[10]

After completing their most extensive test series to date, the Soviets announced on 31 March 1958 they would observe a unilateral test moratorium. The U.S. had not been willing to begin test ban negotiations until agreement had been reached regarding the feasibility of a nuclear weapons test detection system. U.S. and Soviet negotiators were able to agree at the Geneva technical conference in summer 1958 that such a system was workable. Eisenhower then announced the United States would suspend testing for one year, but only after test ban negotiations commenced. The pace of weapons testing accelerated with the impending test moratorium. Thirty-five shots were conducted in the Pacific between April and August 1958 in Phase I of Hardtack, followed by thirty-seven shots in Phase II in Nevada. Hardtack concluded on 31 October, and test ban negotiations began the following day.[11]

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9. Hewlett and Holl, *Atoms for Peace and War*, pp. 398-402.

10. DOE, "The United States Nuclear Weapon Testing Program," pp. 20-23.

11. Ibid; Hacker, *Elements of Controversy*, p. 205.

Dealing with Uncertainty

It was not clear if or when nuclear testing would resume, leading to questions about the scope and size of the nuclear weapons program. In its brief history, Livermore had been more vulnerable to charges of duplication than Los Alamos. Less than one month into the test moratorium, for example, JCAE Chairman Anderson expressed concerns regarding possible laboratory duplication, a striking turnabout from the committee's earlier enthusiasm for vigorous laboratory competition. As senator from New Mexico, Anderson was unlikely to preside over demise of the Los Alamos weapons program, so Livermore was the implicit target of his comments. Until this time it had been the AEC that was most defensive about laboratory duplication. But the AEC now defended the two-laboratory system, arguing that some duplication had been expected in order "to promote complementary competition." The GAC also endorsed the two-laboratory system, though support was not unanimous. One Commissioner argued that the major weapons development problems had been solved. One laboratory might suffice, although it would be "extremely difficult to determine which one to close."[12]

Livermore's position was made even more vulnerable by the fact that its nuclear devices were taking longer to weaponize than first anticipated, an average three years to Los Alamos' two. One explanation was that Los Alamos was "an old hand" at weaponizing devices and had a "more conservative" approach to weapons development. The laboratory's first experimental firing was generally not too different from the final design, making the transition from experimental device to weapon relatively short. In contrast, Livermore designs were harder to calculate theoretically, requiring multiple firings to refine the initial design. This meant a longer time lag between experiment and weapon.[13] This may have been why AEC Chairman John A. McCone raised the possibility of closing Livermore's Sandia laboratory established only a few years earlier to help Livermore transform nuclear devices into weapons.[14] Losing the Livermore Sandia branch—located across the street from the laboratory—and forced to compete with Los Alamos for the resources of the Albuquerque Sandia facilities would have posed a real handicap for Livermore. It could have meant fewer weapons development assignments for the laboratory, not an encouraging prospect during the test moratorium.

Livermore's continued existence, at least in retrospect, was never in serious jeopardy. McCone's suggestion never came to fruition, it serves to illustrate the pressures under which the

12. AEC Meeting No. 1436, 4 Dec. 1958, p. 885.

13. Alfred D. Starbird to Willard F. Libby, "Weaponization Support for Livermore," 23 Sept. 1957.

14. AEC Meeting No. 1436, 4 Dec. 1958, p. 885.

AEC operated during the test moratorium and which had a disproportionate impact on Livermore. The circumstances required a careful justification of the AEC weapons complex, including the two-laboratory structure for nuclear design. Clear mission statements were always important, but especially so during the test moratorium. AEC Chairman McCone remarked during the test moratorium, for example, that the Commission "must arrive at a clear statement of the role of the national laboratories." Their task was so important that the commissioners agreed, if necessary, to delay their report to the JCAE until such a statement had been developed.[15]

The two laboratories developed different strategies for responding to the test moratorium, as might be expected given the different pressures on each. Livermore's promotion of underground nuclear testing is a good example. Edward Teller led the initiative. Livermore director during crucial test moratorium years between March 1958 and July 1960, Teller believed underground testing would create less unfavorable reaction than above ground shots. He advocated it as a means of reducing not only radioactive fallout, but public opposition to nuclear testing.[16] The AEC was not enthusiastic, at least not about an active campaign. AEC Director of Military Application Alfred D. Starbird agreed underground testing would be less problematic from a political standpoint than atmospheric testing. He was also convinced public opposition would continue regardless, as long as testing continued in any form. His main concern was to fend off an atmospheric test ban, and active AEC support for underground testing could jeopardize this goal. The AEC should thus limit testing to the degree possible without impeding weapon development. All possible shots should be placed underground, but limited above ground tests could be conducted if absolutely necessary.[17]

Los Alamos director Norris Bradbury shared the AEC's outlook on the risks involved in advocating underground nuclear testing while test ban talks were underway. He was no more inclined to support Teller and Livermore in advocating underground testing than was the AEC. His differences with Teller were so great he thought devising a joint statement possibly "hopeless." His stated objections were technical. Underground diagnostics would be more difficult, and more expensive. Higher costs associated with underground testing would lead to even greater scrutiny of nuclear testing in general.[18]

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15. AEC Meeting No. 1572, 11 Dec. 1959, p. 662.

16. LLNL, "20 Years in Livermore," *Newsline* [LLL] 3 (Sept. 1972): 15. Teller's position as paraphrased by Alfred D. Starbird in Starbird to Edward Teller, 17/2132Z July 1958.

17. Starbird to Teller, 17/2132Z July 1958.

18. Norris E. Bradbury to Edward Teller, 16 April 1959.

Livermore had a greater incentive to find a solution to the political pressures against nuclear testing and underground testing held such promise. For Los Alamos, the calculus was different. One way or the other the laboratory would probably continue its work, and atmospheric testing was considered preferable. The Los Alamos director's more relaxed outlook on the situation is captured by his response to Teller:

> [W]hatever decisions are reached in Geneva (or unilaterally) . . . the Laboratories will pull in their belts and do their best to do the most important things under whatever set of circumstances are allowed! I am . . . sufficiently impressed by the present status of the national atomic weapon picture that I doubt if the world will come to an end no matter how the Geneva affair finally goes.[19]

Bradbury's principal concern was to garner support for laboratory research of high scientific quality in the event of a test ban and at the same time meet the laboratory's nuclear weapon responsibilities.[20] Livermore operated under different constraints. The laboratory needed to devise a technical program that could survive an atmospheric test ban and at the same time ensure its relevance to the weapons program. Success depended on persuading policy makers the proposed program was important to national security. The programs Teller advocated adhered to these twin imperatives. Underground nuclear testing might reduce public pressures for a test ban. It might also help keep Livermore relevant at a critical time.[21] The laboratory also sought to position itself so that its well-being did not depend entirely on the weapons program. Livermore early on diversified its program into non-weapons related work, though most of it was nuclear-related, including the Plowshare program.

The laboratories also differed in their assessments of the impact of a test ban on potential advances in weapon design. Livermore scientists believed worthwhile advances were possible, while the prevailing view at Los Alamos was that the most important developments had already been made. In early 1958, for example, Livermore Associate Director Harold Brown argued that though nuclear weapons technology was well advanced, more could be done. Factors of 3 to 5 improvement in weight, yield, or cost every few years were

> not to be sneezed at when their reflection in missile weight or readiness, or in numbers of warheads are considered. Had device tests been suspended after 1954, our ICBM warhead would have weighed 6500 pounds and whether the ICBM would have been built is dubious . . . In the current crisis, attempts to move up

19. Ibid.

20. Ibid.

21. Katherine Magraw, "Teller and the 'Clean Bomb' Episode," *BAS* 44 (May 1988): 32-34.

> our missile capabilities seem to depend . . . on using the very latest, or even unborn, warhead technology.[22]

Among future projects Brown held out as potentially interesting were anti-missile and anti-bomber systems, ultra-small fission warheads for recoilless rifles, improved anti-demolition munitions, air-to-air warheads with radioactive contamination properties, "ultra-safe" bombs designed to minimize the probability of accidental explosion, clean bombs, and still smaller and lighter ICBM warheads. If cost were no object, very high yield-to-weight weapons could be developed in small weight classes by extensive use of tritium, oralloy, and plutonium. Brown also mentioned the possibility of extremely low cost fission warheads using small amounts of plutonium.[23]

For his part, Teller explained Livermore was making an "extensive effort" to prepared for testing underground and in deep space should national policy so dictate. Recent theoretical work had increased Livermore's interest in low-weight clean or partially clean weapons. The development of a very light ICBM warhead and the light weight radiation weapon were also important components of the laboratory program. The array of future technological possibilities was so great it would exceed Livermore's capabilities.[24]

Similar enthusiasm was expressed by John S. Foster, head of Livermore's fission weapon program who briefed the AEC's Division of Military Application on Livermore's research and development program. For the distant future, Foster tried to entice his AEC audience with the possibility of atomic hand grenades. A more immediate challenge was to develop a very low weight megaton weapon. Technically, it could already be done, according to Livermore scientist Carl Haussmann. The problem was that "we just can't afford it," given the substantial use of expensive nuclear fuel required.[25]

Livermore enthusiasm about potential future weapon possibilities contrasted sharply with Darol Froman's, a senior scientific advisor to Bradbury. The AEC had "doubled the effort" by establishing a second laboratory. But in an implicit criticism of the two-laboratory system Froman argued that

> the exploitation of the nuclear weapon development business has been accomplished for some time. . . . For the past few years we have been attempting to substitute magnitude of effort for brilliant and productive ideas and it is getting us

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22. Harold Brown to Alfred D. Starbird, 29 Jan. 1958, p. 7.

23. Ibid., pp. 4-6.

24. Edward Teller to Alfred D. Starbird, "Mid-Year Review of the LRL, Livermore Program," 16 July 1959.

25. "Briefing for DMA Personnel by LRL on Their Research and Development Work, November 4, 1959," 27 Nov. 1959 draft, Carl A. Haussmann comments, p. 19.

> just where any activity of this sort always does—slow and rather insignificant improvement with the addition of frills—as in the automobile industry.[26]

The important "scientifically based" achievements were past: the invention of the atomic bomb, implosion, and levitation; two-stage and solid-fueled thermonuclear bombs; gas boosting and inherent one point safety. Reductions in size and weight for both fission and thermonuclear weapons were also among the significant scientifically based achievements listed by Froman. None remained of comparable importance to those already accomplished, and no ideas were "crying for exploitation." The problem had become almost entirely one of improved engineering.[27]

Froman believed Hardtack had not substantially altered the nation's military posture beyond demonstrating the feasibility of smaller fission and thermonuclear weapons. This, despite the huge number of tests conducted which roughly equalled all those conducted by Los Alamos in its entire history through 1956. Many Los Alamos scientists, especially those who had participated in the Manhattan Project, held views similar to Froman's, recalling the wartime years and even the early 1950s as ones of discovery and invention. From their perspective, only incremental progress was made thereafter. Froman's comments are worth citing at length. He was

> not trying to present an argument against improvement of product. . . . My contention is that the promise of either rapid or radical improvement in nuclear weapons is so dim . . . that we have a distinct unbalance in effort judged either on an overall National basis or purely as a defense enterprise . . . if . . . half of the highly technical effort expended on weapons at Livermore and Los Alamos were gradually diverted . . . to <u>any</u> other research and development field which would be attractive to the well trained and experienced personnel involved, the country as a whole would profit significantly.[28]

Los Alamos would meet its stockpile responsibilities, regardless of the benefit or lack thereof of nuclear testing, at least over the next several years. Most weapons scheduled to enter the stockpile applied known principles and techniques. These could be put in the stockpile on schedule, "not exactly with our left hands," as Froman put it, but with only a fraction of the current weapons effort.[29]

Froman supported a test ban to keep the Soviet Union from testing. Nothing of vital interest to the United States, he argued, would be precluded by it. Further tests were not worth fighting

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26. Darol K. Froman to Alfred D. Starbird, 4 May 1959.

27. Ibid.

28. Ibid.

29. Ibid.

for, particularly since they would spur the Soviets to further testing at a net loss to the United States given the U.S. advance on the Soviets: "We have milked the nuclear weapon business pretty dry . . . [and] they [the Soviets] have a few quarts yet to go." Froman thus advocated a gradual reduction in Los Alamos' weapons work. The laboratory should continue at its present size, while gradually transferring personnel and effort into non-weapons activities. The extent to which the transfer from weapons should go would depend on what Livermore wanted.

> If Livermore wished to concentrate heavily in weapons, LASL might get out of the field nearly completely after fulfilling its current commitments. The other extreme, with Livermore [getting out of the weapons field] . . . might [also result in] . . . a reduction of weapons effort at LASL . . . In either case, should a good idea arise, we would be in a position to exploit it, if we keep a vigorous laboratory. A laboratory can remain vigorous and inventive only if it is concerned in large measure with challenging and important technical problems.[30]

The AEC was unlikely to terminate the Los Alamos nuclear weapons program as long as it was deemed important to the country, especially with a New Mexico senator in the chair of the congressional Joint Committee on Atomic Energy. Los Alamos leaders could muse freely about life without nuclear testing, paring back to one nuclear weapons laboratory, and even the termination of the Los Alamos weapon program, without experiencing the consequences. Livermore did not have that luxury.

Resumed Testing

President Kennedy announced on 5 September 1961 that the United States would resume nuclear testing. The Soviet Union had already done so. Radioactive fallout around the world resulting from Soviet tests had revived public concerns, so Kennedy declared the United States would test only in the laboratory or underground.[31] Initially, most U.S. tests were therefore conducted underground, though atmospheric tests soon resumed, if only for a brief period. The Soviets conducted fifty-two tests in 1961, while the U.S. total came to ninety-six in 1962, the largest number of tests either had conducted in a given year.[32] The large numbers anticipated the resumption of test ban talks, which had ended in August 1961 when the Soviets resumed testing. They also anticipated the Limited Test Ban Treaty (LTBT), signed in August 1963 by the United States, Soviet Union, and Great Britain. The LTBT banned nuclear testing in the atmosphere,

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30. Ibid.

31. Hacker, *Elements of Controversy*, p. 206.

32. "Known Nuclear Tests Worldwide: 1945-1994," *BAS* 51 (May-June 1995): 70-71. Total includes two tests in the peaceful nuclear explosions program.

under water, and outer space. After its implementation all U.S. and Soviet nuclear tests were conducted exclusively underground.[33]

Did Livermore's advocacy of underground testing enable the LTBT or preclude a comprehensive test ban? Critics would argue that Livermore's technical fix to the political problem of nuclear testing impeded the larger goal of a test ban. One might also argue that underground testing helped resolve intense political pressures regarding nuclear fallout while allowing the United States to continue nuclear testing. Either way, the case provides evidence of technical and organizational innovation in the face of political uncertainty.

By this time, the AEC had created a nuclear arsenal of tens of thousands of weapons, seven thousand of them in Europe, comprised of strategic and tactical missiles, nuclear bombs, nuclear artillery shells, atomic demolition munitions, nuclear antisubmarine weapons, and nuclear torpedoes.[34] Demands on the weapons complex were so great the AEC grew concerned that in two or three years requirements might significantly exceed capabilities. The AEC sponsored a weapons symposium at Los Alamos in November 1963, attended by representatives of the three services, DOD, the Joint Staff, and others intended to provide a clearer picture of the future.[35] Contrary to AEC and laboratory expectations DOD was entering a period of constraint. Changes instituted by Secretary of Defense Robert McNamara reduced demand on the AEC and the laboratories. Flexible Response emphasized conventional weapons, the Kennedy administration's answer to concerns about the asymmetry of U.S. forces embodied in the New Look. The latter was intended to counter threats with American strengths against weaknesses of the adversary. Kennedy and his advisors, however, argued the policy could inhibit U.S. responses to lower levels of aggression since all-out nuclear response might invite retaliation. In March 1961 Kennedy set out the objectives of a new strategy intended to "deter all wars," general or limited, nuclear or conventional, large or small. Flexible response required developing the capacity to respond to all levels of aggression, with top priority given to decreasing reliance on nuclear weapons in deterring limited aggression. Special emphasis was thus placed on bolstering NATO's conventional capabilities.[36]

The Kennedy administration's desire to reduce U.S. dependence on nuclear weapons did not mean reducing their number or variety. The new policy simply called for increasing conventional

33. DOE, "The United States Nuclear Weapon Testing Program," p. 25.

34. Ibid., p. 26.

35. Richard D. Wolfe, "Briefing for the Commissioners on the Results of the Weapons Symposium held at Los Alamos Scientific Laboratory Nov. 7-8, 1963," 7 Nov. 1963.

36. John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of Postwar American National Security Policy* (Oxford: Oxford Univ. Press, 1982), pp. 213-216.

capabilities alongside nuclear capabilities. Kennedy and his advisors, including Secretary of Defense Robert McNamara, were thus committed to upgrading American strategic capabilities beyond what Eisenhower had contemplated. By mid-1964, there were 150 percent more nuclear weapons available, a 200 percent boost in available megatonnage. The construction of ten additional Polaris submarines (for a total of 29) and 400 additional Minuteman missiles (for a total of 800) above Eisenhower administration plans.[37] These numbers did not translate into new weapons design and development work for Livermore and Los Alamos as the AEC learned in November 1963, although they did tax the AEC production complex. The armed services had gained a broad, balanced, and mature nuclear capability through great effort and at vast expense. DOD would authorize significant upgrades or improvements to the present systems, but service requests for new weapons would be closely scrutinized, especially where they might render an existing system obsolete without a concomitant increase in effectiveness. The AEC was thus not likely to find its research and development capability "saturated" by weapons development requests (Figure 4).[38]

The demand-supply relationship between the AEC and the armed services was thus undergoing a fundamental shift. Demand for new weapons had grown so much by the late 1950s that Bradbury had suggested the standardization of warheads and that missiles be adapted to warheads, not vice versa as was the practice.[39] As it turned out, however, new weapons demand had peaked. Under these circumstances service interest in new weapons was not be expressed until well into the weapon development effort. AEC and laboratory strategies changed in accord. Starting in this period the AEC's Division of Military Application and top laboratory management continually assessed and redefined research objectives to better match laboratory capabilities to military interests.[40]

It was the perceived need for just this sort of proactive work with the military that prompted Livermore's establishment of the Military Applications group in spring 1964. This group institutionalized the laboratory's already entrepreneurial approach to the services and to weapons development. The group's first principal concern were problems associated with operational issues surrounding the deployment of nuclear weapons in Europe, where safety and control were paramount.[41] It eventually developed into something resembling a "marketing" and analysis function

37. Gaddis, *Strategies of Containment*, p. 218.

38. Wolfe, "Briefing for the Commissioners," 7 Nov. 1963, p. 43.

39. Alfred D. Starbird to Kenner F. Hertford, 4 Nov. 1957.

40. Wolfe, "Briefing for the Commissioners," 7 Nov. 1963, pp. 44, 47.

41. Peter Stein and Peter Feaver, *Assuring Control of Nuclear Weapons: The Evolution of Permissive*

for the laboratory. As one Livermore scientist recalls, the "world was getting more and more complicated," and the Military Applications Division, as it came to be called, was needed to focus on "what military needs were even before these were written down."[42] Los Alamos did not establish a comparable group for another decade.[43]

The laboratories were kept busy in the 1960s on work begun the previous decade. The impact of declining military demand for new nuclear weapons was thus not felt immediately. The laboratories continued their practice of "pie splitting." Each laboratory to a large extent continued to specialize, Livermore on the development of strategic missile warheads for the Air Force and Navy, Los Alamos on meeting the needs of the military customers with whom it had long-established ties. Livermore developed the Titan missile and follow-on warheads for Polaris. The laboratory also developed a modified version of the Polaris warhead for deployment on Minuteman II. Concerns about possible deployment of a Russian anti-ballistic missile system encouraged the development of multiple independently-targetable reentry vehicles (MIRVs). The first to develop MRV technology for the Polaris A-3 missile deployed in 1964, Livermore also develop the first MIRV warheads deployed on the Navy's Poseidon and Air Force Minuteman III missiles.[44] Los Alamos continued developing all Air Force gravity bombs as well as most of the tactical nuclear weapons integral to the strategy of Flexible Response.[45]

The end of the test moratorium ended the immediate uncertainty but also ushered in a period of reduced military demand for nuclear weapons. This contributed to the feeling among laboratory scientists that the 1960s were a "competitive" decade. Shifting military priorities also may have also contributed. Navy strategic systems were growing in importance relative to Air Force

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Action Links, CSIA Paper No. 2 (Lanham, Md.: Univ. Press of America, 1987) for problem of weapons deployment in Europe. On formation of the Military Applications Group see Carl A. Haussmann to W. B. Harford/W. E. Humphrey, "Formation of the Military Applications Group," 18 March 1964; Carl A. Haussmann to distrib., "Formation of Military Applications Group," 15 April 1964. See also Jeff Garberson, "Linking Two Realities," *The Quarterly* [LLNL], Dec. 1984: 12-16, for a discussion of the more recent operations of the Military Applications function at Livermore.

42. Interview with Carl A. Haussmann, LLNL, 4 Oct. 1991.

43. The W-9 Division was established c. Jan. 1968. See "W: Weapon Nuclear Engineering Division," Division History," in LLNL Archives, n.d.

44. For W-62 Minuteman III warhead, see SNL, *The First Thirty Years*, p. 133, for Polaris A-3; Thomas B. Cochran et al., *U.S. Nuclear Forces and Capabilities*, vol. 2 of "Nuclear Weapons Databook" (Cambridge, Mass.: Ballinger, 1984), p. 68.

45. Data compiled from several sources, including Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), pp. 150-152; and DOE Office of Military Application, *A History of the Nuclear Weapons Stockpile, FY 1945-FY 1985*.

systems. And strategic missiles were growing in importance relative to strategic bombs (Figure 5). The consequences of these shifts are investigated in the next chapter.

9. WARHEAD WARS

Cooperation and Competition since the 1960s

Several important features of the two-laboratory system emerged from the preceding historical analysis. The AEC encouraged the laboratories to cooperate, which they did during periods of high military demand for nuclear weapons. The allocation of weapon development responsibilities known as the "pie split" operated smoothly during the 1950s and 1960s, and neither laboratory seriously contested the agreed division. This did not mean the division was equal. Patterns emerged that distinguished the number, nature, and kind of weapons developed by each laboratory. Although the AEC had formal authority to make weapon development assignments, it was nevertheless reluctant to act against military wishes. The divisions thus tended to reflect military preferences and priorities. High priority military customers or program demands were accommodated first, while those with lower priority had to make due with remaining resources.

Los Alamos had a head start on Livermore in terms of facilities, resources, responsibilities, and the confidence of the armed services. Los Alamos' prior experience and its strong representation in Congress were important factors. Clinton P. Anderson, for example, held the important chairmanship of the Joint Committee on Atomic Energy from 1954-56 and 1959-60. As the representative from New Mexico, Anderson was particularly attuned to the concerns of Los Alamos, particularly during the latter period of his chairmanship which overlapped with the nuclear test moratorium, a period of uncertainty for both laboratories. Congressman Chet Holifield of California chaired the JCAE during 1960-61, 1965-66, and 1969-70 and might have been expected to be especially attentive to Livermore, although his congressional district did not include the laboratory.[1]

As the "second lab," Livermore bore the greatest burden to justify the two-laboratory system and since its inception had been expected to be the "new ideas" laboratory. This created an incentive structure and environment that promoted technical innovation, which also entailed risk. The laboratory was also attuned to potential future military customers who wanted technical innovation, especially if it could be brought to bear in the political arena. There were risks inherent in serving the needs of military customers whose political position was uncertain or in flux. Los Alamos' were more conservative in their outlook and preferred incremental technical change.

This chapter resumes the account of the two-laboratory system in the early 1970s. By this time, Navy strategic systems had become a mainstay of the strategic triad alongside Air Force strategic bombs and missiles. Sea-based strategic missile warheads alone by the early 1970s outnumbered strategic bombs in the stockpile (Figure 5). As of the early 1970s, Livermore had developed all of these (Table 1). Already declining military demand for new nuclear warheads declined even more during this period (Figure 2). The result was a more competitive laboratory relationship. Los Alamos, as we shall see, grew restless with the traditional allocation of responsibilities: Los Alamos for strategic bombs, Livermore for strategic missile warhead development. It was an important departure from past practice when in 1973 Los Alamos won responsibility for the Navy's new Trident missile warhead. This led to other important changes for the both laboratories. We examine the causes and consequences of this shift, which further illuminate the workings of the two-laboratory system.

From Polaris to Poseidon

As discussed in Chapter 7, Polaris was a success story for both Livermore and the Navy. Working together, they accomplished the deployment of the new missile and warhead under the pressures of the test moratorium and the program's acceleration after the Soviets launched Sputnik. Nevertheless the safing mechanism in the early Polaris W-47 warheads, installed to prevent unintended or accidental nuclear detonation, were discovered to be defective. The risk of unintended or accidental detonation was small. Instead, a substantial fraction of the Polaris warheads were potential "duds," unlikely to give a nuclear yield when intended. The problem was addressed in the early 1960s and thought to have been resolved. Stockpile surveillance tests in

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1. Chet Holifield also served as MLC Chairman in 1970. See DoE History Division, "The United States Nuclear Weapon Testing Program: A Summary History," draft DoE/ES-0005, August 1984, appx. II, "Personnel Associated with the Atomic Energy Commission," p. 57.

1966 indicated otherwise, and the problem was worsening as the warheads aged.[2] Livermore scientists proposed adapting a one-point safe Livermore-designed primary similar to the one used in the Polaris A-3 missile warhead (the W-58) to substitute in the defective W-47s.[3] An initial shipment of the retrofitted warheads was made to the Navy in March 1967 and the process was complete by October.[4]

The reliability of the Polaris fleet was restored, although not without political sensitivity for the Navy, especially the Strategic Systems Project Office (SSPO), responsible for new missile development. A former director of the SSPO Reentry Body Coordinating Committee recalls the consternation of the Joint Chiefs of Staff when they realized there was "almost zero confidence that the [Polaris] warhead would work as intended."[5] The problem was still a sensitive one several years after it had been solved recalls Robert J. Stinner who went to Washington to direct SSPO's Reentry Body Section in 1969. Today, Stinner believes the Polaris problems were unrelated to Livermore's loss and Los Alamos' gain of the Navy strategic missile warhead business in the early 1970s. SSPO and other Navy representatives interviewed for this study agreed. Problems were to be expected in deployment of new systems, although mechanical safing schemes became increasingly suspect as a result of the Polaris experience. As long as these were resolved, however, they would not have been held against the laboratory. Stinner nevertheless recalls telling a gathering of laboratory and service representatives at a Los Alamos sponsored meeting to consider future Navy strategic programs that the Navy "did not want to have to deal with [such problems] again."[6]

Poseidon was the next generation Fleet Ballistic Missile (FBM) developed after Polaris.[7] Studies of extremely lightweight thermonuclear warheads started in the early 1960s. By 1964 scientists believed lightweight reentry vehicle and nuclear warhead combinations in the 100 to 150

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2. Frank S. Eby, "History of W-47 Nuclear Safety," Doc. No. COPJ 79-27, 29 Jan. 1979; Chuck Hansen, *U.S. Nuclear Weapons: The Secret History* (New York: Orion Books, 1988), pp. 204-205.

3. Carl A. Haussmann to D. L. Crowson, 24 Dec. 1966. The Polaris A-1 missile warheads were phased out of the stockpile by 1966. See Thomas B. Cochran et al., *U.S. Nuclear Forces and Capabilities*, vol. 1 of "Nuclear Weapons Databook" (Cambridge, Mass.: Ballinger, 1984), p. 104, for dates of A-1, A-2, and A-3 entry and retirement from stockpile.

4. Eby, "History of W-47 Nuclear Safety," 29 Jan. 1979.

5. Telephone interview with Alec Julian, 21 June 1995.

6. Telephone interview with Robert J. Stinner, 21 June 1995.

7. For a history of Poseidon missile development, see Graham Spinardi, *From Polaris to Trident: The Development of US Fleet Ballistic Missile Technology* (Cambridge: Cambridge Univ. Press, 1994), pp. 86-112.

pound range technologically feasible. DOD asked the AEC to study the possibilities in September 1965, and in July 1966 asked it to develop such a warhead for the Poseidon missile. Livermore was selected to do the job.[8] The Poseidon warhead, dubbed the W-68, was considered unconventional and unusual for a number of reasons. It would allow the lightest warhead/reentry vehicle combination yet. The combined requirement to harden the warhead and to make it lightweight would demand more extensive system component integration than ever attempted. Weight restrictions were severe, since up to fourteen reentry bodies might be deployed on a single Poseidon C-3 missile, a consequence of its intended deployment in a possible Soviet anti-ballistic missile system environment. By comparison, the Polaris A-3 missile carried only three nuclear warheads.

Given the high degree of integration it seemed to make sense for one organization to coordinate Poseidon warhead/reentry body assembly. A good idea in principle, it presented difficulties in practice. Each side wanted to assign blame to the other for delays. The AEC blamed them on deficiencies in DOD-supplied parts. Key parts supplied by the Lockheed Missile and Space Corporation indeed had to be returned and replaced.[9] But staff in the Office of the Secretary of Defense (OSD) blamed delays on the AEC.[10] And the combination of problems created by an AEC strike and delays in furnishing Poseidon reentry bodies on schedule were described in internal DOD documents as an "AEC production problem."[11]

The Navy's Strategic Systems Projects Office was not immune to criticism. Robert H. Wertheim, who held a number of positions in SSPO and was then Technical Director, recalls little difficulty in taking Poseidon through the development program. The production phase was another matter. The concept of a highly integrated AEC device had been a good one, but in practice had been challenging beyond expectations. The decision to treat the reentry body and warhead as an "all-AEC device" created difficulties. SSPO had had a reputation for "doing things right and not overfronting," but the Poseidon team "took a lot of lumps."[12]

Although the problems were not insurmountable, they had been costly, and delayed the program, but only by a few months. Armed with the W-68 warhead Poseidon was deployed in March 1971. The warhead yield was half what Livermore had promised. Livermore scientists

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8. SLL and LRL, *Final Development Report on the W68-0/Mk3 Re-Entry Body*, Feb. 1971, pp. 5-6.

9. Edward B. Giller to Carl Walske, 4 Nov. 1970; Giller to Walske, 17 Nov. 1970.

10. James W. Stansberry to Adm. Murphy, 19 Nov. 1970.

11. L. M. Mustin to Chairman JCS, "Poseidon Warhead (W68-0/Mk-3) Production, 3 Nov. 1970; John S. Foster to George H. Mahon, draft letter, n.d.

12. Telephone interview with Robert H. Wertheim, 20 June 1995.

had believed they could increase yield by modifying the secondary design, and had proposed doing so for the next generation Navy FBM warheads. The proposed means of achieving the higher yield was attractive in principle and physics tests showed it could work. In practice it proved difficult as well as expensive, utilizing costly nuclear materials. A February 1968 internal Livermore laboratory memorandum on the subject provides insight into the difficulties of the task Livermore scientists had carved out for themselves. The merits of the modified secondary design were uncertain, and, as noted the author, "no matter what we do, we gamble."[13] William B. Shuler, a physicist who started work at Livermore in 1972, recalls his first assignment was to "fix" the W-68 yield. By fall 1972, Livermore scientists had confidence in the secondary design modification and proposed it be included in the last 30 percent of Poseidon warheads produced. But the Navy declined the option.[14] By then, however, there was interest in applying the design modification to the Navy's next generation FBM: Trident.

Trident

Even as Poseidon was being deployed in the early 1970s the Navy was looking ahead to Trident, a new generation FBM system.[15] Although the new missile and submarine were approved by the Secretary of Defense in 1971 it would be another two years before the system's technical characteristics were agreed upon. The size of the new submarine and missile were debated within the Navy and among DOD officials. Of particular interest to this analysis is to understand the conflicting bureaucratic interests regarding the need for a new warhead. Also at stake were warhead yield and the missile's potential counterforce capability.

The Navy's Strategic Systems Planning Office did not want a new warhead for the new missile, preferring to use a modified Poseidon warhead for Trident. This would save costs in both the reentry body (RB) and nuclear warhead development and avoid costly flight testing. Refurbishing the missile's front end would also allow SSPO to concentrate on extending missile range by as much as 50 percent, its principal goal. Finally, utilizing the relatively small yield Poseidon warhead for Trident would also be consistent with SSPO's vision of the FBM system as an urban-industrial weapon, not one with significant counterforce capability.[16] Although the Navy

13. R. A. Greene to distrib., 20 Feb. 1968.

14. Telephone interview with William B. Shuler, 23 June 1995.

15. For a history of the Trident I and Trident II programs, see D. Douglas Dalgleish and Larry Schweikart, *Trident* (Carbondale and Edwardsville: Southern Illinois Univ. Press, 1984); for a discussion focused on the Trident I missile program, see Spinardi, *From Polaris to Trident*, pp. 113-140; Levering Smith, Robert H. Wertheim, and Robert A. Duffy, "Innovative Engineering in the Trident Missile Development," *The Bridge* 10 (Summer 1980): 10-19.

16. Spinardi, *From Polaris to Trident*, pp. 125-131.

would not be responsible for new warhead development costs, these were nonetheless a concern when promoting the system as a package to the Congress.

SSPO had thus hoped to avoid a phase 2 study for the Trident warhead, the purpose of which would have been to determine new warhead options. Instead, SSPO preferred to move directly into phase 3—the warhead development stage—using the Livermore-designed Poseidon W-68 as the basis of the Trident warhead.[17] But this was not to be, and SSPO got a new warhead for Trident. Vice Admiral R. Y. Kaufman, then senior Navy representative on the Military Liaison Committee, later director of Submarine Systems in SSPO, recalls SSPO got "a lot of help" on this outcome.[18] That help came from the Office of the Secretary of Defense, the Chief of Naval Operations, and the Chairman of the Joint Chiefs of Staff, all of whom wanted a Trident warhead with a higher yield than Poseidon's "small" warhead afforded.[19] SSPO was defeated by this array of actors, and new nuclear warhead was gotten for Trident. Livermore's advocacy of a new warhead, as we shall see, contributed to the final outcome, though not exactly as the laboratory had hoped.

John S. Foster, Director of Defense Research and Engineering (DDR&E), who earlier had served as Livermore director, asked the AEC in January 1972 to join DOD in a phase 2 feasibility study of potential warhead candidates for a new missile, later named Trident. The turnaround time for the study was expected to be fast, and Foster asked that the report be completed by March. DOD studies had indicated a warhead with the approximate size and weight characteristics of the Poseidon W-68 warhead and its Mk 3 reentry body would be a good match for the new missile. Foster wanted the AEC to investigate other nuclear warhead candidates as well, including options for achieving the same yield as the W-68 in a smaller warhead, or larger yields in the same size warhead.[20]

Foster's request came amid political maneuverings of the Secretary of Defense, who pressed for a new submarine against White House wishes. In December 1971 without consultation with the president, the Secretary had ordered accelerated development of a new submarine. Foster's letter to the AEC was sent on 12 January 1972, the same day DOD actions were leaked to the press.[21] Aware of the uncertainties involved and awaiting final decisions on the new submarine,

17. Telephone interview with C. Milton Gillespie, 21 June 1995.

18. Telephone interview with R. Y. (Yogie) Kaufman, 17 June 1995.

19. Spinardi, *From Polaris to Trident*, pp. 126-127.

20. John S. Foster to James R. Schlesinger, 12 Jan. 1972.

21. Spinardi, *From Polaris to Trident*, p. 121.

missile, and most importantly, warhead yield, the AEC was reluctant to commit resources. Instead, the AEC promised to conduct the feasibility study as soon as the missile's military characteristics were defined. The earliest possible date for the study's completion was July 1972.[22]

The laboratories considered low-, mid-, and high-yield warhead designs. Livermore and Los Alamos each submitted a list of potential warhead candidates to the Navy, as was the practice in phase 2 studies. Each laboratory had one candidate among the options it viewed as its best for meeting stated requirements.[23] Los Alamos highlighted the "conservative" nature of its proposals which would not stress the nuclear and engineering design process. The laboratory explained that a conservative approach was desirable given the short development times for the missile and reentry body. Although nuclear testing would be required for any of the Los Alamos options, the laboratory assured its potential sponsors that most important features would be tested within the year. And a full yield test was scheduled for fiscal year 1973. Los Alamos also included information about more advanced technical designs to cover possible interest in these.[24]

Livermore's self-described best candidate for meeting the Trident reentry body requirement was a mid-range yield warhead. Reading between the lines of the phase 2 study, however, one gets the impression this was not the laboratory's preferred option. The report language described Livermore's "best" candidate as a mere improvement on the Poseidon W-68 warhead, described as "1965 technology." Substantial discussion was devoted to what appeared to be the laboratory's real preferred option, which incorporated future "design goals," available perhaps by 1974. As the report indicated acceptance of the more ambitious option would mean delaying final warhead specifications to at least 1974.[25]

Ongoing debates within the Navy and among OSD officials over the specific characteristics of the new missile and its payload delayed selection of the new warhead design long after the phase 2 study was complete. The scientist in charge of preparing Los Alamos warhead options recalls the "long long time where Washington politics took over" after the phase 2 study submission and prior to warhead selection.[26] The more usual practice was an interval of only a few months between phase 2 study completion and final warhead selection, unless there were uncer-

22. Edward B. Giller to John S. Foster, 1 Feb. 1972.

23. SSPO, *Joint AEC/DOD Mk 400 Phase 2 Feasibility Study Report*, 1 July 1972, p. 1.

24. Ibid., pp. 8-10.

25. Ibid., pp. 40-41, 64.

26. Interview with C. Milton Gillespie, 5 April 1991.

tainty about the future of the weapon system in question. In the case of Trident much was contested.

Agreement among key DOD and Navy actors on yield and other characteristics of the new Trident warhead had still not been reached as of September 1972. Foster informed AEC Chairman James R. Schlesinger that DOD was still reviewing the phase 2 study and that the Navy had not yet made any decisions. A further delay of several months was expected, but Foster nevertheless asked the AEC to have the warhead ready by December 1977.[27] Schlesinger responded that the Navy's choice would have a significant impact on which laboratory was selected to develop the new warhead. Until DOD supplied the warhead's desired military characteristics, the "appropriate laboratory" could not be selected. Schlesinger would await the Navy's decision before making the phase 3 engineering development assignment to one of the laboratories.[28]

Both laboratories had submitted phase 2 study options that included high yield designs, but initial expectations had been that the new warhead would have a low- or mid-range yield. Livermore favored the secondary design modification discussed above for producing the high yield.[29] Los Alamos promoted a different approach, one laboratory scientists believed presented minimum risk and a better method for achieving desired warhead characteristics.[30] Strong DOD interest in a high yield warhead, however, prompted Los Alamos to test a secondary device in fall 1972 with features similar to the one favored by Livermore. Based on tests conducted by both laboratories, AEC Chairman Schlesinger felt confident the AEC could provide DOD an initial mid-range yield warhead which could be increased by later modifying the secondary.[31]

AEC Director of Military Application Frank Camm brought the results of the Los Alamos high yield nuclear test to SSPO's attention in what appears to have been an explicit AEC effort to promote a Los Alamos-designed warhead.[32] SSPO Director Rear Admiral Levering Smith was surprised by the AEC's suggestion to reopen the issue since he had agreed with Los Alamos' earlier assessment that a different design was preferable. Smith nevertheless agreed to reconvene the phase 2 warhead study group in November 1972 to consider the high yield design feature.[33]

27. John S. Foster to James R. Schlesinger, 25 Sept. 1972.

28. James R. Schlesinger to John S. Foster, 20 Oct. 1972.

29. SSPO, *Mk 400 Phase 2 Feasibility Study*, 1 July 1972, p. 50.

30. Levering Smith to Frank A. Camm, 11 Nov. 1972.

31. James R. Schlesinger to John S. Foster, 20 Oct. 1972.

32. Frank A. Camm to Levering Smith, 8 Nov. 1972.

33. Smith to Camm, 11 Nov. 1972.

This also opened the way for Livermore to submit a supplement to its original phase 2 findings.[34]

In January 1973 Foster was finally ready to ask the AEC to begin development of the Trident missile nuclear warhead. Before doing so, he solicited comments from the Office of the Secretary of Defense, the Joint Chiefs of Staff, and the Navy. He emphasized the importance of controlling AEC and DOD production costs. Acceptance of a heavier and larger reentry body, for example, would allow warhead designs which could be produced at lower cost while still providing high yield. On the other hand, deploying only 8 reentry bodies per missile would still allow the Navy to achieve extended missile range.[35]

By March 1973, one issue at least was resolved. Assistant to the Secretary of Defense for Atomic Energy Carl Walske informed the AEC that a high yield warhead based on the secondary design modification discussed above was not desirable. It would have a large impact on AEC production facilities and DOD was trying to hold overall costs as low as possible. Other warhead characteristics remained to be finalized, but the phase 3 warhead development request would not ask for the expensive design option.[36]

The principal area of debate and discussion among senior DOD and Navy officials now was warhead yield and the number of reentry bodies carried on the new missile. Would the Navy use the same warhead as the Poseidon, the W-68? Or would a new higher yield warhead be requested? Would the missile carry eight or ten reentry bodies? In March 1973, John D. Christie, the Principal Deputy Assistant Secretary of Defense for Systems Analysis analyzed these questions. Christie argued that the military effectiveness of a Trident payload of eight reentry bodies (RB) at the high yield was about the same as ten at the lower yield. The greater number of RBs with the lower yield was more effective against soft targets, while the larger yield was more effective against harder targets. The high yield weapons would not, however, pose a significant hard target capability.[37] Overall effectiveness would be reduced somewhat if eight lower yield RBs were used per missile versus eight higher yield warheads. His calculations showed the loss would be small and acceptable, however, in view of cost and nuclear materials savings.[38] In

34. William B. Shuler, "Mk 400 Phase 2 Supplement Draft," CMA-73-21, 26 Jan. 1973. There is no copy of this document at Livermore, only a record of its existence.

35. John S. Foster to ATSD(SA), ATSD(AE), ASN(R&D), and Director, Joint Staff, "Phase III Nuclear Warhead Development for the Trident I Missile," draft letter to AEC attached, 26 Jan. 1973.

36. Carl Walske to Frank A. Camm, 2 March 1973.

37. John D. Christie to DDR&E, "Phase III Nuclear Warhead Development for the Trident I Missile," 9 March 1973.

38. John D. Christie to DDR&E, "Phase III Nuclear Warhead Development for the Trident Missile," 9 April 1973.

short, retaining the W-68 yield would result in cost savings relative to the higher yield warhead, and without great loss in military effectiveness.

Since the effectiveness of the two payloads was about equal, Christie argued for the higher yield warhead but only if significant cost savings could be realized over the lower yield option. Should the estimated cost of the higher yield warhead exceed the cost of the W-68 by more than 15 percent, Christie urged impressing upon the AEC this would raise questions about the choice of a new warhead.[39] While DOD could not directly control AEC costs, maintaining the W-68 warhead option could provide leverage over the AEC. At the same time, Christie had set up a neat argument for the high yield option; it represented the thrifty approach, assuming the cost of purchasing fewer high yield warheads would be less than producing greater numbers of lower yield warheads. Why might DOD want to set up such an argument? Underlying these considerations was Navy—though not SSPO—desire to gradually move the Navy towards a hard target capability. Sentiment against this was strong in Congress, making it difficult to push Trident as a counterforce weapon.[40] Navy and DOD hard-target advocates thus avoided defining the missile as such, despite their desire for increased capabilities.[41] Studies showing the high yield option was the most economical might thus deflect potential criticisms.[42]

DOD and Navy advocates of the higher yield warhead for Trident commanded more power than did SSPO, which still preferred refurbishing the W-68 warhead. SSPO finally agreed to the new warhead, reportedly because it recognized the "political benefit of agreeing with OSD [Office of the Secretary of Defense]."[43] On 23 March 1973 the Secretary of the Navy informed Foster of the Navy's preference for the high yield warhead. He also emphasized the importance of maximizing missile range over warhead yield, noting that if tradeoffs were to be made the Navy preferred range over yield.[44]

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39. Christie to DDR&E, "Phase III Nuclear Warhead Development for the Trident I Missile," 9 March 1973.

40. Spinardi, *From Polaris to Trident*, pp. 125-126.

41. For the Navy's ultimate achievement of hard target capability, see Graham Spinardi, "Why the U.S. Navy Went for Hard-Target Counterforce in Trident II (And Why it Didn't Get There Sooner)," *IS* 15 (Fall 1990): 147-190.

42. Christie to DDR&E, "Phase III Nuclear Warhead Development for the Trident Missile," 9 April 1973.

43. As quoted in Spinardi, *From Polaris to Trident*, p. 127.

44. John W. Warner to DDR&E, "Trident Mk 400 (MIRV) Nuclear Warhead Development," 23 March 1973, with attached "Military Characteristics for the Warhead for the Navy Mk 400 Re-entry Body."

Dixie Lee Ray replaced Schlesinger as Chairman of the AEC. Foster informed her on 23 April 1973 that the DOD Trident study was finally complete. Changes had been made since his January 1972 correspondence on the subject, reflecting DOD efforts to minimize system acquisition costs while maintaining effectiveness. DOD had thereby "accepted" a reduction in the number of reentry bodies carried by each Trident missile. Instead of ten, Trident would carry eight. Overall system effectiveness would be maintained by utilizing a higher yield warhead. Throughout his discussion Foster was firm with the AEC Chairman about the need to minimize total system costs, AEC as well as DOD, which he stressed "cannot be overemphasized."[45]

The End of Pie Splits

The AEC soon asked Los Alamos to take responsibility for the Trident warhead development, explaining this would strengthen laboratory competition and the two-laboratory system. Los Alamos' new assignment would

> contribute . . . to overall viability of the two-laboratory competitive concept. This concept has proven its value many times over in meeting national defense needs in imaginative ways which led to dramatic improvements in defense capabilities while at the same time reducing greatly overall system costs for achieving specific military effects.[46]

The decision represented a change in Livermore's longstanding claim to the strategic missile warhead field. The selection of Los Alamos over Livermore, however,

> should in no way be construed as a reflection on LLL capabilities and contributions. On the contrary LLL contributions have essentially monopolized strategic RV warheads for the last decade to the extent that the Mk 400 [Trident] is an appropriate opportunity for LASL to design one.[47]

The AEC also informed Foster of Los Alamos' new assignment.[48]

Los Alamos' new responsibility represented a major change from past practice, with important consequences. After developing the nuclear warhead for what came to be called Trident I, Los Alamos also won the Trident II warhead. In the future, Los Alamos will have a monopoly on future Navy strategic systems, which will comprise critical and substantial portions of the

45. John S. Foster to Dixy Lee Ray, 23 April 1973, with attached "Desired Characteristics for the Warhead for the Navy Mk 400 Reentry Body."

46. Frank A. Camm to Harold Agnew, Roger Batzel, et al., 27 April 1973.

47. Ibid.

48. Dixy Lee Ray to John S. Foster, 10 May 1973.

post-Cold War stockpile. In short, Los Alamos was "captured" by the Navy, as one Livermore scientist described it. And as more than one veteran of the laboratories' told me, Los Alamos became the "Navy's lab."[49] How did this happen?

The AEC had formal authority for selecting the laboratory that would develop new nuclear warheads. Formal authority, however, was just one important element of the decisionmaking process. The power of persuasion and influence is also critical, as Richard Neustadt has observed in his study of the powers of the president.[50] What role did each organization play in the Trident nuclear warhead development assignment, who tried to persuade whom, and with what consequences?

Pie splitting had characterized laboratory relations into the late 1960s. After Harold M. Agnew replaced longtime Los Alamos director Norris Bradbury in 1970 "all bets were off" with regard to pie splitting as one Los Alamos scientist described what happened.[51] An important factor was the changed composition of the stockpile. The strategic bombing mission had reigned supreme in the 1950s, and Los Alamos had developed all Air Force strategic bombs. By the 1970s the strategic missile leg of the triad had grown, both in strategic importance and in numbers of weapons. In fact, Navy strategic systems alone outnumbered strategic bombs by then (Figure 5).

Livermore's virtual dominance of the strategic missile warhead field, which had become among the most politically prestigious nuclear weapon systems, did not sit well with Los Alamos. One laboratory scientist recalls his feeling that by the end of the 1960s Los Alamos was playing "second fiddle" to Livermore.[52] Another felt it "vital to the lab that we reestablish our position" and aggressively pursue strategic missile warhead development assignments. By the early 1970s "the lab felt hungry," perhaps for the first time in its history.[53]

Agnew's rise to directorship of Los Alamos corresponded with the laboratory's need to become more attuned to military customers who had grown in importance since its early days. A former director of the Military Liaison Committee who had frequent interactions with the laboratory directors recalls Agnew's comfort with and good understanding of the military.[54] Unlike

49. Telephone conversation with John Harvey, Feb. 1991.

50. Richard E. Neustadt, *Presidential Power: The Politics of Leadership With Reflections on Johnson and Nixon* (New York: John Wiley & Sons, 1976), p. 78.

51. Interview with Del Bergen, LANL, 3 April 1991.

52. Telephone interview with Del Bergen, 20 June 1995.

53. Gillespie interview, 5 April 1991.

54. Telephone interview with W. J. (Jack) Howard, 17 June 1995.

Bradbury, Agnew "carried a lot of weight with the military," agreed the director of Los Alamos' Weapons Program office during the Trident deliberations.[55] And the Los Alamos director was a "favorite" of AEC Director of Military Application Edward Giller.[56]

It was Harold Agnew who finally established a "sales" division for Los Alamos comparable to Livermore's Military Application division established nearly a decade earlier. Called W-9, it was the first Los Alamos organizational division dedicated to providing information to the military about Los Alamos systems, learn about military interests, and perform systems analyses. Los Alamos, according to the recollection of the Los Alamos scientist in charge of W-9 during the Trident warhead decision had been

> . . . slow to recognize how complex things were getting in terms of [weapons development] phases. Things were a lot simpler when it was bombs that were being designed. Los Alamos was slow to recognize that it needed a dedicated interface with the military.[57]

Los Alamos often waited for the formal start of the phase 1 and 2 processes before actively engaging the services, whereas Livermore initiated interactions at an earlier and more informal level.[58] One Los Alamos scientist also recalls that prior to the establishment of the W-9 division, military representatives would not be invited to meet directly with representatives of the laboratory's weapons divisions when traveling to the laboratory. Instead, they would be escorted to see Jane Hall, Bradbury's Associate Director for Weapons, who would later pass information to the weapons division staff. It was considered a "waste of precious resources" to devote much effort to interactions with the military. Rather, Hall asked military representatives to "write us a letter, tell us what you want, and in a few years we'll give it to you." An admitted exaggeration, these recollections are indicative of the divergent approaches of the two laboratories.[59]

Los Alamos' approach was not ill-suited to its job. The Air Force was not enthusiastic about highly integrated systems.[60] Early bomb designs provided flexibility for both Los Alamos and the Air Force. Having agreed on the technical characteristics of the interface between the labora-

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55. Bergen interview, 20 June 1995.

56. Telephone interview with Edward B. Giller, 16 June 1995.

57. Gillespie interview, 5 April 1991..

58. Telephone interview with Milton Gillespie, 4 October 1991.

59. Gillespie interview, 5 April 1991.

60. Discussions at LLNL and LASL.

tory-supplied warhead and the Air Force's delivery system, each could independently develop the portion of the weapon for which it was responsible. With minimal system integration involved, the "bolt two lugs in it, hang it in the bomb bay" approach to weapons development worked, as one Los Alamos weapon designer recalls.[61] The Air Force's preference for incremental technical change prompted Edward Teller's derision and frustrated Livermore weapons designers, accustomed as they were to close technical and organizational integration in developing Navy systems.[62] Los Alamos' management and organization, however, had been formed in a different kind of environment than had Livermore's by virtue of the kinds of systems each developed. Extensive communication between the working levels of Los Alamos and the Air Force was not as critical in early bomb development. Modern bombs, however, are "constantly talking to the airplane." The shift to greater integration in modern systems was a difficult one for the laboratory, and the establishment of the W-9 division was part of this process.[63]

In any case, Agnew, who has been described as an "aggressive salesman," signalled to the key actors in the Trident warhead decision that he wanted Los Alamos to win the assignment.[64] He was the force behind a meeting sponsored by Los Alamos and attended by Navy and other key actors to discuss future post-Poseidon Navy programs.[65] R. Y. Kaufman, member of the Military Liaison Committee and Director of the Navy Strategic Submarine Division during the Trident warhead decision recalls Agnew's efforts to "drum up business." On a first class business flight arranged by Agnew, the Los Alamos director took the opportunity to raise the subject of Navy strategic systems. In an indirect reference to Polaris, Agnew reminded Kaufman of the problems that could arise when a "vulnerability in the warhead" surfaced in the fleet, urging him to consider the merits of procuring two nuclear warheads for Trident, one supplied by each laboratory. Today Kaufman believes problems experienced with Livermore-designed warheads were unrelated to the selection of Los Alamos to develop the Trident warhead.[66] Agnew, it seems, believed otherwise.

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61. Gillespie interview, 5 April 1991.

62. Interview with Edward Teller, Palo Alto, Cal., 12 Oct. 1991; discussions at LLNL and LASL.

63. Gillespie interview, 5 April 1991.

64. William J. Broad, "Present Since the Atom Was Split, Physicist Reflects on a Turbulent Era," *New York Times*, 1 Dec. 1992, Section B, pp. 5, 9 at 9, quotes Raemer Schrieber; Bergen interview, 3 April 1991, for signalling that Los Alamos wanted Navy warhead assignment.

65. Stinner interview, 21 June 1995.

66. Kaufman interview, 17 June 1995.

Picking Teams

SSPO had hoped to utilize a modified Poseidon W-68 warhead for Trident. Since the Navy already had "a very good warhead, why not go for the W-68?" as a former head of SSPO's reentry body section described SSPO's view.[67] Its preference for the W-68 indicates willingness to continue close cooperation with Livermore. In fact, Los Alamos scientists were persuaded SSPO's preference for the W-68 signified a preference for continuing its partnership with Livermore.[68]

Ironically, SSPO's desires to refurbish the W-68 set the Navy organization at odds with Livermore, perhaps for the first time in their joint history. Livermore had its sights set on designing a new warhead. The scientist in charge of preparing Livermore's contribution to the phase 2 study recalls "Livermore wanted to do something new . . . putting the [W-]68 on Trident was no challenge."[69] He might have added that refurbishing the W-68 for Trident would not have commanded the same level of resources nor been as prestigious as a new warhead program. High priority programs helped the laboratory "stay well" in times of relative fiscal constraint as were the early 1970s. New programs made it easier to justify added facilities and staff.[70]

Admiral Wertheim recalls some of the considerations that went into the Trident missile and warhead decision, particularly as they related to the two-laboratory system. He put Trident in its historical context, recalling the Navy's earlier experience with Polaris. Lockheed, the missile contractor, and the Navy, like Livermore, had all been trying to "prove" themselves to "the big boys." The Polaris success was a "beautiful example of the reason to have a second laboratory to try things for the first time." The Polaris A-3 missile, developed by Livermore in the early 1960s, had also been a first, carrying three nuclear warheads per missile. And when the Navy developed Poseidon, "we wanted to continue the tradition, but in taking it a step further, we went too far. We made something almost unproduceable." When thinking began on Trident, the SSPO wanted a "less stressing design." The high degree of integration in the Poseidon reentry body and warhead had led to the numerous production problems. In Trident, SSPO wanted a "much more conventional design." Cost-effectiveness was also very important to the program at that time. As Wertheim recalls, "we were looking for low-cost manufacture and not to take risks where we didn't need to."[71]

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67. Stinner interview, 21 June 1995.

68. Bergen interview, 20 June 1995.

69. Shuler interview, 23 June 1995.

70. Discussion with Carl A. Haussmann, LLNL, 23 June 1995.

71. Wertheim interview, 20 June 1995.

Livermore's preference for a new warhead undermined SSPO political objectives and its leverage with other segments of the Navy and DOD who wanted a new high yield warhead. It was also a source of aggravation for Robert Stinner, head of SSPO's reentry body section and the principal contact with the laboratories.

> There we were, trying to work with Lockheed to put together a proposal that was very important to us, that could sell in Washington, and Livermore was causing problems. . . . Things would come back to SSPO . . . it made my job more difficult. On the basis of things they would have heard from Livermore, people would ask, why are you doing things that way?[72]

How, exactly, did the Livermore-Navy split occur? How important were Navy preferences? SSPO was key, according to Admiral John T. (Chick) Hayward, long involved in developing the Navy's nuclear role and also an important actor in the founding of Livermore. SSPO Director Levering Smith would not have forfeited decisionmaking to the AEC when it came to the Trident warhead. As Hayward phrased it, "[y]ou didn't just deliver a weapon from the AEC and say to the Navy, 'take it'."[73] Stinner, then director of SSPO's Reentry Body Coordinating Committee, can be more specific. Smith's job promoting Trident, as he put it, was made more difficult by Livermore's promotion of a new high yield warhead and he grew "sick of hearing about Livermore not wanting to refurbish the W-68."[74] William Shuler of Livermore confirms the spirit of Stinner's account, recalling how the laboratory was "hyperactive in getting the Navy to agree to a new warhead."[75]

Livermore lost its privileged "team player" status and SSPO began to integrate Los Alamos into the Navy strategic missile program. SSPO director Smith asked Stinner to invite Los Alamos representatives to Project Officers Group (POG) meetings, Los Alamos' first opportunity to do so. The POG met every couple of months at various facilities, including at Lockheed and Livermore, to review FBM program progress and to discuss critical technical issues and decisions. It was an important forum for learning about Navy concerns, for getting to know the principals involved, and for becoming part of the SSPO team. Inviting Los Alamos to the POG meetings was intended to "send a signal" that SSPO was "not happy with the activities of the

72. Stinner interview, 21 June 1995.

73. Telephone discussion with John T. (Chick) Hayward, 20 June 1995.

74. Stinner interview, 21 June 1995.

75. Shuler interview, 23 June 1995.

Military Applications group at Livermore."[76] Stinner was referring to Livermore's "marketing" division, then under the leadership of Charles (Chuck) McDonald. Although Stinner singled out the director of Livermore's Division of Military Application, the results of this study suggest that underlying issue was not principally one of personalities. Rather, Livermore's actions should be viewed principally as the outcome of the structure and dynamics of the two-laboratory system. As this analysis has shown, Livermore throughout its history has faced the need to be more aggressive and pro-active in winning weapon development assignments than has Los Alamos.

The observations of Robert Wertheim about the different roles and approaches of the two laboratories, their relationships to the Navy, and how these changed over time, support this conclusion. Wertheim had long experience with both laboratories, having worked closely with Livermore on the Polaris and Poseidon warhead developments and with Los Alamos on Trident. He describes Los Alamos as a "more disciplined" organization, Livermore as more "free-wheeling . . . less inhibited." The Navy "gravitated" to Livermore early on when it first sought to establish a role in strategic missiles. For their part, the Navy's FBM staff were "upstarts . . . we had to prove ourselves with the big boys" in the Air Force. All involved were doing things for the first time with Polaris: Livermore, Lockheed, and the Navy." No one was "hung up about how we've always done things." The Navy was "second cousins" to the Air Force, much as Livermore was to Los Alamos. Once established, however, the Navy could afford to be more conservative. The Trident missile warhead development program was carried out under much more "normal" arrangements. The time allowed was more reasonable, and "risks had to be managed more carefully." Wertheim acknowledges the Navy was "a little nervous" at first about working with Los Alamos. Without actually stating that the Navy preferred one laboratory over the other, however, he agreed that one laboratory's approach might have been more appropriate than the other at different phases of the Fleet Ballistic Missile program.[77]

What was the role of the AEC in the Trident warhead decision? The Division of Military Application was the AEC entity with formal authority to recommend which laboratory would develop a nuclear warhead. DMA was comprised largely of military officers on loan from their respective service organizations. Like others in the Division of Military Application, for example, Edward Giller was on loan from the uniformed military. His ties to Los Alamos were strengthened by prior service as Commander of the Air Force Special Weapons Center in Albuquerque. Giller and others were likely to be sympathetic to military concerns. The principals involved in the Trident warhead decision—Division of Military Application director Frank Camm and SSPO's Wertheim—had known each other since the 1940s when both served at Sandia Base

76. Stinner interview, 21 June 1995.

77. Interview with Robert H. Wertheim, San Diego, 19 March 1991.

in Albuquerque as members of service teams assembling the earliest nuclear devices.[78] Both were then junior officers in the Army and Navy respectively. Camm described himself, as "on loan from the Army . . . [but] an agent of the AEC."[79] It was important to Camm that all parties felt they had been treated fairly. This was facilitated by ensuring that decisions were justified on a basis acceptable to all. As one Los Alamos scientist recalls, Camm "wanted a proper relationship between the laboratories and the services, and didn't want any problems." Camm also knew he would return to the Army. He was a "soldier, no doubt about it, but he knew how to play politics." He was soon promoted to Major General following his tour at DMA.[80]

Service nuclear warhead preferences were usually pretty clear to the laboratories, at least nearing the time a decision would be made. Each laboratory therefore routinely included at least one among its list of warhead options that met military preferences. As a consequence, laboratory proposals could often look very similar.[81] It was also usually the case, however, that one among each laboratory's options represented its most serious proposal. It might build on the design of a warhead previously developed by the laboratory, for example, or on past nuclear test experience. Both laboratories were technically competent, so the AEC could easily defend the selection of either laboratory to develop a given warhead. It was rare, however, for one laboratory to develop a warhead designed by the other, although some designs might rely heavily on the results of the other's nuclear tests. In fact, the laboratories resisted such arrangements. Doing so would have made it more difficult to assign credit.

Senior service representatives would not normally state an explicit preference for one laboratory over the other, at least not on record. All sides agree, however, that the services had substantial input in warhead selection. Given that the laboratories rarely developed each other's warhead designs, this meant, effectively, that the service selected the laboratory that would develop its new warhead.

Camm insists the AEC was responsible for the decision to assign the Trident warhead to Los Alamos although he acknowledges the armed services had substantial input.[82] The SSPO's Admiral Wertheim agrees.[83] Edward Giller, DMA director prior to Camm during the last part of

78. Wertheim interview, 20 June 1995.

79. Telephone interview with Frank A. Camm, 21 June 1995.

80. Bergen interview, 20 June 1995.

81. Shuler interview, 23 June 1995.

82. Camm interview, 21 June 1995.

83. Wertheim interview, 20 June 1995.

the Polaris warhead retrofit, was not involved in the Trident decision. Based on his experience, however, he acknowledges the possibility the Navy "might have had a preferred lab and that it may have had an impact on our [AEC] decision." The AEC, according to Giller, was "not in the practice of imposing decisions [on the services that] it couldn't defend."[84]

When the time came for the AEC to make a decision, each laboratory would present their respective proposals to DMA and service representatives. DMA staff would also meet informally with armed service representatives to learn about their off the record. Wertheim recalls that during such meetings service representatives "weren't bashful about telling them [AEC] what we wanted."[85] The AEC, however, would not tolerate any "shoving."[86] This meant that tact and political skills were required. Levering Smith, for example, would not have told the DMA's Frank Camm directly that the Navy did not want to work with Livermore on Trident, as Stinner explains. Rather, Smith might have outlined the consequences of various choices, thereby making his preferences clear. Having reached an understanding about Navy preferences through informal interactions the AEC might have said something like the following: "'There are good reasons we would like to use Los Alamos—do you [the Navy] have any objections?' and the Navy would have said no."[87]

Stinner was not surprised about the decision to select Los Alamos to develop the Trident warhead: "I saw it coming. . . . You try to send signals and for whatever reason you don't get through." Stinner mused that perhaps Livermore and Chuck McDonald "didn't have any choice." Either the laboratory pressed for a new warhead, or it did not gain substantial new responsibilities. In the end, Livermore's opposition to the W-68 refurbishment option was the critical factor in SSPO thinking: "We couldn't get Livermore on the team—that was the factor that led to the desire to get some competition in and invite Los Alamos" to participate. The consequences were profound in that Livermore's wanting a new warhead "opened the door to Los Alamos."[88] Los Alamos' test of a high yield warhead also helped. Without it, justifying its competence to develop the warhead would have been more difficult.[89]

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84. Telephone interview with Edward B. Giller, 16 June 1995.

85. Wertheim interview, 20 June 1995.

86. Bergen interview, 20 June 1995.

87. Stinner interview, 21 June 1995.

88. Ibid.

89. The Los Alamos test had been aimed at a possible decision to deploy a high yield warhead for the Minuteman 3 missile. Had a new warhead for Minuteman 3 been approved, the tacit understanding was that Los Alamos would win the assignment. The warhead was never approved, although Los Alamos' nuclear test experience lent credence to its argument that it was capable of developing the high yield Trident I warhead.

Laboratory cooperation and "pie splits" ended with Trident and the AEC took a stronger role in allocating weapon development responsibilities. This was not an outcome welcomed by Livermore. As former Livermore director Michael M. May recalls, the laboratory was "shook up by Los Alamos getting the Trident assignment."[90] From Livermore's perspective, the "pie splits between the labs [had] worked out well. The pie splits from D.C. never worked as well."[91] Or as one Livermore scientist described the situation in the 1950s through early 1970s: "we chose what we wanted and Los Alamos was perfectly happy with the low-leverage work."[92] Things changed after Trident when Livermore began "taking every phase 3 [weapons development assignment] that Los Alamos didn't want."[93] As the preceding narrative on Trident indicates, however, the laboratories did not control warhead development assignments to the extent they might have thought, even during the era of pie splitting. Always present were the preferences and bureaucratic interests of the services and their subunits such as the Navy's SSPO.

Aftermath

Having lost Trident Livermore sought to realign its programs in accord with the interests of other military customers. The laboratory turned its focus to the development of atomic artillery for the Army, much as it had in the 1950s. This time, the effort called for developing the enhanced radiation weapon. The history of this technology dated to the mid-1950s when public concerns regarding radioactive fallout prompted the AEC to request laboratory investigation of clean weapons, as discussed in Chapter 8.[94] Clean weapons were designed to have reduced radioactive fallout and one form of these was the enhanced radiation weapon, also called the neutron bomb.

Despite their feasibility, enhanced radiation weapons had been deemed "cost-ineffective" compared with high yield fission warheads. The Air Force was unenthusiastic since the dominant philosophy was to get as much "bang per buck" from nuclear material and lean weapons were relatively expensive in nuclear for given yields. In the 1940s and 1950s, the Army had fought

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Telephone interview with Harry Hoyt, 27 June 1995; Hansen, *U.S. Nuclear Weapons*, p. 201.

90. Interview with Michael M. May, LLNL, 20 March 1991.

91. Interview with Carl A. Haussmann, LLNL, 26 Feb. 1991.

92. Interview with Carl A. Haussmann, LLNL, 4 Oct. 1990.

93. Interview with William B. Shuler, LLNL, 25 July 1991.

94. Ruth R. Harris and Richard G. Hewlett, "The Lawrence Livermore National Laboratory: The Evolution of its Mission, 1952-1988," report for LLNL (Rockville, Md.: HAI, 21 March 1990), p. 5.

a hard battle against this argument and was reluctant to revive it in the 1970s. Proponents of enhanced radiation warheads, however, pointed to their reduced propensity to destroy infrastructure and their relatively low contamination properties which made them desirable battlefield weapons.

Things changed in 1973 when the JCAE's Subcommittee on Military Applications chaired by Senator Stuart Symington conducted hearings on new weapon requirements. Los Alamos and Livermore both were then developing the next generation atomic shells, but the program was effectively cancelled when the committee turned down the Army's proposal to "modernize" its 8-inch and 155 mm atomic shells, deeming these standard fission weapons "obsolete."[95] The Army then found itself in a difficult situation. Without approval of new atomic artillery shells its nuclear role in Europe would come into question. As one participant recalls, "[a]n alternative had to be found quickly." The JCAE action prompted the Secretary of Defense to ask the Army to reexamine atomic artillery requirements, making it clear the guiding criteria should be to minimize collateral damage and utilize radiation as the kill mechanism. Enhanced radiation weapons were the only available technology that would satisfy these requirements.[96]

The Army's dilemma provided an opportunity for Livermore, which "corner[ed] the nuclear artillery shell market" recalls Livermore scientist Carl Haussmann. Livermore nuclear weapons program director Harry Reynolds "push[ed] . . . this assignment," to the disgruntlement of some at laboratory who deemed these systems low prestige. But in 1973 Livermore began development of the W-79 enhanced radiation warhead for the Army's 8 inch projectile. They "were not the world-wide meaningful" systems, but the laboratory had little choice: Los Alamos had encroached into the strategic missile warhead business.[97] Reynolds reportedly won leadership of the nuclear weapon program over Chuck McDonald because the latter "lost" the Trident warhead to Los Alamos.[98] Reynolds' rise to leadership position suggests Livermore's management understood his interests and experience were will suited to laboratory needs. As Charles Perrow has argued, organizations tend to be controlled by those individuals or groups who perform the most difficult or critical tasks. A task is critical not only because the organization will cease functioning if it is not performed, but because it represents the distinctive problem faced by the organi-

95. The W-74 155-mm shell and the W-75 8-inch shell. For a history of the enhanced radiation (or neutron) bomb by a participant, see S. T. Cohen, *The Neutron Bomb: Political, Technological and Military Issues* (Cambridge, Mass., and Washington, D.C.: IFPA, 1978).

96. Cohen, *The Neutron Bomb*, p. 33-34.

97. Haussmann interview, 4 Oct. 1990.

98. Shuler interview, 25 July 1991.

zation at its particular stage of development.[99] Livermore needed to adapt to changes in the weapon development workload.

Throughout the 1970s, Livermore developed warheads for weapon systems many laboratory scientists did not consider important relative to strategic systems. Their unstable political support influenced such thinking. The enhanced radiation artillery and tactical systems, for example, were "technically challenging, but high risk as far as longevity [in the stockpile] was concerned. . . . A fair number of systems Livermore got disappeared [from the stockpile.]" By the 1980s, this was perceived as problematic. Laboratory managers grew concerned about Livermore's responsibility for "non-visible systems."[100] As it had in the past, the laboratory took opportunities where it found them. Ronald Reagan's ascendancy to the presidency was one such opportunity to search for program support outside traditional Department of Defense channels. Edward Teller's widely publicized remarks about the feasibility of space-based strategic defense was typical of the laboratory's more aggressive approach and some believe Teller was crucial in persuading President Reagan to announce an initiative meant to render nuclear weapons "impotent and obsolete."[101] The Strategic Defense Initiative (SDI) was envisioned as a comprehensive ballistic missile defense system and Livermore won substantial funding to investigate one piece it. The nuclear-pumped x-ray laser was once an SDI centerpiece though it which eventually became marginal to the program.

Livermore also aggressively promoted a new warhead for the Air Force's MX missile. Early Air Force plans had not included a new warhead for MX which instead was slated to use the Los Alamos Minuteman III warhead. But Livermore promoted a new warhead designed to include a number of new features, including insensitive high explosive (IHE).[102] The laboratory's efforts were not appreciated by the head of the Air Force Ballistic Missile Office who claimed the decision for a new warhead, though attributable to a variety of causes, was "driven by the appetite of the DoE labs, particularly Livermore, for more work, not [by] military requirements." Livermore won the MX warhead assignment, the only new strategic missile warhead the laboratory placed in the stockpile since the mid-1970s. It increased the cost of the MX missile program by one billion dollars when the costs to DoE and the Air Force development of the new reentry ve-

99. Charles Perrow, chap. 4, "Goals and Power Structures: A Historical Case Study," p. 113.

100. Shuler interview, 25 July 1991.

101. Speech by Ronald Reagan, Washington, D.C., 23 March 1983.

102. Insensitive high explosive is much less likely to be detonated by fire or impact than the high explosive in nuclear warheads that entered the stockpile prior to 1978. See Sidney D. Drell, John S. Foster, and Charles H. Townes, "Report of the Panel on Nuclear Weapons Safety of the House Armed Services Committee," Dec. 1990, pp. 38-40.

hicle required were counted.[103] Only 50 of the 200 missiles first sought were deployed, nominally carrying 500 warheads.

Did Livermore bear primary responsibility for the MX warhead outcome? Not likely. The evidence considered in this analysis simply does not support an argument for that level of laboratory influence. What is more likely is that laboratory interests were shared by others who also wanted a new weapon system, strengthening their common cause. This would be consistent with patterns of alliances already considered, including Livermore's cultivation in the 1950s of Navy strategic missile advocates and Army supporters of atomic artillery. One important ally in the MX case was James P. Wade, Assistant to the Secretary of Defense for Atomic Energy, a strong supporter of Livermore's proposal for inclusion of IHE and other features. Reentry vehicle politics may also have been an important factor in the MX warhead pattern of alliances. The Livermore warhead would not fit on the proposed General Electric reentry vehicle. The laboratory's interests were thus more naturally allied with those of the Avco Corporation, which won the reentry vehicle contract.[104]

Livermore interests sometimes worked against those of potential sponsors. In such cases the laboratory was likely to lose its bid for a new warhead. There is a neat symmetry to Livermore's relationship with the Navy which illustrates the point. In the Polaris and Poseidon cases Livermore and the Navy Strategic Systems Projects Office interests were well matched. In the Trident case, however, Livermore interests conflicted with SSPO's, and Livermore lost the warhead assignment to Los Alamos. When it came time for the Trident II warhead development program, the Navy and Los Alamos continued their cooperative working relationship. The Trident II account does not end with deployment of the Los Alamos-designed W-88 warheads in the late 1980s. The Rocky Flats production plant shutdown halted Trident II warhead production indefinitely, leaving the fate of future Trident II warheads unknown. Livermore saw this as an opening to suggest development of a new warhead and the laboratory proposed that future Trident II missiles be fitted with a new "safer" Livermore warhead incorporating IHE. Warhead safety had become a subject of public debate, surfacing on the congressional agenda largely at the laboratory's instigation. In the late 1980s Livermore revealed that a warhead of its own design still in the stockpile was not one-point safe.[105]

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103. Alovsius G. Casey to James D. Watkins, 20 Jan. 1989.

104. Shuler interview, 25 July 1991. For information on the MX see Cochran et al., *U.S. Nuclear Forces and Capabilities*, pp. 120-131.

105. George H. Miller, Paul S. Brown, and Carol T. Alonso, *Report to Congress on Stockpile Reliability, Weapon Remanufacture, and the Role of Nuclear Testing* (Livermore: TID, Oct. 1987), p. 19.

Livermore's proposal to develop a new Trident II warhead created great consternation for the Navy, which resented having to argue against the laboratory that its nuclear warheads were indeed safe. New Livermore director John Nuckolls was reportedly surprised at the vehemence of the Navy's response, expecting Livermore would be seen as a champion of nuclear safety. But this was simply another example of Livermore's "not being very sensitive to Navy needs," recalls one of the Livermore scientists involved.[106] It created a myriad of political problems for the Navy and provided ammunition for Trident critics. As this study has argued, however, the issue was not one of laboratory sensitivity but of the structure of laboratory competition and the two-laboratory system. For better or worse, Livermore actions grew out of its role as the second laboratory.

Livermore's strategy aggravated relations with Secretary of Energy Admiral James D. Watkins who had been Chief of Naval Operations in 1982 at the time of the Navy decision against inclusion of IHE in the Trident II warhead. This placed Watkins in a doubly difficult position when Livermore raised the issue during his tenure as Secretary of Energy. For its part, the Navy was reportedly so angered by Nuckolls' proposal that it refused to continue providing information directly to Livermore. The laboratory, which had once offended SSPO by pressing for a new warhead for Trident I, had now offended the highest levels of the Navy as well as the Department of Energy in failing to be a "team player." The Navy thereafter insisted Livermore obtain needed information about Navy systems from DoE instead of enjoying the convenience of obtaining it directly.[107]

Customers in Control

The factors that went into the AEC's decision to assign the Trident warhead development to Los Alamos reveal important features of the two-laboratory system. The transfer of the Navy's business from Livermore to Los Alamos was a two-stage process beginning with the Trident I warhead assignment. The shift would not have occurred had the Navy not wanted it. Military preferences, particularly those of high priority customers, were the most important determinant of which laboratory developed a given nuclear warhead. Livermore and the Navy had been a "perfect match" in the late 1950s. Each needed the other in order to succeed. A rift occurred in this relationship in the early 1970s, with important consequences for both laboratories. The decision to assign the Trident I warhead development to Los Alamos was a crucial turning point in the history of both laboratories. Warheads of Los Alamos design will comprise the larg-

106. Shuler interview, 25 July 1991.

107. Ibid; and other LLNL discussions.

est fraction of total warheads in the stockpile into the foreseeable future (Figure 3).[108] The Trident I and Trident II nuclear warheads alone, both of Los Alamos design, will comprise about one-third of the future total when arms reduction treaties are implemented.[109] Implementation of the Strategic Arms Reduction Treaty (START I) would leave Los Alamos with five warheads of its design in the stockpile, Livermore with four. If history is any guide, Livermore systems are more likely to be eliminated than Los Alamos' should a future decision be made to reduce the stockpile further for arms control or other reasons placing Los Alamos in a better position to argue for greater resources. The challenge to Livermore as the "second lab" are thus likely to continue.

108. Los Alamos was responsible for seven of eleven warheads in the stockpile as of winter 1995. See SEAB, Task Force on Alternative Futures for the Department of Energy Laboratories, chaired by Robert W. Galvin (Galvin Committee), *Alternative Futures for the Department of Energy National Laboratories*, Report to the Secretary of Energy, 2 vols., Feb. 1995, p. 14.

109. Under START I, Los Alamos-designed nuclear weapons remaining in the stockpile will be the B-61 tactical and strategic bombs, W-76 Trident C-4 warhead, W-78 Minuteman III warhead, W-80 Tomahawk and ALCM warhead, W-88 Trident II warhead. Livermore-designed systems will be the W-62 Minuteman III warhead, B-83 strategic bomb, W-84 GLCM warhead, W-87 MX warhead.

10. MAKING WEAPONS
Origins, Character, and Consequences of the Two-Laboratory System

Despite essentially identical missions, the Lawrence Livermore and Los Alamos laboratories adopted different strategies and approaches to their task. Why? I looked to their joint histories for answers. How did the two-laboratory system originate and evolve? How did the system function? What impact did it have on the number and kind of nuclear weapons developed? My analysis shows that the structure and dynamics of laboratory competition was key. The political decision to maintain civilian control removed the weapon development program from competition with other military programs, creating an essentially free military resource. At the same time, laboratory competition was shaped by what the military wanted, since the military was the ultimate arbiter of the nuclear program's merits. The process was structured by the AEC, which sought to portray an orderly process and encourage laboratory cooperation while reaping the benefits of competition. Program funding and even survival were at stake for the laboratories, guiding organizational strategies. Sometimes competition was called for, increasing policy maker leverage and information. Sometimes cooperation seemed best, in which case information and decision making was confined to well defined groups.

Origins of the Two-Laboratory System

Explaining the origins of an organization requires understanding the interests of the organizational actors involved and their ability to get what they want. Founding a new organization is a complex process involving numerous organizational actors, not all of whom agree on the desired outcome. The form and purpose of the new organization will reflect the interests of these actors and their bureaucratic skills in achieving their goals. Explaining the origins of an organization can illuminate who these actors are and their interests. This will also help explain the organization's early form and function, both of which can have lasting impact. An organization is shaped by its need to survive in a specific social, economic, or political environment.[1] Understanding its relationship to that environment will explain much about the organization.

The Atomic Energy Commission, the entity with management responsibility for the nuclear weapons complex, initially opposed the establishment of a new nuclear weapon design laboratory. The commissioners sided with Los Alamos, who feared the impact of a second laboratory on the H-bomb program. The Los Alamos director warned a rival would compete for resources, jeopardizing his laboratory's chances of success. Proponents believed a second laboratory would speed the process, not hinder it. Supporters on the Joint Committee on Atomic Energy believed laboratory competition would invigorate the nuclear weapons program. Besides, they saw it as an opportunity to expand the weapons complex and their committee's reach.

Edward Teller, Hungarian-born physicist and Manhattan Project participant, was an early and ardent advocate of establishing a new laboratory, and Livermore is often referred to as "Teller's lab." As demonstrated in Chapter 2, however, Teller's efforts were unsuccessful until senior Air Force officials joined the cause. Interservice rivalries help explain Air Force advocacy of the proposal. Defending its strategic bombing role against Army ambitions for a role in the tactical nuclear defense of Europe, the Air Force went on the offensive. Conflicts over allocation of resources for fission and thermonuclear weapons development were played out at Los Alamos where deliberations over the size and scope of the weapons program reflected these larger debates. Robert Oppenheimer's advocacy of tactical nuclear weapons only aggravated the situation. Los Alamos became implicated through its association with the scientist, the laboratory's first director.

Los Alamos might have blunted Air Force criticism of its H-bomb program had laboratory leaders been more demonstrably responsive to Air Force officials. But their monopoly on nuclear weapon development gave laboratory leaders little incentive to be overly solicitous. In fact, Los Alamos leaders viewed military inquiries about laboratory programs as intrusive, fueling Air Force interest in a new laboratory. Organizational and programmatic changes suggested by the AEC might have helped defuse military concerns. But they were rebuffed by laboratory leaders. Los Alamos' technical success in developing the H-bomb suggests it had nothing for which to apologize. The problem, however, was not so much technical as it was political. Laboratory

1. As discussed in W. Richard Scott, *Organizations: Rational, Natural, and Open Systems*, 3rd ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1991), p. 111.

leaders might have handled the situation with more political skill, but there is nothing they could have done about what was fundamentally at stake: nuclear weapons resources and missions.

The turning point for the second laboratory occurred when the Department of Defense agreed to back the proposal. Justified or not, continued DOD criticism of the H-bomb program and the perception that Los Alamos was not adequately meeting its responsibilities would reflect badly on the AEC. Although DOD's commitment was not as strong as the Air Force's, it was nevertheless a powerful actor. The boundaries between civilian and military control of atomic weapons were matters of ongoing debate and in continual flux. The AEC did not want to open an opportunity to erode these boundaries further. DOD's suggestion that the H-bomb and second laboratory proposals be considered in the National Security Council raised the prospect of the president's involvement. Wishing to avoid bringing the issues to the president for arbitration, a discussion likely to leave its reputation and credibility tarnished, the AEC finally acted. It thereby defended its organizational integrity and retained control of the nuclear weapons program when it agreed to establish a new nuclear weapons research center separate from Los Alamos. This was the first instance, but not the last example of how the military helped shape the nuclear weapons program. DOD leverage over the AEC was mitigated by DOD's unwillingness to acquire the responsibility—and the costs—of developing nuclear warheads. The matter was of great consequence for the AEC but of little ultimate import to DOD or the Air Force. The relative importance to the Air Force and the AEC of these issues is illustrated by the fact that Livermore's founding barely merits mention in Air Force histories, but is discussed at some length in AEC histories.[2]

The desire to maintain the health and vigor of Los Alamos continued to shape AEC actions, with consequences for Livermore's development and for laboratory competition. Initially, the AEC insisted Livermore was not <u>the</u> second laboratory, but a supplement to Los Alamos. Establishing Livermore as a branch of the University of California Radiation Laboratory did not require major resource commitments, one of the attractive features of UCRL solution. The AEC did not inhibit Livermore's expansion, but neither did it actively promote the laboratory's growth, at least early on, and approval of key facilities was often slower than laboratory leaders

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2. See, e.g., Lee Bowen, *The Development of Weapons*, vol. 4 of "A History of the Air Force Atomic Energy Program, 1943-1953 in Five Volumes" (Air Force Historical Division, 1959). This history, which addresses the Air Force atomic weapons program explicitly, does not discuss the Air Force role in the founding of Livermore. Even the DOE-sponsored history, Richard G. Hewlett and Francis Duncan, *Atomic Shield, 1947-1952*, vol. 2 of "A History of the United States Atomic Energy Commission (Berkeley: Univ. of California Press, 1990), cites Teller as a principal source for the Air Force role in the founding of Livermore. Interestingly, Ivan A. Getting does not mention Teller, but rather emphasizes the role of David Griggs, Air Force Chief Scientist, in the founding of Livermore. See Jacob Neufeld, ed., *Reflections on Research and Development in the United States Air Force, an Interview with General Bernard A. Schriever and Generals Samuel C. Phillips, Robert T. Marsh, and James H. Doolittle, and Dr. Ivan A. Getting, conducted by Dr. Richard H. Kohn* (Washington, D.C.: Center for Air Force History, 1993), p. 49.

would have liked. But growing military demand for nuclear weapons fueled by Eisenhower's New Look policies helped secure Livermore's place as the second laboratory and most necessary facilities were in place by the late 1950s.

No specific charter or responsibilities beyond providing diagnostic support for Los Alamos H-bomb tests were in place when work began at Livermore. Its leaders recognized, however, that laboratory growth would depend on showing utility to the military, which meant developing weapons for the stockpile. The AEC was reluctant to commit to a specific charter but pressure from DOD soon prevailed, ensuring a thermonuclear weapons research program would be part of the new laboratory's informal charter.

Different expectations ruled the AEC's outlook on each laboratory, another important factor shaping Livermore's development and the two-laboratory system. AEC reluctance to commit to a full-scale second laboratory, its small and inexperienced staff, and lack of facilities precluded Livermore's full participation in the nuclear weapons program for several years. Meeting military requirements for nuclear weapons was the AEC's highest priority, which had little choice but to continue relying on Los Alamos after Livermore's founding. Different expectations also explain AEC tolerance of what might be described as Livermore's test "failures" in 1953 and 1954, which might have been used as a reason to curb Livermore's further growth. Instead, they attested to the AEC's commitment to the second laboratory and the investigation of new ideas. Livermore thereby served as a kind of buffer against further military interference in AEC management of the nuclear weapons program. Edward Teller's presence was also a kind of seal of approval, especially since he had not initially favored the Livermore option.

Perhaps the most important factor shaping laboratory competition was the informal AEC mandate to avoid program duplication. Overlapping programs could seem wasteful to congressional sponsors and create laboratory competition for resources. Because Los Alamos had a well defined weapons program by the time Livermore was established, the burden fell largely on Livermore to adhere to the mandate against overlapping programs. Laboratory leaders tailored Livermore's weapon program accordingly. Teller, for example, early on assured Commissioners Livermore's first thermonuclear test device would "not entail competition with Los Alamos for materials . . . in short supply."[3] Or as one Livermore scientist phrased it, "If you're going to build bombs don't build them like Los Alamos bombs."[4]

In short, the origins and early character of the two-laboratory system grew out of the bureaucratic struggle between the AEC and Department of Defense over the pace and direction of the U.S. nuclear weapons program. DOD pressure on the AEC led to the creation of Livermore,

3. AEC Meeting No. 744, 8 Sept. 1952, p. 461.

4. Interview with Frank Eby, LLNL, 12 March 1991.

and its continued watchfulness ensured Livermore's growth and independence from Los Alamos. At the same time, the AEC continued to rely on Los Alamos to fulfill top-priority military requirements, at least at first. In the meantime, the "second lab" investigated new weapons ideas and served as a kind of insurance policy against further direct military interference.

How the System Worked

The central problem for complex organizations is to control uncertainty.[5] They will thus seek self-control or the ability to act independently of environmental forces.[6] The founding of Livermore was the outcome of competing organizational strategies for dealing with uncertainty. The AEC wanted to avoid getting the president involved in a joint AEC-DOD discussion of the H-bomb program; Los Alamos leaders did not want a rival; the JCAE sought to expand its domain; the Air Force was concerned about interservice rivalries; and DOD sought to exert control over the AEC, a bureaucracy over which it had no direct authority but on which it depended for nuclear weapons.

Competition creates a particular kind of uncertainty for organizations, linking their survival to actions of others over which they have no formal authority or control. When organizations compete for the resources of the same sponsor or sponsors they are in what is defined as a relationship of competitive interdependence.[7] Richard M. Cyert and James G. March observed that organizations will try to negotiate with other organizations under such circumstances.[8] Jeffrey Pfeffer and Gerald R. Salancik argue similarly that cooperation is a typical solution to the problems of uncertainty created by organizational interdependence.[9] Cooperation is used here to describe the mutual commitment of two or more organizations to exchange the capacity to reduce uncertainty. Conversely, competitive strategies reduce uncertainty through unilateral action. Under what conditions did the laboratories cooperate, and when did they compete? How was this linked to environmental uncertainty?

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5. James D. Thompson, *Organizations in Action: Social Science Bases of Administrative Theory* (New York: McGraw-Hill, 1967), pp. 1-13.

6. Harvey M. Sapolsky, *The Polaris System Development: Bureaucratic and Programmatic Success in Government* (Cambridge: Harvard Univ. Press, 1972), p. 252.

7. As discussed in Scott, *Organizations*, p. 198.

8. Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 119-120.

9. Jeffrey Pfeffer and Gerald R. Salancik, *The External Control of Organizations: A Resource Dependence Perspective* (New York: Harper & Row, 1978), pp. 42-43.

This study considered two sets of competing organizations, and their strategies for relating to each other. The armed services competed among themselves for resources and prestige, just as did the two nuclear weapons design laboratories. The interests of the two sets of organizations were linked because the laboratories supplied nuclear weapons to the military services. The health and legitimacy of the laboratories depended on winning nuclear weapons development assignments, while the health of the services depended on their success in persuading DOD and congressional sponsors of the legitimacy of their claims to resources and military missions. Staking out a role in nuclear strategy could lead to increased service resources overall, at least during the New Look era. Patterns of cooperation and competition can be complex under these circumstances.

Relations between customers and suppliers—the armed services and the laboratories—followed classic patterns of bureaucratic politics. Coalition building, the importance of alliances, political interests, organizational objectives, all came into play in determining organizational survival strategies in particular environments. The nuclear weapon design laboratories constituted a substantial bureaucratic force in promoting their organizational interests. The armed services, however, retained the upper hand. Where their interests were engaged, the laboratories thrived, where they were not, the laboratories adapted, as shown in the historical analysis. It would be a mistake to treat the services as monolithic, however, since they are themselves comprised of various interests which compete among themselves. This further complicated patterns of cooperation and competition among and between the services and the laboratories. In their search for new weapons development assignments, for example, the laboratories sought alliances with elements in the services favorable to new weapons development. This could set a laboratory at odds with other elements in the service that might not have wanted a new warhead.

Growing military demand for nuclear weapons in the 1950s prompted an AEC shift in outlook towards Livermore. Instead of a drain on resources and a threat to Los Alamos, Livermore was now viewed for its potential to offer design and development capacity military customers wanted. The AEC took action to speed Livermore's transition from an exploratory venture into a full-scale weapons development laboratory, approving key facilities and programs. Livermore's first weapon development assignment in 1955 promptly raised questions about the allocation of weapons development responsibilities. Cooperation was the solution favored by the laboratories and encouraged by the AEC. It provided a mechanism for avoiding program duplication, important to the AEC when it came to justifying programs to Congress. Laboratory cooperation helped the AEC manage laboratory workloads and conveyed professionalism and control, minimizing outside scrutiny and interference. In short, cooperation reduced uncertainty for the laboratories as well as for the AEC.

The "pie split" procedure had different advantages for each laboratory. The process helped legitimize Livermore's existence and its leaders argue for resources and facilities. Los Alamos benefitted because it helped reduce the laboratory's workload. Cooperation also offered Los Alamos greater control over the process, which would otherwise have been left to the AEC. In any case, the AEC had formal authority to make weapon development assignments, though it was reluctant to act against military wishes. AEC choices were thus guided by the standing of the particular service or program, and high priority military customers and program needs were met first. The armed services seemed to have greater confidence in Los Alamos, particularly in Livermore's earliest years given the senior laboratory's established track record. The Air Force's reaction to the joint laboratory suggestion that the Titan I nuclear warhead development be transferred from Los Alamos to Livermore is illustrative. The Air Force ultimately agreed, but not until the AEC persuaded it that other high priority Los Alamos Air Force programs would suffer if it did not. Los Alamos thus tended to acquire the immediate high priority assignments, leaving Livermore the speculative, technically challenging, lower priority projects.

Laboratory cooperation had one significant drawback. It undermined the political justification for the two-laboratory system: the benefits to the nuclear weapons program of laboratory competition. Laboratory cooperation was also risky, however, potentially exposing the AEC to critics who argued the two-laboratory structure was redundant. Which threat was larger depended on the political environment, and throughout the history of the two-laboratory structure the AEC and the laboratories have had to balance these dual imperatives.

James D. Thompson argued that organizations competing for support seek prestige.[10] It was issues of laboratory prestige that sped the end of laboratory pie splitting and growing competition in the early 1970s. Throughout most of the Cold War strategic weapons constituted the core of nuclear deterrence and were considered the most prestigious and high priority weapon systems. Air Force strategic bombs constituted the mainstay of strategic forces, far outnumbering warheads in the stockpile into the 1960s. The system was stable through the 1960s because military preferences and demands remained stable. Tactical systems grew in importance although they were considered lower priority than strategic systems. What did change was the relative status of strategic bombs and strategic missiles, and in particular, the importance of the Navy's Fleet Ballistic Missile system. No longer second in line behind the Air Force, the Navy had gained political clout. The growing importance of strategic missiles left Los Alamos dissatisfied with its share of strategic missile warhead development, especially given Livermore's monopoly on sea-based systems. Declining military demand for new nuclear weapons in the 1970s and shrinking military budgets contributed to Los Alamos' dissatisfaction. Competition replaced cooperation as the predominant characteristic of laboratory relations, although both were always present.

10. Thompson, *Organizations in Action*, p. 33.

A new Los Alamos director challenged the status quo, no longer willing to abide by the traditional division of responsibilities.

Discovering the reasons for Los Alamos' gain and Livermore's loss of the Navy strategic missile warhead development was important to understanding the dynamics of the two-laboratory system. Los Alamos' success in winning the Trident I warhead assignment left Livermore with weapons assignments Los Alamos "didn't want," as one Livermore scientist described the 1970s and 1980s.[11] His statement implies the laboratories had more influence over such decisions than was in fact the case. The laboratories may have believed they were "taking" weapons assignments, but these were contingent on the tacit, if not explicit, agreement of the Department of Defense and the armed services. If not the laboratories, who made weapon development decisions? The AEC had formal authority, and argued that Los Alamos' entry into the strategic missile warhead field was important to maintain a balanced laboratory workload. Although such thinking may have influenced the Trident I decision, Chapter 9 showed it was DOD and the Navy were key. Determined to develop a new warhead over the objections of the Navy's Strategic Systems Project Office Livermore lost its place as a "team player" with SSPO. Los Alamos was invited to begin attending Navy planning meetings, and when the time came to make the warhead development assignment SSPO did not indicate a strong preference for the Livermore warhead. Stated in the strongest terms, the Trident I case leads to the conclusion that military preferences were paramount in making weapon development assignments. At the very least, such decisions were unlikely to be made over the objections of the service involved.

Despite the 1980s Reagan defense buildup, new weapon systems were few and far between relative to previous decades, with demand declining to lowest levels since the immediate postwar (Figure 2). When demand was high and the policy environment stable, the laboratories had tended to cooperate, as they did in the 1950s and 1960s. Fewer new weapons in the 1970s and 1980s forced the armed services to make more tradeoffs than previously. Nuclear weapons had lost their value to the services as a basis for acquiring resources in the bureaucratic arena. In fact, sometimes the opposite, as the services tried to demonstrate they were controlling overall weapon costs. A new warhead, for example, might require flight testing, reducing available resources for other program priorities. Disagreement among sponsors or changing preferences and priorities catalyzed a more competitive laboratory relationship as each sought to exploit differences among sponsors and align itself with the winning side. Relations between the services and the laboratories grew more adversarial as the laboratories sometimes promoted new weapons the

11. Interview with William B. Shuler, LLNL, 25 July 1991.

services did not want. Shifting bureaucratic alliances could mean the difference between winning and losing, and Livermore became even more aggressive in pursuing new assignments.[12]

Causes and Consequences of Competition

Under what conditions will organizations compete and when will they cooperate? We generalized Arnold Kanter's model discussed in Chapter 1, positing that the least favored organization in a relationship of competitive interdependence would use competitive strategies (that is, would not cooperate), but only if it could expect outside support.[13] In the case of the laboratories outside support might come from Congress, or even a military customer with a less well established claim to nuclear resources. Without the possibility of external support competitive strategies were too risky. What would be the point of losing the benefits of cooperation if competition had no advantage?

The historical analysis showed that the single most important feature of Livermore's operating environment was its "second lab" status. This refers not only to Livermore's chronological position relative to Los Alamos, but to its more uncertain operating environment. Livermore's congressional sponsors, the public, and even the Atomic Energy Commission and its successors regularly asked about the need for two nuclear weapons laboratories. Livermore's weapon program, not Los Alamos', was singled out for possible closure by Congress or the AEC when political or budgetary circumstances changed for the worse. The questions began with the founding of Livermore, with detractors of the proposal questioning the value of a second laboratory. Initially, Livermore's number two status resulted from AEC concerns to protect the Los Alamos H-bomb program, while the military was reluctant to rely on Livermore. Livermore's less secure position was perpetuated by a variety of factors which continued to create uncertainty for the laboratory even as it matured and gained weapon development responsibilities. Livermore's congressional support was less reliable, and this was reflected in AEC actions. JCAE Chairman Clinton Anderson of New Mexico, for example, expressed concern about overlapping laboratory programs in an implicit reference to Livermore during the 1959-1962 test moratorium. Soon after the AEC Chairman floated the idea of closing the Livermore Sandia branch. With the exception of the 1960s, and the current member of Congress representing Livermore, the laboratory's congressional representation has been indifferent and sometimes hostile to the nuclear weapons program. But Los Alamos represents a substantial fraction of the New Mexico economy, reflected in its consistently solid congressional support.

12. For historical figures on U.S. military spending, see "More Security for Less Money," *BAS* 51 (Sept.-Oct. 1995): 34-50 at 36.

13. Kanter, *Defense Politics*, pp. 36-42.

Livermore's nuclear weapon program continues to be at greatest risk when budgets become constrained or when the political environment becomes less supportive of the nuclear weapons enterprise. Congressman George E. Brown, Jr., for example, introduced legislation in 1993 which if enacted would have consolidated nuclear weapons activities at Los Alamos, transforming Livermore into a "green" laboratory focused on energy, environment, and technology transfer.[14] Congressional proposals for phasing out the Livermore nuclear weapons program were joined by Department of Energy consideration of the idea, winning administration support in 1993.[15] Los Alamos took the opportunity to break ranks with Livermore—or to compete, to put it in the terms of this study. Los Alamos suggested that the two-laboratory system might need radical change in light of the budget constrained post-Cold War environment. Under such circumstances, one well funded nuclear weapons program was better than two inadequately funded programs. Los Alamos was the clear choice for the site of the consolidated weapons program. Livermore leaders disagreed. They argued that peer review was still a necessary ingredient of the weapons program, necessary to insure a safe and reliable stockpile. The word competition, however, did not appear in the argument.[16] Livermore's worst fears were realized when a government-sponsored task force assembled to consider the future of the Department of Energy laboratories recommended in February 1995 that Livermore's nuclear weapons responsibilities be transferred to Los Alamos.[17] The Secretary of Energy reacted favorably to the proposal. As her predecessor AEC Chairman Gordon Dean had, the Secretary of Energy joined in questioning the need for two nuclear weapons laboratories. She was later overruled by political and other considerations.[18] Located in California, a key electoral state whose economy suffered from defense

14. Rep. George E. Brown, Jr., "The Department of Energy Laboratory Technology Act of 1993," H.R. 1432, introduced 23 March 1993, to reorganize and consolidate DOE activities. Both Livermore and Los Alamos have long had diversified programs. See, e.g., LLNL Budget Office, UCAR 10185/94, *Fiscal Year 1994 Annual Report* (Livermore: TID, 1994), pp. 74-75.

15. See, e.g., Christopher Anderson, "Clinton Asks for a Greener DOE," *Science* 260, (9 April 1993): 153.

16. See GAO, "Nuclear Weapons Complex: Issues Surrounding Consolidating Los Alamos and Lawrence Livermore National Laboratories," GAO/T-RCED-92-98, statement of Victor S. Rezendes, Director, Energy and Science, Resources, Community, and Economic Development Division, before the House Comm. on Science, Space, and Technology, 24 Sept. 1992, pp. 2, 20 on the suggestion that the weapons program be consolidated at Los Alamos. A CBO study also recommended retaining Los Alamos as the sole weapons design and stockpile stewardship laboratory should a choice have to be made: *The Bomb's Custodians*, CBO Papers, July 1994.

17. SEAB, Task Force on Alternative Futures for the Department of Energy Laboratories, chaired by Robert W. Galvin (Galvin Committee), *Alternative Futures for the Department of Energy National Laboratories*, 2 vols., Feb. 1995, p. 14. See Tom Zamora Collina, "Livermore on the Defensive," *BAS* 51 (May-June 1995): 42-45, for a discussion of the implications of the Task Force findings for Livermore.

18. See Office of the Secretary of Energy, "Response by Secretary Hazel O'Leary to the Final Report

spending cuts in the early 1990s, Livermore's nuclear weapons program enjoyed another reprieve. President Bill Clinton announced in September 1995 that all three nuclear weapons laboratories, Livermore, Los Alamos, and Sandia, were "essential" to national security.[19]

The structure of the two-laboratory system led to different laboratory approaches to nuclear weapon design. Differentiation was the answer to the mandate to avoid laboratory duplication. Livermore responded by exploring longer-term weapon technologies and military requirements. Livermore's role as the new ideas laboratory also created incentives for technical innovation. Livermore worked on the margins of nuclear weapon development while the more established and politically secure Los Alamos held the center. The small primary designs invented by Livermore grew out of this system of incentives and constraints. So did the Polaris missile warhead. Livermore received fewer nuclear weapon development assignments, for systems that at the time they were assigned to the laboratory were of lower priority, that were less long-lived in the stockpile due to their uncertain or controversial status, and have represented a smaller percentage of total warheads in the stockpile (Figures 2 and 3, Table 1). Its longer-term outlook and more risk-taking approach created uncertainty since it was not always clear there would be interest or support in what the laboratory had to offer. Forced to be more aggressive in seeking military sponsors, Livermore established organizational mechanisms for forging ties with the military long before Los Alamos did. Los Alamos more relaxed approach gave Livermore scientists the impression it was not "competing." Livermore also tended to "go public," appealing to congressional or public interests in its search for weapons assignments. E. E. Schattschneider explained that this strategy is generally employed by the "losing" side of a political debate. It is a strategy of last resort for the weaker party, involving as it does exposure to risks over which it has little control.[20] It is worth the risk, however, if it the only option. In short, Livermore pushed the technological envelope, differentiated its product from Los Alamos, sought out less established military sponsors, and sometimes went public in its effort to survive, expand, and grow. When the environment was relatively stable, as it was during the 1960s, the strategies delineated above tended to be less pronounced, though they did not disappear entirely.

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of the Task Force on Alternative Futures for the Department of Energy National Laboratories," 1 Feb. 1995. For the president's decision on the need to maintain three laboratories, Livermore and Los Alamos for nuclear design in addition to the Sandia National Laboratories, see DOE, "President Announces Plan for National Laboratories: Three Weapons Labs Seen as 'Essential' for Nuclear Security," News Release R-95-142, 25 Sept. 1995.

19. DOE, "President Announces Plan for National Laboratories," News Release R-95-142, 25 Sept. 1995.

20. E. E. Schattschneider, *The Semisovereign People: A Realist's View of Democracy in America* (Hinsdale, Ill.: Dryden Press, 1975), pp. 16-17.

Los Alamos' legacy differs from Livermore's, and is distinguished not as much by innovation as it is by incremental technological change, less entrepreneurship, and less political controversy. Los Alamos had many firsts, especially early on, developing both the first atomic bombs and H-bombs, and key improvements to these original designs in the 1950s. The laboratory also developed the first strategic missile warheads for the Air Force and Army.[21] As far as Los Alamos scientists were concerned, there was no competition between the laboratories in Livermore's first years in existence.[22] As time went on, however, Los Alamos focused on improving and perfecting existing weapons. This was perfectly in accord with the interests of its military sponsors. Los Alamos was not aggressive and did not work against the interests of its sponsors because it had no need to. The stronger party, as Schattschneider's explains, plays an insider strategy. Los Alamos was willing to toe-the-line and avoid challenging important and powerful military sponsors. First the Air Force, and later the Navy, valued this approach, preferring stability over change, conservatism over innovation. Livermore's aggressiveness was appreciated by others, like the Army and Navy early on. Its willingness, even propensity, to take actions that sometimes conflicted with the interests of potential sponsors, as illustrated in the MX, and Trident I and II cases, also won it hostility.

Los Alamos always retained its status as the senior laboratory. Its favored position with the more conservative and established military customers persisted throughout the history of the two-laboratory system. The preferences of the services were influenced by what each laboratory had to offer technically, and whether or not their bureaucratic interests were thereby served. Livermore's role as the "new ideas" laboratory, necessary to justify the two-laboratory system, propelled its search for and promotion of new weapons. Livermore's propensities often linked it to political controversy, as did its outspoken publicists like Edward Teller. The risks associated with Los Alamos' approach differed from those associated with Livermore's. Los Alamos' focus on present and near term military requirements detracted from its ability to anticipate future military interests. This left laboratory scientists as well as outside observers with the perception that the Los Alamos weapons program had stagnated by the early 1970s. But Los Alamos did not need to be "different" in order to survive, and this made for a more conservative outlook, one that appealed to military customers who did not want radical change.

Innovation as Organizational Strategy

The organizational search for predictability, stability, and the control of uncertainty is usually thought to impede innovation. Wilson argues that many organizations will not adopt major

21. These included boosting and sealed pit weapons.

22. Discussions at LANL.

innovation unless faced with crisis, an "extreme change in conditions for which there is no adequate, programmed response."[23] The organizational quest to reduce uncertainty is sometimes thought to be associated with a lack of organizational dynamism, flexibility, and adaptability. What might prompt organizations to innovate short of organizational crisis? The results of this analysis show that the organizational search for uncertainty control can be a dynamic force, even promoting technical innovation. As we have seen, Livermore operated in a constant state of low level crisis. That is, its continued survival, or at least the vigor of its existence, was regularly questioned. Hence its designation as the "second lab." In response, Livermore became entrepreneurial and innovated technically. The structure of the two-laboratory system created the incentive to do so. In James Q. Wilson's terms, innovation became Livermore's programmed response to being the second laboratory.

Organizational innovation usually accompanies technical innovation since technical innovation is likely to entail substantial changes in organizational tasks. The transition from atmospheric testing to underground testing in the 1960s required such changes, for example. So has the end of nuclear weapons testing in the present era. Livermore eliminated its nuclear test division and is restructuring the laboratory to accommodate new missions.[24] One of these new missions is scientific stockpile stewardship, the DOE proposal for ensuring stockpile safety and reliability without nuclear testing. The changes required have been difficult and real, reflecting the changing status of scientists and organizations at the laboratories. At Livermore, for example, the relationship between scientists in the weapons program and the rest of the laboratory has shifted. Weapon scientists—who once funnelled funding to other programs that worked for them—now find themselves less able to influence outcomes. These developments are worth highlighting because they indicate greater adaptability and possibilities for organizational innovation than critics might expect. Even if scientists pursue their own ideas, "creating their own reality about the need for new weapons," as one critic has charged, there is little the laboratories can do to make that reality happen without resources.[25] Organizations cannot survive independent of their environments, as much as organizational leaders and strategists might like to. Nor can they operate without the resources provided by that environment. What this means is that they must persuade others of their need for resources. And this makes them attuned to and responsive to sponsors.

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23. James Q. Wilson, "Innovation in Organization: Notes Toward a Theory," chap. 5, pp. 193-218 at 208.

24. See, e.g., Peter Weiss, "Labs Turn From Bombs to Business in Quest to Survive," *Valley Times*, p. 3A.

25. William Arkin, "Nuclear Junkies: Those Lovable Little Bombs," *BAS* 49 (July-August 1993): 22-27, at 25, 27.

In order to effect fundamental change then, it is the sponsors and their interests that must change.

Another indicator of organizational innovation can occur at the management level. Did laboratory personnel changes reflect evolving organizational needs and changing environments? Where should we look for answers? Charles Perrow posits that organizations will promote individuals or groups who perform critical tasks, defined as distinctive problems faced by organization at particular stages of development.[26] Organizations—at least those most likely to survive change—will thus implement personnel and organizational change to help them navigate the particular environment in which they find themselves. Recent laboratory personnel changes suggest the laboratories are doing that. Los Alamos has eliminated several hundred jobs, Livermore is reorganizing. And in 1994, Livermore director John Nuckolls resigned under pressure from the University of California Regents, the laboratory management contractor. He had been heavily criticized both inside and outside the laboratory for keeping Livermore on a Cold War track despite substantial changes in the domestic and political environment. Nuckolls had served as director since 1988, a period of transition for the laboratory and the world. Where the nation would come out in the debate over the future role of nuclear weapons in the new international environment—and the future of the nuclear weapons laboratories—was uncertain and in flux during his tenure. He navigated this transition with an eye to maintaining the viability of the laboratory for which he was responsible, which gave him a rather conservative outlook. His approach seems to have been vindicated by recent political changes in Washington which favor weapons programs over non-weapons programs such as energy and environment. Had he wholeheartedly plunged the laboratory into a "green" agenda, the laboratory would now be in a much more uncertain position. In any case, Nuckolls was replaced by Bruce C. Tarter. A laboratory physicist —though not drawn from the ranks of the weapons program, a first for the laboratory—Tarter faces a different challenge. One of his jobs is to look out for the interests of the non-weapons programs in the context of a political environment that is more favorable to the weapons program, though budgets are constrained for all programs. Nuckolls' tenure suited the times in which he served, Tarter's his.

The Arms Race

This analysis has considered the international environment as a given, thus not addressing the larger political forces, both domestic and international, which shaped perceptions about military requirements for nuclear weapons. The results can nevertheless lead to some conclusions

26. Charles Perrow, "The Analysis of Goals in Complex Organizations," *American Sociological Review* 26 (Dec. 1981): 854-866.

about the relative roles of the military and the laboratories in determining the pace and direction of nuclear weapons development. The relative push and pull of the laboratories and the military depended largely on military demand, which in turn was primarily determined by national policy. The New Look, for example, accelerated military demand for nuclear weapons in the 1950s. Demand was so high that Los Alamos spearheaded an effort to "standardize" warhead designs, proposing that missiles be adapted to fit nuclear warheads. But the services preferred that nuclear warheads be adapted to delivery systems, not vice versa, so warhead standardization never materialized.

The military could afford to prefer individually tailored nuclear weapons since the AEC incurred the cost of nuclear warhead design, development, and production, while DOD was responsible for delivery system costs. Tailoring nuclear warheads to delivery systems was cost-effective from the military's point of view since it simplified delivery system design. In any case, nuclear warhead costs were relatively small in comparison to delivery system costs.[27] Still, had DOD been responsible for nuclear warhead production it seems plausible that fewer nuclear designs would have been developed. In this case, nuclear weapons programs would have been forced to compete for resources with other military programs and it would have been cheaper to employ standardized warheads, or to forego new systems altogether. As it was, about 90 different designs were developed, with 63 deployed in the stockpile (Figure 1).

As we have seen, the dynamics of laboratory competition and customer-supplier relations shifted in the 1970s. Budgetary and political constraints reduced military demand for new nuclear weapons, reversing trends of the prior two decades. The demise of the congressional Joint Committee on Atomic Energy in 1977 transferred legislative oversight to other committees, and the laboratories lost their strongest advocates.[28] Total overall weapons costs became more important considerations, although the weapons laboratories continued to be funded through DOE. Prior to these changes the laboratories' principal concern was about how to manage their sizeable workloads. Now they worried about having enough to do. The inventory of new warheads grew slowly. The rate of entry of new warheads in the stockpile declined substantially after 1965 (Figure 1). Fewer than twenty new warheads entered stockpile between 1970 and 1990. This compares with the approximately seventy new warheads deployed between 1945 and 1970 (Figure 2).

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27. Stephen I. Schwartz, "Atomic Audit: What the U.S. Nuclear Arsenal Has Cost," *Brookings Review* 13 (Fall 1995): pp. 14-17, at 15.

28. Barton C. Hacker, *Elements of Controversy: The Atomic Energy Commission and Radiation Safety in Nuclear Weapons Testing, 1947-1974* (Berkeley: Univ. of California, 1994), p. 259.

These findings lead to an interesting and somewhat counterintuitive conclusion about the relationship of laboratory competition to the arms race. In the 1970s, reduced military demand for nuclear weapons led to increased laboratory competition and promotion of new warheads. At the same time the number of new designs declined. The overall impact of laboratory competition on numbers of nuclear weapons was therefore small in that the most competitive period corresponds to the lowest level of warheads developed. Funding did not vary as much, perhaps because of fixed costs, although budget figures do nevertheless reflect changing priorities. The number of full time employees in the laboratory weapons research development and test (RD&T) program, for example, declined starting in Fiscal Year (FY) 1971, going from a high of over twenty-four hundred in 1970 to twenty-two hundred in 1971. In FY 1969 the weapons RD&T personnel represented approximately 40 percent of the laboratory's full time equivalent staff of fifty-seven hundred. By contrast, in FY 1994, the weapons program accounted for approximately nine hundred of the laboratory's seventy-three hundred full time equivalent employees, or about 13 percent. The remainder work in other laboratory programs, conducting energy, environmental, and biomedical research. It is difficult to accurately compare figures across time because programs change or are sometimes redefined. Livermore's inertial confinement fusion program (ICF), for example, has sometimes been defined as part of the weapons program, sometimes in the energy program.[29] This is not merely a matter of nomenclature. Changing what a program is named also changes its contours. In any case, such changes are further indication of laboratory desires to be in step with changing political and economic environments. In short, overall budget figures suggest laboratory budgets, staff, and programs rose and fell in concert with overall political support and defense spending, not that the laboratories "drove" the process, at least in the big picture.

There is no doubt, however, that the existence of two laboratories did create extra capacity into which the services could tap for their sometimes specialized needs and wants. In 1955, for example, the Navy might have had to accept the Air Force's Los Alamos designed Class D warhead for its Regulus missile had Livermore not been eager to take on the assignment. Regulus was not a high DOD priority relative to the Air Force gravity bomb (the B-28), so the Navy would have had to use the Air Force warhead or wait longer for what it wanted. Promises of technical innovation also helped win the laboratory the Polaris warhead assignment, perhaps changing the timing of deployment of the Navy's FBM system and the way it looked. Laboratory success in promoting new weapons nevertheless depended on finding supporters and allies in the military or elsewhere. Scores of weapons design ideas were pursued at the conceptual level by laboratory scientists—including 100-megaton weapons and nuclear hand grenades—but received

29. LLNL Budget Office, *Fiscal Year 1994 Annual Report*, pp. 74-75.

no substantial funding because they did not have sponsor support or interest. Neither laboratory had much success in promoting weapons technologies which did not resonate with client interests. The services cared little for clean or enhanced radiation weapons, for example, because they gave less "bang for the buck." It was the AEC that encouraged laboratory research in this area with the hope of finding a solution to political pressures resulting from public concerns about radioactive fallout. Things changed in 1973 when the JCAE became a strong supporter, rejecting the Army's proposal to modernize its standard atomic artillery shells in favor of the neutron bomb. The Army then teamed up with Livermore to develop the new technology when it became clear its alternative was to be forced out of a role in nuclear defense.

Fundamentally, the laboratories want to survive, rendering them potentially responsive to sponsors, even willing to endorse proposals that might seem to run counter to their core organizational interests, including ending nuclear testing. Their quest for survival even more than allegiance to a particular world view or program, helps explain the relative ease and speed with which they are adapting to post-Cold War political realities.

Competition and Peer Review

Proponents of the two-laboratory system have argued that peer review is one of its key benefits. Under an idealized model, scientific peer review is conducted by disinterested parties with only the advance of science as a criterion for judging the merits of proposals. Laboratory peer review is supposed to involve deliberate, systematic, and objective assessment of the scientific and technical merits of laboratory weapon design proposals. We have seen, however, that the laboratories are care about what their sponsors want. Long before Secretary of Energy Hazel O'Leary began speaking of the Department of Energy's "stakeholders" the armed services were commonly referred to as the "customers" of the nuclear weapons design laboratories. The "products" are the nuclear weapons designed by the laboratories, which "sell" their ideas to Washington and the military.

Product, customer, sell, and stakeholder are not the terms of peer review, but of market-driven or client-driven competition. When one laboratory's design team reviews the other's design in the presence of a potential military customer all the parties are interested in the outcome. The arbiter is the ultimate user or customer of the product being reviewed. One laboratory will "win" the assignment, the other will "lose." Take away the customer and the possibility of winning or losing and there is little if no incentive for competition. That laboratory competition in the two-laboratory system bears only partial resemblance to an idealized peer review process is not necessarily a drawback from the perspective of the customer. In fact, the system provided a strong incentive for reviewers (the laboratories) to focus on matters of importance to the customer (the armed services). The system did not encourage peer review in features of little interest to the customer that might be considered important in some objective sense. If military customers cared little about the cost of nuclear warheads, for example, not a farfetched notion given that the Department of Energy pays all costs associated with the warhead, but cared only principally about weapons yield, the laboratories would have little incentive to control costs while paying close attention to yield characteristics. Reversing the interests would reverse the outcome. For these reasons the laboratory relationship is more properly defined as competitive.

Peer review is no more likely to work than is competition if there is no customer or sponsor. Who are the customers and what information are they willing to pay for in the post-Cold War world? The problem is illustrated by the new method for ensuring the safety and reliability of remaining stockpile weapons. Each laboratory has responsibility to monitor the other's remaining warheads, and to raise concerns or issues discovered. This sounds reasonable, but there are some inherent difficulties in the process. One laboratory must be willing to share information about its remaining stockpile weapons for the other to be able to evaluate their safety and reliability. The former may be reluctant to share damaging information, the latter about embarrassing the former. The embarrassed laboratory might turn the tables on the other when the time came for its evaluation.[30]

While peer review can work, even if imperfectly, there are other potential weak points in the system. Who will judge a laboratory's claim that a weapon should be pulled from the stockpile and who will pay for a retrofit or rebuild if required? Neither the Department of Defense nor the Department of Energy can be expected to have much enthusiasm for expensive fixes given budgetary constraints under which each operates. The identification of a problem with a nuclear weapon in the stockpile might lead to its removal rather than to its repair. The laboratory that identifies the problem may thus be helping to put the other out of the nuclear weapons business (or itself, should the laboratories be responsible for monitoring their own weapons). Where is the incentive to raise problems in this scenario? In short, the simple existence of two laboratories does not guarantee peer review. This is not due to individual or organizational failings. Rather, it is an expected outcome of the structure of laboratory relationships to their sponsors.

Post Cold War Policymaking

What leverage do policy makers have over the nuclear weapons laboratories? Not much according to some observers. Scilla McLean, for example, who studied nuclear weapons decisions, concluded that the large bureaucracies involved in weapons development circumvent the will of policy makers, assured they will remain in operation long after legislators are gone.[31] E. E.

30. Discussions at LLNL.

31. Scilla McLean, ed., *How Nuclear Weapons Decisions are Made* (London: MacMillan, 1986), pp. 257-258.

Schattschneider offers a less pessimistic vision, one that suggests large bureaucracies can be compelled to be responsive to their sponsors. The problem is similar to that of the public, which seeks satisfaction from policy makers. An essential problem of democratic politics—and for policy makers—is to elicit policy options from the organizations they oversee but over which they have only limited control. As Schattschneider explains, the public is "like a very rich man who is unable to supervise closely all of his enterprise. His problem is to learn how to compel his agents to define his options . . . conflict, competition, leadership, and organization are the essence of democratic politics."[32]

Competition for resources among agencies within a government department or between departments is one means of compelling large bureaucracies to present alternative policy options. Interservice rivalry, for example, helped shape U.S. strategic doctrine and weapons procurement.[33] What Aaron Wildavsky has called the institutionalization of advocacy can be a tool albeit a blunt one for generating alternative policy options.[34] Arnold Kanter argues that efforts to overcome the parochialism of large bureaucracies have rarely met the objectives of policy makers. Instead, policy makers ought to take a politically instrumental view of organizational arrangements. They can enhance their bargaining advantage by changing bureaucratic relationships and procedures to exploit organizational responsiveness.[35] Kanter's suggestion is based on the assumption that large bureaucracies have important bargaining resources but can also react to changes in the incentive structure. By structuring relationships among bureaucracies and their sponsors, policy makers might thus elicit information on alternative policy options. This might be achieved, for example, by defining missions and encouraging laboratory competition for resources. This is a mechanism suggested by Harvey Sapolsky and Owen Coté for gaining leverage over the armed services. The outcome would not have to be winner take all, but incentives would have to be sufficient for the laboratories to risk open dissent.[36] Competition will not occur if there is no clear advantage. This would be the case if policy makers—at least those that matter

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32. Schattschneider, *The Semisovereign People*, p. 136.

33. See, e.g., Edmund Beard, *Developing the ICBM: A Study in Bureaucratic Politics* (New York: Columbia Univ. Press, 1976); Kanter, *Defense Politics*, Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance* (Cambridge: MIT Press, 1990).

34. Aaron Wildavsky, *The New Politics of the Budgetary Process* (Glenview, Ill.: Scott, Foresman, 1988), p. 355.

35. Kanter, *Defense Politics*, pp. 120-123.

36. Owen Coté, "The Politics of Innovative Military Doctrine: The U.S. Navy and Fleet Ballistic Missiles," Ph.D., Pol. Sci., MIT, Sept. 1995; Harvey M. Sapolsky, "Notes on Military Innovation: The Importance of Organizational Structure," unpublished mss., 24 Feb. 1994, 8 pp.

to the laboratories—agree on desired outcomes. Dissent can create information and options for policy makers, but is unsettling for organizations. It makes their claim to exclusive expertise suspect and reduces their control. Consensus among bureaucratic actors weakens the leverage of policy makers because it shields organizations from criticism or outside interference.

The evidence considered in this study leads to the conclusion that laboratory competition—when it occurred—worked in the sense that it provided options to policy makers (or sponsors) when they sought them and it benefitted one of the laboratories. Critics could reasonably argue this was not a desirable outcome and that too much was spent on developing too many nuclear weapons. This may be so, but the outcome cannot be explained on the basis of a technological imperative or of independent laboratory action.

The end of the Cold War increased the laboratories' uncertainty about their future. Budgets have fallen and no new nuclear warheads are being designed. Over the past few years, Livermore's future as a "green laboratory" investigating environmental problems has been proposed and withdrawn and then promoted again. Los Alamos reactions to proposals to consolidate nuclear weapons research and development at the laboratory have varied depending on the political climate in Washington. When "green" seemed the direction of the future, inheriting responsibility for nuclear weapons was unsettling.[37] The 1994 election changed that, with Republicans in power in both Houses of Congress vocal supporters of defense spending.

The historic concern of first the AEC and then DOE regarding program duplication asserted itself again. The Department of Energy responded to changes brought by the end of the Cold War—reduced demand for nuclear weapons and falling budgets—by seeking to streamline and consolidate nuclear weapons activities. The laboratories responded by emphasizing the benefits of cooperation and sharing of programs and facilities, much as they had in the past. Laboratory competition, once the rationale used to justify the two-laboratory system, was submerged and transformed.

DOE has encouraged laboratory cooperation and unified positions on important policy questions. Where competition once was rewarded, streamlining and cooperation are now the watchwords. This dynamic is evident, for example, in the implementation of DOE's plan for scientific stockpile stewardship. The relevant agencies—DOE, DOD, and the laboratories—have worked hard to agree on the scope of the program, including the need for new large facilities such as the National Ignition Facility, a large laser Livermore would like built at the laboratory.[38] In-

37. Discussions at Livermore and Los Alamos.

38. Discussions at LLNL; Sybil Francis, "Save the Labs?" *Breakthroughs* [MIT Defense and Arms Control Studies Program] 4 (Spring 1994): 18-22. See also Hugh Gusterson, "NIF-TY Exercise Machine," *BAS* 51 (Sept.-Oct. 1995): 22-26, for an insightful discussion of NIF; see also the response by Jacqueline Cabasso and John Burroughs, "End Run around the NPT," ibid., 27-29.

deed, the laboratories have been encouraged by the DOE to cooperate, much as they were encouraged to cooperate during the nuclear test moratorium in the late 1950s when the AEC and Los Alamos sought to discourage Livermore from actively promoting underground testing.

Cooperation benefits the DOE and the laboratories. Are there any losers? The impulse towards centralization, cooperation, and streamlining is understandable in times of budget constraints. In the political environment of the mid-1990s it makes sense for the laboratories to have dropped competition from their vocabulary because the term raises the specter of excessive cost and duplication. Can policy makers achieve <u>their</u> goals in this new environment? It depends on who they are and if they are in a position to effect change. The structure of organizational relationships will be decisive. Closely held information is a hallmark of cooperation, leaving some within the decision making circle, others outside it. Either way of proceeding has important implications for policy making. Under the best circumstances competition does not guarantee answers to all potentially important questions. But competitive pressures can generate options for consumers when they seek them. Whether or not this continues to be relevant in the future will depend on who the customers are and what they want.

If policy makers wish to promote the generation of alternative policy options from organizations with a stake in the outcome they must pay attention to how organizational relationships are structured. The problem is how to compel government agents, in this case the laboratories, to define options. One approach would be for policy makers to outline objectives and spending limits and then to ask the laboratories to argue why they should be given the responsibility they seek.[39] Conflict and competition are the essence of the process, but these are politically unpopular because they raise the specter of waste and duplication and threaten organizational interests. How to design systems of incentives and constraints that do not abdicate policy making and at the same time do not appear wasteful or duplicative will be the challenge.

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39. Sapolsky, "Notes on Military Innovation," p. 7.

TABLE AND FIGURES

TABLE 1

Lifetime of Warhead Types in the Stockpile

Warhead	Years in the Stockpile	Total	Laboratory
Fat Man	1945-1949	4	LANL
Little Boy	1945-1948	3	LANL
B-3	1945-1951	6	LANL
B-4	1948-1953	5	LANL
B-5, W-5	1952-1963	6	LANL
B-6	1951-1962	11	LANL
B-7, W-7	1952-1967	15	LANL
B-8, W-8	1951-1956	5	LANL
W-9	1952-1957	5	LANL
B-10	cancelled	-	LANL
B-11	1956-1960	4	LANL
B-12	1954-1963	9	LANL
B-13	cancelled	-	LANL
B-14	1954	1	LANL
B-15	1955-1965	10	LANL
B-16	cancelled	-	LANL
B-17	1954-1957	3	LANL
B-18	1953-1956	3	LANL
W-19	1956-1963	7	LANL
B-20	cancelled	-	LANL
B-21	1955-1957	2	LANL
B-22	cancelled	-	LLNL
W-23	1957-1959	2	LANL
B-24	1954-1957	3	LANL
W-25	1957-1985	28	LANL
B-26	cancelled	-	LANL
B-27, W-27	1958-1965	7	LLNL
B-28, W-28	1958-1991	33	LANL
W-29	cancelled	-	LANL
W-30	1959-1978	19	LANL
W-31	1958-1989	31	LANL
W-32	cancelled	-	LANL
W-33	1956-1992	36	LANL
W-34	1958-1977	19	LANL
W-35	cancelled	-	LANL
B 36	1956-1962	6	LANL
W-37	cancelled	-	LANL
W-38	1961-1965	4	LLNL
B-39, W-39	1957-1966	9	LANL
W-40	1959-1972	13	LANL
B-41	1960-1976	16	LLNL
W-42	cancelled	-	LLNL
B-43	1961-1991	30	LANL
B-44	1961-1989	28	LANL
W-45	1962-1988	26	LLNL
B-46	cancelled	-	LANL
W-47	1960-1975	15	LLNL
W-48	1963-1992	29	LLNL

TABLE 1 *continued*

Lifetime of Warhead Types in the Stockpile

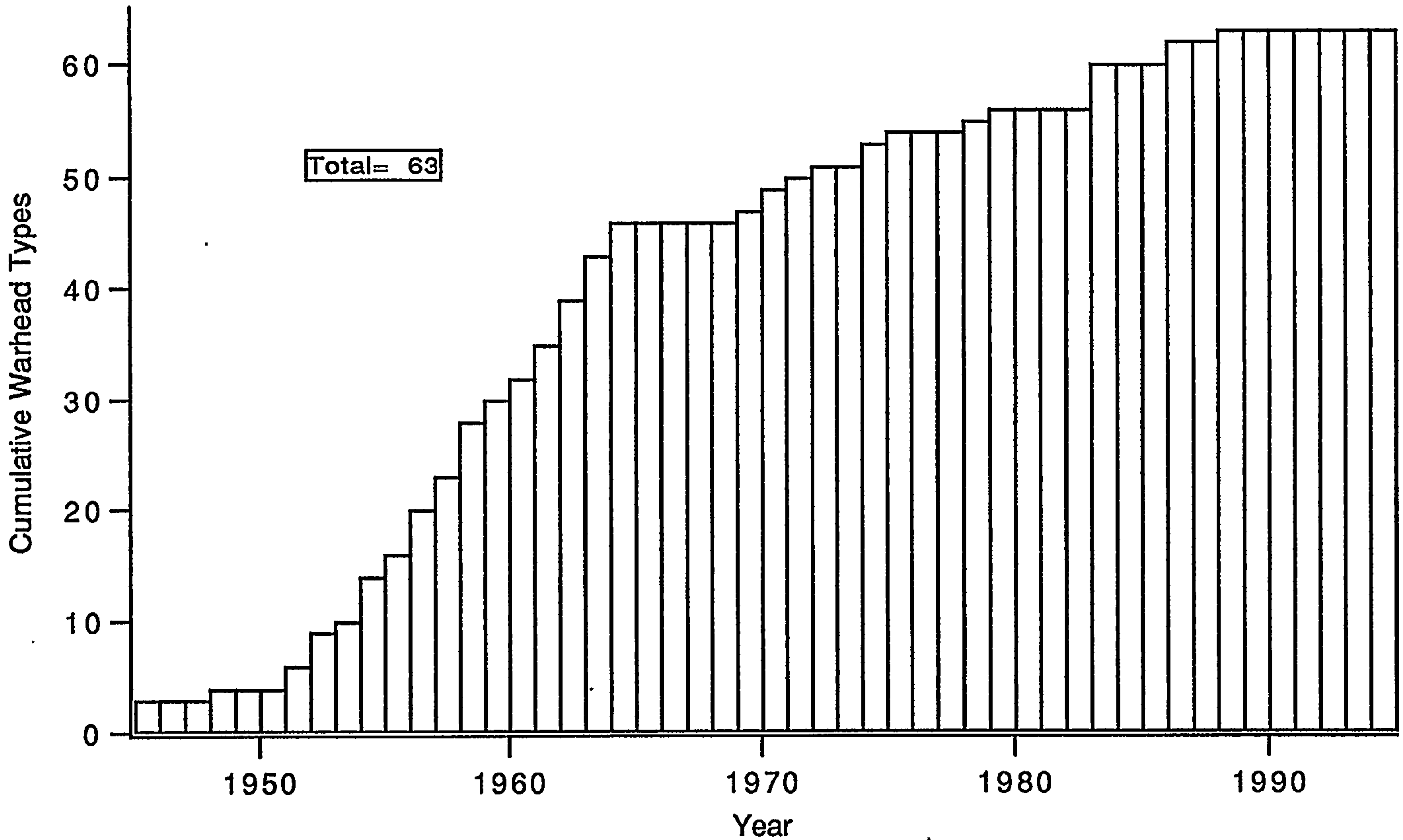
Warhead	Years in the Stockpile	Total	Laboratory
W-49	1958-1975	17	LANL
W-50	1963-1991	28	LANL
W-51	cancelled	-	LLNL
W-52	1962-1977	15	LANL
W-53	1962-1987	25	LANL
B-54	1964-1989	25	LANL
W-55	1964-1990	26	LLNL
W-56	1963-1993	30	LLNL
B-57	1963-1993	30	LANL
W-58	1964-1982	18	LLNL
W-59	1962-1970	8	LANL
W-60	cancelled	-	LLNL
B-61	1969-1995*	25	LANL
W-62	1970-1995*	25	LLNL
W-63	cancelled	-	LLNL
W-64	cancelled	-	LANL
W-65	cancelled	-	LLNL
W-66	1974-1986	12	LANL
W-67	cancelled	-	LANL
W-68	1970-1993	23	LLNL
W-69	1972-1994	22	LANL
W-70	1974-1992	18	LLNL
W-71	1975-1992	17	LLNL
W-72	1971-1978	7	LANL
W-73	cancelled	-	LANL
W-74	cancelled	-	LANL
W-75	cancelled	-	LLNL
W-76	1978-1995*	17	LANL
B-77	cancelled	-	LLNL
W-78	1979-1995*	16	LANL
W-79	1986-1992	6	LLNL
W-80	1983-1995*	12	LANL
W-81	cancelled	-	LANL
W-82	cancelled	-	LLNL
B-83	1983-1995*	12	LLNL
W-84	1983-1995*	7	LLNL
W-85	1983-1991	8	LANL
W-86	cancelled	-	LANL
W-87	1986-1995*	9	LLNL
W-88	1988-1995*	7	LANL
W-89	cancelled	-	LANL

* Current systems

LANL nuclear warheads deployed after 1958=24; LLNL systems=17.
Data compiled from several sources, including Chuck Hansen, *U.S. Nuclear Weapons: The Secret History*, and Office of Military Application, *A History of the Nuclear Weapons Stockpile, FY 1945-FY 1985*.

Figure 1

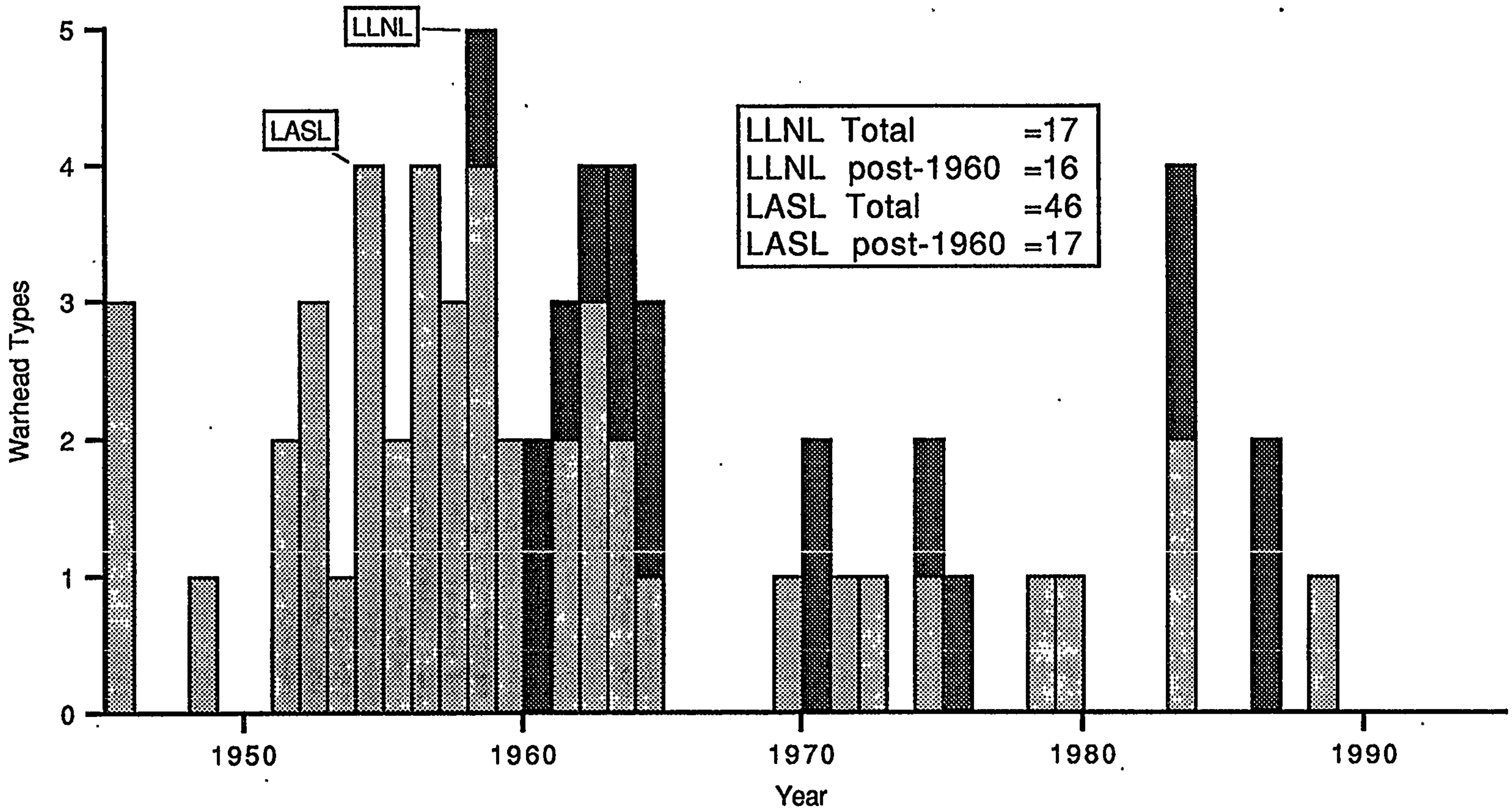
Sum of Warhead Types That Have Entered Stockpile, 1945-1990



Source: Thomas B. Chochran, William M. Arkin, and Milton M. Hoenig, "Nuclear Weapons Databook," vol. 1, U.S. Nuclear Forces and Capabilities (Cambridge, Mass.: Ballinger Publishing Company, 1984), pp 7-9; Chuck Hansen, U.S. Nuclear Weapons: The Secret History (New York: Orion Books, 1988), pp. 106-107.

Figure 2

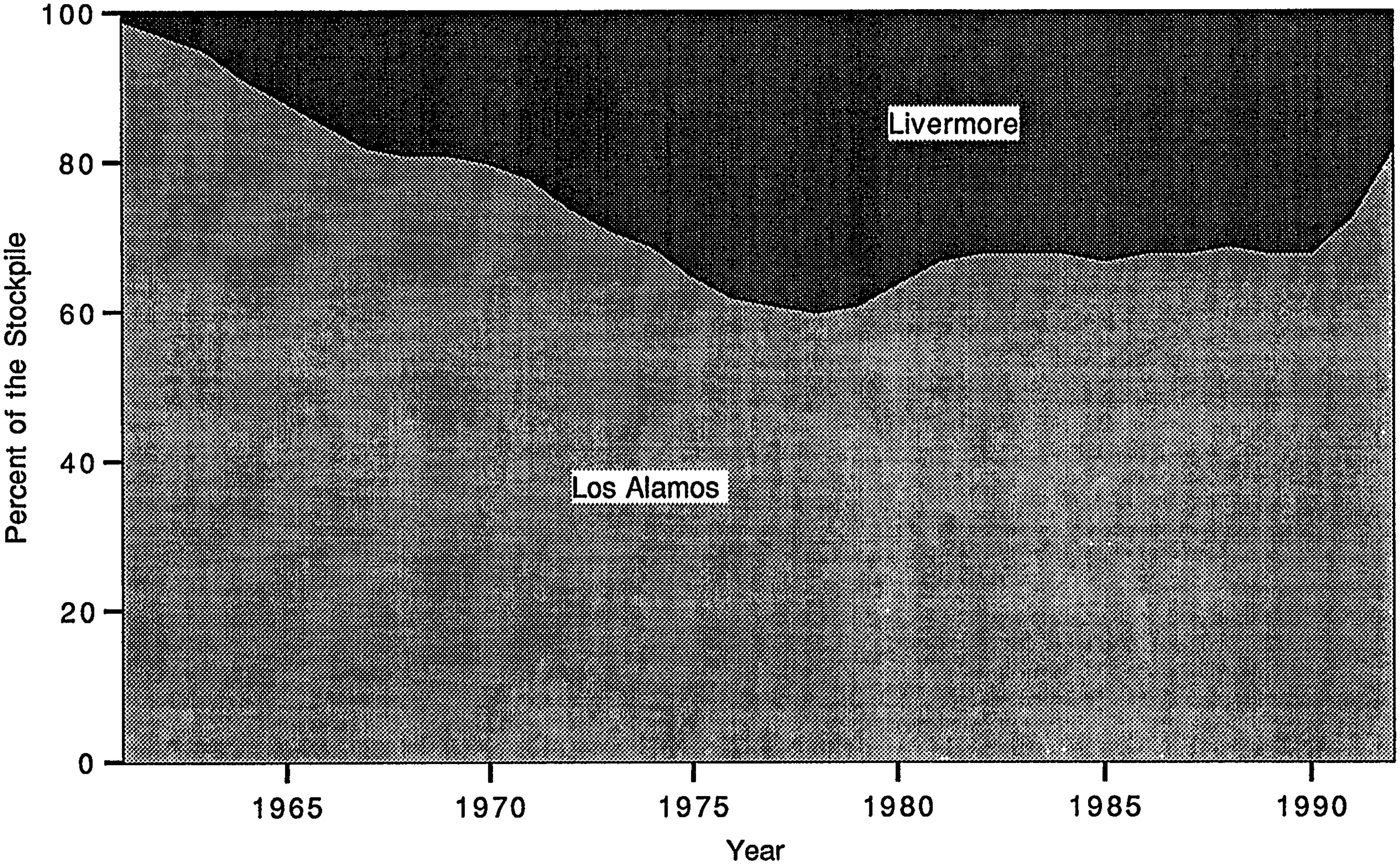
Number of Warhead Types Entering Stockpile Each Year



Source: Thomas B. Chochran, William M. Arkin, and Milton M. Hoenig, "Nuclear Weapons Databook," vol. 1, U.S. Nuclear Forces and Capabilities (Cambridge, Mass.: Ballinger Publishing Company, 1984), pp 7-9; Chuck Hansen, U.S. Nuclear Weapons: The Secret History (New York: Orion Books, 1988), pp. 106-107.

Figure 3

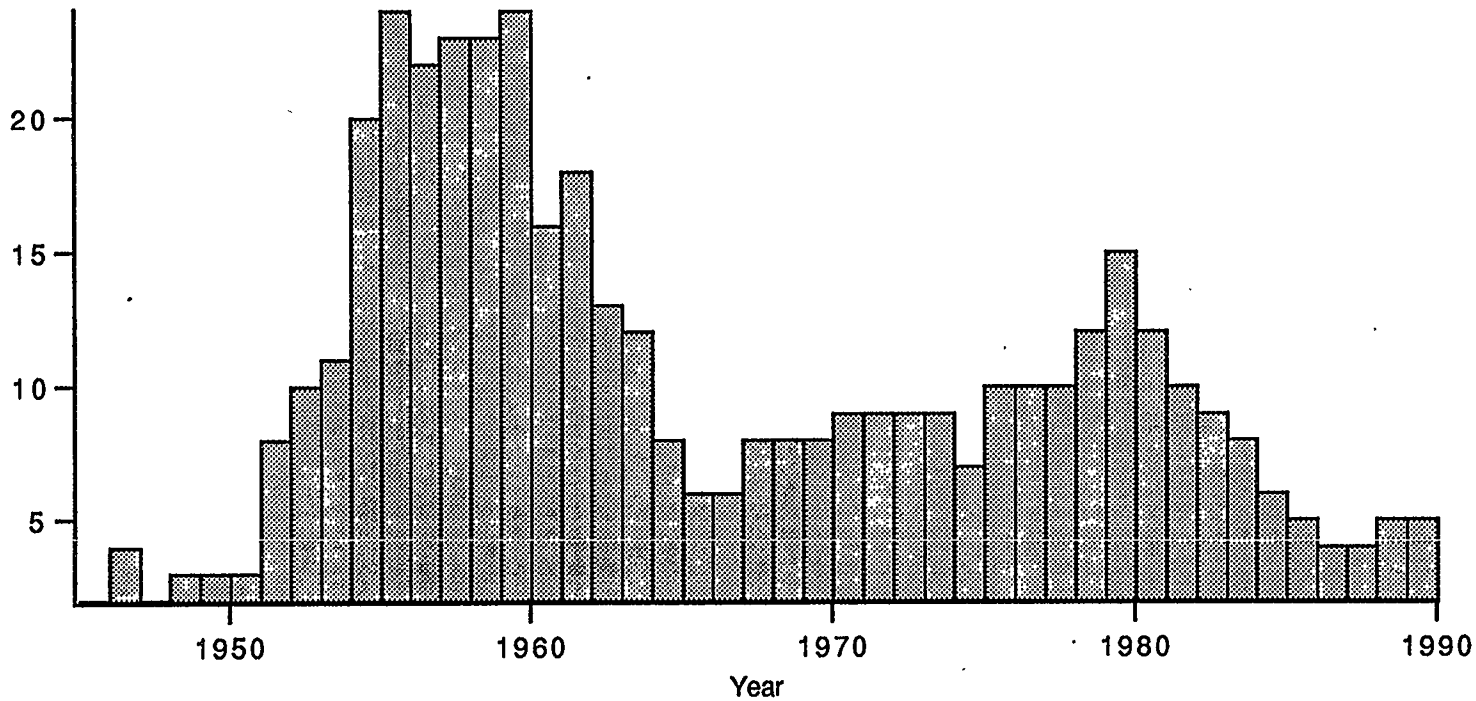
Livermore and Los Alamos Share of Total Warheads in Stockpile



Source: DoE Office of Military Application, A History of the Nuclear Weapons Stockpile, FY 1945-FY 1985 Doc. No. TID-26990-7, Dec. 1986.

Figure 4

Nuclear Weapons in Engineering Development Through First Production*

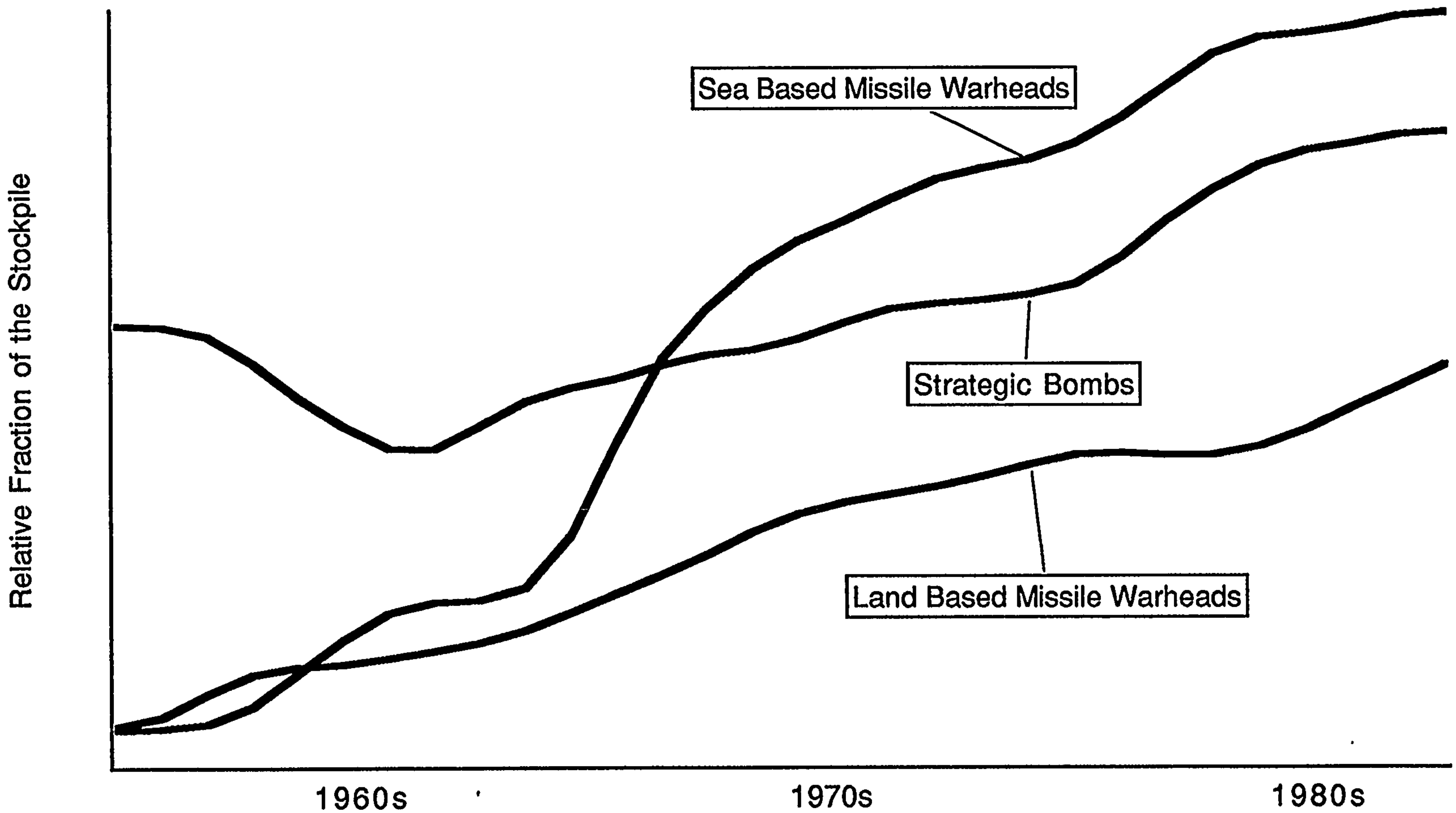


*Warheads require several years of development, and once deployed may be modified, so individual systems are counted over several years in the totals shown.

Source: "Number of Weapons in Engineering Development (Phase 3) through First Production (Phase 5)," Sandia National Laboratories, 3 August 1994.

Figure 5

Navy Warheads Begin to Dominate Strategic Stockpile in Early 1970s



Curves have been smoothed for classification purposes.
Source: DoE Office of Military Application, A History of the Nuclear Weapons Stockpile, FY 1945-FY 1985
Doc. No. TID-26990-7, Dec. 1986.

SOURCES AND BIBLIOGRAPHY

NOTE ON SOURCES

This dissertation is based largely on archival research and interviews. While working on it I was in residence at the Lawrence Livermore National Laboratory, in the Center for Security and Technology Studies. The Department of Energy granted me a 'Q' clearance, allowing me access to classified information about nuclear weapon design. The laboratory made all its relevant unclassified and classified records available, including not only the Laboratory Archives but also the holdings of the several weapons-related groups. I also conducted research in the classified and unclassified collections of other archives, including the University of California in Berkeley, Los Alamos National Laboratory, and the Department of Energy in Washington, D.C. All are listed below.

Interviews with former laboratory directors, weapons designers, armed service representatives, and Washington officials served primarily for background and to help structure my documentary research. They ranged from substantial discussions over a broad range of topics to attempts to clarify particular points of information or interpretation. Numerous conversations (not listed) with Livermore and Los Alamos scientists and administrators gave me a vivid sense of both present and past activities of the laboratories. I regularly attended presentations by current and former Livermore laboratory directors, laboratory managers, scientists in the weapons program, and Washington officials. These gave me a sense for the laboratory's broad interests and approach to problems, even if they were not directly relevant to my historical analysis.

Published works cited in the "Bibliography" played an important though relatively minor role in constructing the history of the Livermore and Los Alamos nuclear weapons program.

There simply is not much published of direct relevance. Chuck Hansen's *The Swords of Armageddon: U.S. Nuclear Weapons Development Since 1946* is the exception, an invaluable source of declassifed documents collected over decades. Published by Chukelea Publications in Sunnyvale, 1995, it was not available when I conducted my research. Much of the documentation Chuck generously provided me now appears in this impressive collection, available in microfiche or on CD-Rom.

I have provided the Lawrence Livermore National Laboratory Office of History and Historical Records (OHHR) a complete set of all documents cited, but have not listed these individually except as they appear in the footnotes. Access to the documents can be gained by making a request to the OHHR for the "Nuclear Weapons Historical Collection" compiled by Sybil Francis. Some of the documents may still be classified and thus would require special clearances to be examined. With the intent of making the documents on which the thesis is based as accessible as possible, I have requested declassification review of a substantial number of the documents cited and intend to submit the remainder to the appropriate agencies for review. The status with regard to particular documents may be determined by writing to the Office of History and Historical Records, P.O. Box 808, L-451, Lawrence Livermore National Laboratory, Livermore, California, 94551, or by calling (510) 422-1033. OHHR can provide information regarding the original provenance of documents, or distribute copies directly.

Because the thesis is based in part on classified sources, it has been reviewed by the Livermore Classification Office to ensure it contains no classified information. I knew from the start of my project that this would be required. I had some apprehensions about it, although I was confident the results would be well worthwhile. My apprehensions were groundless, and I am pleased with the outcome. A few technical matters could not be discussed in detail, but these were of no consequence to the sense of the story, the analysis, and certainly not to the conclusions. In the few cases I needed to delete a classified term from a source, I indicated so by using a substitute term in brackets. A few other required changes are also indicated where they occur in the text.

ARCHIVES VISITED

Bancroft Library, University of California, Berkeley
- Ernest Orlando Lawrence Papers

Center for Air Force History, Washington, D.C.

Coordination and Information Center, DoE facility operated by Reynolds Electrical & Engineering Co., Inc., Las Vegas, NV

Department of Defense, Washington, D.C.
- Air Force History Office
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- AEC-MLC Records
- General Advisory Committee Meeting Minutes
- Military Research and Applications
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Delmar Bergen
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Robert (Bud) Budwine
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Frank A. Camm
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ABOUT THE AUTHOR

Sybil Francis grew up in Cambridge, Massachusetts, and Washington, D.C. Raised in a bilingual family, she often visited her relatives in France. Her youthful outlook was influenced by the Vietnam War, environmental activism, and the energy crisis of the early 1970s. Determined to understand the scientific underpinnings of environmental issues, she pursued a B.A. in Chemistry at Oberlin College, awarded in June 1979. Broader interests in science and technology policy then brought her back to Washington where she became a legislative assistant for science and technology policy to Congressman George E. Brown, Jr.

After almost six years in Washington she returned to graduate school, her studies at the Massachusetts Institute of Technology forging a link between her earlier concerns with war and academic study. She joined the MIT Department of Political Science in 1985, supported by the Defense and Arms Control Studies Program in the Center for International Studies. While a a graduate student, she also engaged in other related activities. As a teaching assistant in the Harvard University Government Department, she shared her longstanding interest in the American political process with undergraduates. She spent a summer working at the congressional Office of Technology Assessment, another at the Lawrence Livermore National Laboratory in 1987. From the latter position she witnessed internal laboratory debates about the Strategic Defense Initiative. This experience led her to propose a dissertation on the Livermore weapons program.

She spent 1990-1995 in residence at the laboratory in the Center for Security and Technology Studies, which supported her dissertation research and writing. She found that technology, even when conducted in secrecy, is inextricably bound to the political and social environment in which it is produced. There are no simple answers, but her interest in science and technology policy continues. She now works in the White House, at the Office of Science and Technology Policy.