Guiding “Big Science:” Competing Agency of Scientists and Funding Organizations in American Cold War Research

by

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ABSTRACT

This research project aims to evaluate the agency of scientists participating in American Cold War research initiatives funded by the government. The aim will be to weigh the internal direction of scientific programs versus the external pressures faced from patron organizations such as the Department of Defense. The project utilizes secondary sources supported by governmental documentation as well as written and oral accounts of scientific and technical personnel involved in select research efforts. The two initiatives examined were aerospace research and its eventual adaptation to the space program, as well as nuclear testing and the national laboratories which supported it. Sources strongly suggested significant internal direction on the part of rank-and-file laboratory and technical personnel and very little pressure to orient research toward defense-related activities, despite some cooperative overlap.

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Introduction

Within the narrative of the United States' rapid ascension to geopolitical leadership, the period beginning in 1946-47 and ending in 1990 saw exceptional technological and scientific progress. This scientific progress, fueled in major part by enormous government investment, attained international reach and ultimately played a central role in defining current social and political patterns. The controversy over techno-whistleblowing, such as that undertaken by Edward Snowden, a former CIA employee, links directly to the rapid and perhaps haphazard computerization of the intelligence apparatus permitted by advances in computing during the 1970s. Concerns over militant Islam are fueled, in large part, by fears that politically unstable states could acquire nuclear or radiological weapons, a topic which first became relevant after European nuclear states failed to properly secure the technological and industrial secrets of their nuclear arms. The strong links between contemporary social and political conditions and the technological advances made under the banner of 'big science' beckon more focused appraisal of that period of scientific and technical progress.

The objective of this study is to appraise the relative agency of science as a body, and of scientists, versus the external governmental bodies which provided the funding and materiel necessary for its spectacular growth. In other words, were major scientific efforts within the United States shaped by the individual or collective interest of scientists, or by the political and military goals of those agencies which funded them? To what degree were research institutions and personnel able to insulate their goals and direction from the external pressure of their patron organizations? Was the relationship between these organizations asymmetrical or did their mutual dependency ensure a degree of genuine cooperation? How did both groups compromise their individual goals in order to ensure greater eventual gain?

The scope of this paper will be limited to two programs which were highly interrelated and which became archetypical of government-funded scientific efforts: the United States nuclear and aerospace research programs. The chronological bounds are dictated by the conveniently near-simultaneous birth of both fields; 1946 saw the dual detonations of Operation *Crossroads*, marking the beginning of the US nuclear weapon testing program, and Chuck Yeager broke the sound barrier in October of the following year. This paper will not attempt to address peripheral involvement on the part of Britain, whose efforts were frequently exploited or deliberately stifled by their wartime-allies, nor the Soviet Union, whose own progress chronically lagged behind or depended upon (due to highly effective espionage) breakthroughs made by the United States.

Historiography

Historiographical context ranges from commentaries on the history of science and peoples' interpretations of science's role in human progress to more focused appraisals of nuclear and aeronautical advancement in the last half of the 20th century. The first set of materials includes foundational texts such as physicist Thomas Kuhn's *The Structure of Scientific Revolutions*,[1] in which he proposed a non-linear model of scientific pursuit based upon popular fields of studies, or paradigms. Kuhn also broached the incapacity of traditional historical accounting for properly attributing simultaneous pursuit and discovery of ideas, and the principle of whether an idea alone or its ultimate practical

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[1] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

implementation constitutes “discovery.” In a sense, Kuhn asserted that discovery did not take place when a principle was practicable, but when it was first envisioned – and that that moment could happen simultaneously in multiple locations for multiple thinkers. Thus, history is incapable of clearly and accurately attributing a given discovery to any single individual. Kuhn’s work has gained widespread notoriety and has assumed a role as the fulcrum for considerable debate, but prior to it, Charles Gillispie’s *The Edge of Objectivity*[2] posited many of the same questions. Gillispie’s work also examined the social role of scientists, finding them culpable for discoveries which have ultimately produced harm to human society and indicting them for not exerting greater political pressure to control the eventual misuse of their scientific discoveries.

The latter half of the 20th century saw a welcome expansion of the body of scholarly research and popular writing addressing the United States nuclear infrastructure and its history. These studies were aided by geopolitical developments, particularly the easing of Cold War tensions and the eventual dissolution of the Soviet Union. Increasing access to international archival material, declassification of aging nuclear weaponry and its associated technology, and the failure of non-proliferation efforts ensured scholars documentary evidence in sufficient quantity to inspire a deluge of comprehensive and engaging histories.

American historian and Pulitzer Prize-winning author Richard Rhodes is one of the most prolific and influential figures within the field of nuclear history. His four-book series examining the nuclear age spans pre-World War II fundamental physics research to

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[2] Charles Coulston Gillispie, *The Edge of Objectivity: An Essay in the History of Scientific Ideas* (Princeton, New Jersey: Princeton University Press, 1960).

the dismantling of nuclear arsenals and the ongoing Stockpile Stewardship program. It is the first two entries in this series, *The Making of the Atomic Bomb*[3] and *Dark Sun: The Making of the Hydrogen Bomb*,[4] which are the most significant to this study. These two monographs contain intimate details of the individuals and organizations, as well as the motives, behind the creation and testing of nuclear arms.

Independent researcher Chuck Hansen assembled a more concise but intimidatingly technical history of nuclear weaponry in 1988, entitled *U.S. Nuclear Weapons: The Secret History*.[5] The product of extensive archival research and declassification requests, *U.S. Nuclear Weapons* is the most explicit and detailed resource on nuclear arms available; it contains technical drawings, design lineages, photographic component break-downs of select weapons, and details of testing procedures.

Pulitzer Prize-winning journalist Eileen Welsome's extensively-researched and enthralling *Plutonium Files*[6] examines Cold War research efforts to investigate the effects of radiation on the human body. The experiments Welsome describes were often conducted on uninformed, impoverished, or otherwise vulnerable populations and her book raises poignant questions of experimental ethics and informed medical consent. *The Plutonium Files* also describes military exercises conducted in conjunction with nuclear testing; these exercises involved movement through or decontamination of testing areas immediately after a blast in order to assess their psychological reaction and to monitor their physiological response to ionizing radiation.

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[3] Richard Rhodes, *The Making of the Atomic Bomb* (New York: Simon and Schuster, 1986).
[4] Richard Rhodes, *Dark Sun: The Making of the Hydrogen Bomb* (New York: Simon and Schuster, 1995).
[5] Chuck Hansen, *US Nuclear Weapons: The Secret History* (Arlington, Texas: AeroFax, 1988).
[6] Eileen Welsome, *The Plutonium Files: America's Secret Medical Experiments in the Cold War* (New York: Dell Publishing, 1999).

Primary sources relating to nuclear research include testimony by J. Robert Oppenheimer to the United States Congress as part of an investigation into his eligibility for continued security clearance and a firsthand interview conducted with physicist and former Los Alamos researcher Dr. L. Raymond Fawcett of Longwood, Virginia. Dr. Fawcett's invaluable contribution helps to illuminate the routine workings of the national laboratories and to elaborate upon the material discussed in secondary sources. The author also consulted Howard Morland's groundbreaking 1979 article in *The Progressive* magazine describing the design of thermonuclear weapons.[7]

The United States space program's historiography is much more visible and accessible, a circumstance largely attributable to its status as a civilian organization. From its inception, NASA operated with a great degree of openness and transparency, particularly obvious when contrasted with the secrecy of its Soviet rivals during the same period. NASA's budget and utilization of that budget was directly accountable to the United States Congress, and many discussions of budgetary and technical matters were thus immediately entered into the public record – one notable example of this is the scathing congressional hearings into the Apollo 1 fire which killed veteran astronauts Edward White and Gus Grissom, along with naval aviator and rookie astronaut Roger Chaffee.[8] This trait dramatically increased the pressure on NASA to safely and successfully carry out its mission and left the agency vulnerable to intense controversy after visible, catastrophic failures such as the aforementioned Apollo 1 fire, as well as the

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[7] Howard Morland, "The H-Bomb Secret: To know is to ask why," *The Progressive*, November 1979, https://www.progressive.org/images/pdf/1179.pdf.

[8] All Apollo 1-related congressional documentation, including the Phillips Memo which examined difficulties between primary Command Module contractor North American Aviation, and transcripts of both Senate and House hearings, is hosted by NASA at http://history.nasa.gov/Apollo204/inv.html.

losses of the shuttle orbiters *Challenger* and *Columbia*. One immeasurably positive outcome, however, is the volume of literature and testimony elucidating NASA's inner workings, much of it directly from the recollection of the personnel involved and published by, or with the help of, the agency itself; many of these resources can be considered primary sources. As a result, the United States space program is one of the most extensively-documented scientific initiatives in human history.

A comprehensive discussion of NASA's history, particularly with consideration to the Space Transportation System which played such a prominent role therein, must include the aeronautical revolution of the 1950s and 1960s. Beginning immediately after the Second World War, the United States began to invest significant effort into investigating the performance of aircraft at ever-increasing speeds and altitudes. The North American X-15 was the pinnacle of this research, reaching into space and bridging the concepts of high-speed terrestrial flight and consistent, reusable space vehicles. *At the Edge of Space: The X-15 Flight Program* is a personal memory of the program written by former X-15 pilot Milton Thompson describing the intense excitement and danger of piloting what he refers to as "…the last of the real exploratory aircraft."[9]

Author Dennis R. Jenkins is responsible for a pair of much more technical examinations of the X-15 program, *X-15: Extending the Frontiers of Flight*[10] and *Hypersonic: The Story of the North American X-15*.[11] The latter, written in conjunction

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[9] Milton O. Thompson, *At the Edge of Space: The X-15 Flight Program* (Washington, D.C.: Smithsonian Books, 1992), 271.

[10] Dennis R. Jenkins, *X-15: Extending the Frontiers of Flight* (Published by NASA and distributed digitally free of charge in EPUB, MOBI and PDF forms, hosted at www.nasa.gov/ebooks/aero_x15_detail.htm. No date of publication is provided).

[11] Dennis R. Jenkins and Tony Landis, *Hypersonic: The Story of the North American X-15* (North Branch, Minnesota: SpecialtyPress, 2003).

with Tony R. Landis, contains stunning visual documentation such as design studies and photos from production, instrumentation testing, and flight operations. *Hypersonic* goes so far as to describe the development of radical new systems necessitated by the X-15's unique characteristics, such as a hypersonic-rated ejection seat, a nose cone which would sustain superheated airflow while still providing directional and barometric data, and a reaction control system which could orient the aircraft outside of the Earth's atmosphere.

The Space Transportation System flew 135 times from 1981 to 2011 and possesses its own extensive historiography; the centerpiece of this literature is undoubtedly NASA's own *The Space Shuttle Decision: NASA's Search for a Reusable Space Vehicle*. "Independent writer and historian"[12] and Ph. D.-educated (aerospace engineering from University of Michigan)[13] author T. A. Heppenheimer chronicles the research process and political vacillations which occurred between NASA, the United States Air Force, and the Office of Management and Budget which determined the literal shape of the Space Shuttle. Another work by Dennis Jenkins, *Space Shuttle: The History of the National Space Transportation System: The First 100 Missions*,[14] expands upon the Space Shuttle's design process to include flight operations, the kinds of payloads the orbiter carried, the process of refurbishing the Shuttle equipment for subsequent flights, and the ground facilities which supported Shuttle flights. *Space Shuttle: The First 100 Missions* is an exhaustive technical history of the Space Shuttle program.

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[12] T. A. Heppenheimer, *The Space Shuttle Decision: NASA's Search for a Reusable Space Vehicle* (Washington, D.C.: NASA History Office, 1999), inside rear cover.

[13] http://www.nss.org/settlement/ColoniesInSpace/heppenheimer.html

[14] Dennis R. Jenkins, *Space Shuttle: The History of the National Space Transportation System: the First 100 Missions* (Stillwater, Minnesota: Voyageur Press, 1992).

Primary sources relating to the space program include a 1986 NASA report evaluating Soviet progress in space stations, outlining the *Salyut* program, looking ahead to *Mir* and discussing how those stations compared to historical and planned US space stations, entitled "Soviet Space Stations as Analogs."[15] The *Columbia Accident Investigation Board*'s final report was also very helpful not only for determining the causal factors which led to the *Challenger* and *Columbia* accidents, but also for providing a comprehensive yet concise history of the Space Shuttle system.

Scientists' Agency

Scientists hold the exceptional status of being the first to envision solutions, the first to posit new avenues of research. In the vast majority of cases, application follows the formulation or discovery of principles, rather than the other way around. In fact, fundamental science has often been viewed as an island, and indeed, until large-scale government involvement, acted as one; individuals across the world endeavored to investigate scientific phenomena, replicating experimental configurations of which they had heard or read. In fact, this is the genesis of Kuhn's thesis – since science has historically been so geographically disparate and communication so immature, private scientific efforts produced identical results within a narrow chronological window in multiple places, by multiple people. Prior to 'big science,' anyone could be a scientist; even within the contemporary model of the government-funded scientific community, individual achievement and notoriety is a primary incentive for discovery. Conversely,

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[15] B.J. Bluth and Martha Helppie, "Soviet Space Stations as Analogs, 2nd Edition," (Washington, D.C.: NASA, 1986), 3. Accessed via NASA Technical Reports Server (NTRS), http://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/19870012563.pdf.

application requires both foreknowledge and, often, significant investment. Whether in producing light bulbs or nuclear reactors, the transition between experimentation and manufacture presents an enormous organizational and material step. The organizational considerations of applying and manufacturing the results of scientific discovery present a sort of inertia to which individual scientists are not subject. Scientists possess tremendous potential agency, but then some – such as Gillispie – might retort: why do they not exercise it? In his 1960 essay, *The Edge of Objectivity: An Essay in the History of Scientific Ideas,* he writes:

> Whatever the private opinion of scientists, their civic role has normally been to provide the governing authorities with powers, while drawing authority and resources from the state for science. The relation of scientists to the state in modern times has been one of partnership rather than partisanship, whether for or against programs of particular political parties. In my view, this consideration is further evidence that science is intrinsically relevant to means and indifferent to the choice of ends.[16]

Gillispie unfairly holds scientists complicit for the eventual application, whatever it may be, of the knowledge they uncover. Conveniently ignoring the vicious political schism over the hydrogen bomb which had occurred in the early 1950s, Gillispie's implication serems to be that the only morally acceptable means for scientists to protest is to simply *stop* doing science. His subtle disenfranchisement of scientists does more to restrict their agency than their acquiescence to national or military priorities ever could.

In fact, scientists have very aggressively articulated their priorities through major scientific programs, particularly in the United States over the last fifty years, even when those programs have enormous relevance to defense. They have often worked in parallel with military initiatives, diverging only when it was opportune to do so. Through

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[16] Gillispie, *Edge of Objectivity*, xvii.

selective cooperation and the subtle, prolonged pressure exerted on their parent programs toward their individual goals, scientists reshaped ‘big science’ to realize their own dreams. The following are two broad, programmatic examples of research and technical personnel pursuing paths of fundamental scientific inquiry within programs with overarching military involvement.

Chapter I: Aerospace Research

The Origins of the Space Station

During the height of the Apollo Program, NASA decided to place its hopes for the future of manned spaceflight in a radical departure from then-operational systems and pursue permanent space stations. Throughout the 1960s, NASA's immediate goals were broad in scope, but its overall mission was highly constrained; many new techniques, technologies, and competencies were developed in support of a specific goal – mounting a manned mission to the moon. As the completion of this objective appeared imminent, in the late 1960s, key minds within the scientific community, anticipating a potentially painful transition to a yet-undetermined agency-wide mission, were already contemplating the future of human spaceflight. Of NASA's looming identity crisis, aerospace historian Dennis Jenkins writes, "Separate from, yet intimately related to, the efforts to define what sort of reusable launch vehicle was desirable were the efforts to determine the future of the entire American space program."[17] Keen to preserve technological momentum and the myriad design and manufacturing jobs which had been created during the Apollo Program, NASA sought to define a long-term mission, and with it, a launch vehicle which could meet the demands of that mission. Ultimately, the organization determined to pursue what had long been a fantasy of scientists and artists: permanent, orbiting space stations.

The personal scientific interests of prominent scientific and artistic individuals strongly influenced NASA's ultimate decision to pursue a permanent orbital space station. Until the middle of the 20th century, artists and scientists were the only two

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[17] Jenkins, *Space Shuttle*, 78.

groups contemplating permanent human habitation of space. Defense planners only began to support the idea once the cleavage of the world into East and West, coupled with technological advances, made the prospect of space stations appealing. One of the first mentions of space stations was by a Unitarian minister from Boston, Edward Everett Hale. *Boys' Life* writer Neil McAleer notes, "In 1869, the *Atlantic Monthly* magazine published Hale's story, "The Brick Moon"… Hale's station was a hollow brick sphere 200 feet in diameter that could be shot into space by waterpower."[18]

A contemporary of Hale's living on the other side of the globe, Konstantin Tsiolkovsky, also considered the possibilities for manned settlement of space. Tsiolkovsky resided in Tsarist Russia during the late 19th century; he was a prolific thinker and designer, envisioning many spaceflight concepts which eventually became reality. Considered one of the pioneers of rocketry and spaceflight, Tsiolkovsky "…generated a stream of technical ideas ranging from metal aircraft and multistage rockets to solar sails, orbiting space stations, and transparent domed habitats on the surface of asteroids."[19]

Space historian T.A. Heppenheimer traces the historical lineage of space stations at length, beginning with speculative work by one of the founders of rocketry, and a protégé of Tsiolkovsky, Hermann Oberth. In the 1920s, Oberth drew a parallel between submarines and the eventual shape of spaceflight:

> Having demonstrated to his satisfaction that space flight indeed was achievable, Oberth then considered its useful purposes… he envisioned that similar crews,

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[18] Neil McAleer, "The First Space Station was a Big Hollow 'Moon' Made Entirely of Brick," *Boy's Life* (September 1989).

[19] Vladimir V. Lytkin, "Tsiolkovsky's Inspiration," *Ad Astra* Vol. 10, Issue 9, 1998. P34-39.

> with oxygen provided through similar means, would live and carry out a variety of tasks in a space station as it orbited the Earth.[20]

The author notes that Oberth's notions of the precise dimensions and capabilities of such a station were very vague, partially due to the immature state of rocketry, but, he asserts, "Although Oberth was shy and retiring by nature, the impact of his ideas, during subsequent decades, would rival that of von Braun's a generation later."[21] As rocketry advanced, the concept of a permanent habitable platform in space slowly became more tangible. For this to happen, the aspirations of Oberth and Goddard had to be transmitted to a younger generation; according to Heppenheimer, art played a prominent role in perpetuating the dream of a space station.

> Fritz Lang, a leading German film producer, then became interested… Drawing heavily on Oberth's writings, Lang's wife, actress Thea von Harbou, wrote the script for *Frau im Mond*. Fritz Lang hired Oberth as a technical consultant. Oberth then convinced Lang to underwrite the building of a real rocket… The project attracted a number of skilled workers… Among them, a young Wernher von Braun.[22]

The 1929 publication of *The Problem of Space Travel* by Austrian Herman Potočnik introduced the classical rotating wheel station design, which von Braun adopted; Heppenheimer writes, "Except for being two and a half times larger, von Braun's *Collier's* space station closely resembled that of Potočnik, and it is tempting to view von Braun as the latter's apt pupil."[23] It is clear, then, that the eventual scientific leader of the United States' space program was interested in the concept of a space station long before the Second World War.

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[20] Heppenherimer, *Space Shuttle Decision*, 7.
[21] Ibid., 9.
[22] Ibid.
[23] Ibid., 11.

Though the war constituted a devastating global interruption, it did not deter the upward gaze of science or the arts – if anything, the advances in rocketry enhanced interest in the exploration and development of near-Earth space. Renowned science fiction author Arthur C. Clark wrote in 1945:

> Using material ferried up by rockets, it would be possible to construct a "space-station" in such an orbit. The station could be provided with living quarters, laboratories and everything needed for the comfort of its crew, who would be relieved and provisioned by a regular rocket service…[24]

Clarke's vision of space operations is a far cry from Oberth's early prophecy, and mirrors the eventual logistical reality of maintaining the International Space Station. Jenkins describes the post-war coalescence of a prescient fraternity of space visionaries and their growing consensus that space stations were not merely a possibility, but an inevitability:

> The First Symposium on Space Flight was held on 12 October 1951 at the Hayden Planetarium in New York City… contributors included von Braun, Joseph Kaplan, Heinz Haber, Willy Ley, Oscar Schachter, and Fred Whipple. Topics [included] manned orbital space stations and orbiting astronomical observatories…[25]

It is important to note that this interest in space habitats for scientific observation predates military interest in space stations by a decade. At a time when military minds were still coming to terms with the ramifications of the internal combustion engine, the jet turbine, and aircraft, early interest in space stations was driven entirely by artists, thinkers, and, of course, scientists. Despite scientific fervor for space stations, the concept would have to wait – first through the aeronautical revolution of the 1950s and then through the decade-long sprint to the moon.

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[24] Heppenheimer 55 orig. Clarke

[25] Jenkins, *Space Shuttle*, 4.

Military Applications

Throughout the period of technological innovation spurred by Kennedy's appeal to the nation to land a man safely on the moon, delivered in Congress on May 25, 1961, the military often struggled to apply NASA's discoveries in strategically relevant ways. Advances which were indigenous to the civilian manned space program often required significant mission-specific alteration, or were not sufficiently closely related to immediate military application to justify the required expenditure. Such was the case with Blue Gemini, an Air Force initiative to launch a series of Gemini missions for the specific purpose of developing proficiency in manned spaceflight.

The ultimate goal of the USAF was to develop a space-borne reconnaissance station, known as the Manned Orbital Laboratory. MOL, which would be connected to a modified Gemini capsule, be manned by two astronauts, and would make observations from orbit of strategic displacements. The concept had its roots in the Dyna-Soar project, which will be discussed later, and suffered much the same fate, cancelled in 1969 after the selection of 17 astronauts. Technology had advanced to the point that unmanned satellites adequately filled the surveillance role and the Department of Defense could no longer justify the tremendous expenditure manned spaceflight demanded. The death of military aspirations for a space station, however, coincided with the realization of those same aspirations for peaceful purposes. Military interest in space did not end with the failure of MOL, as its eventual accommodation in the Space Shuttle's design proved, but its immediate demand for manned capability ceased as remote reconnaissance technology matured.

Early Space Stations

The Apollo Program was a vital step in American manned spaceflight, less for its ultimate objective than for the myriad proficiencies it fostered in obtaining that objective. The American effort to mount a lunar landing was structured into three separate programs. Mercury was intended to prove the basic capabilities required for manned spaceflight, such as achieving stable orbit, reentering the atmosphere and recovering astronauts and their vehicles from the ocean, where they landed. The second program, Gemini, developed skills such as spacewalking (physically exiting a vehicle and performing activities in space), rendezvous and docking between multiple vehicles, and increased mission duration. Apollo demanded mastery of extra-vehicular activity, the rendezvous and docking of multiple vehicles, a reliable heavy lift launch vehicle, and a versatile crewed capsule. While Project Gemini first explored many of these capabilities, it did not synthesize them in the same way that Apollo did; Project Apollo promoted the interconnection of techniques that were vital in permanent space operations and, for the first time, permitted the construction and servicing of a space station. Prior to Apollo, space settlement could be envisioned only speculatively because the expertise required to sustain it did not exist. Seizing upon this fundamental shift in knowledge and correctly anticipating an organizational crisis as the Apollo Program wound down, Wherner von Braun, now NASA's primary rocket designer, acted preemptively, cultivating alliances with several other influential administrators. Von Braun's urgency derived from concerns over the well-being of his personnel and the irreplaceability of their talent:

> The ubiquitous von Braun played a key role in initiating this new effort [the Apollo Applications Program], not because he succeeded in convincing senior

> NASA officials of the merits of a space station, but rather because he knew that his staff would soon need new work.[26]

Von Braun gained support from Associate Administrator for Manned Space Flight George E. Mueller and the Office of Space Science and Applications for a program based around the promising new Saturn IB and Saturn V vehicles. However, in what would become habit for the space program, early visions of the program were, to be generous, overly optimistic:

> …NASA's initial schedule envisioned 26 launches of the Saturn I-B and 19 of the Saturn V. Flight hardware would include three S-IVB stages intended for on-orbit habitation, four ATMs [Apollo Telescope Mount], and three more capable space stations that would ride atop the Saturn V.[27]

Project Apollo could only spare a single Saturn V, the massive and incredibly expensive rocket originally intended to deliver men to the moon, and what eventually came to be known as *Skylab*, America's first space station, constituted a single S-IVB upper stage (the top-most portion of the Saturn V rocket, in *Skylab*'s case, with its fuel tanks removed in order to provide space in which astronauts lived and conducted scientific observations) which was launched on May 5, 1973 and visited by manned Apollo capsules on three separate occasions. The *Skylab* missions lasted 28 days, 59.5 days, and 84 days respectively, a simultaneously meager yet momentous start for American steps to permanent space habitation. Though the American space program's space station effort was built upon personal initiative and investment in the idea, so too was the Soviet Union's – and it was significantly further developed than its American counterpart.

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[26] Heppenheimer, *Space Shuttle Decision*, 60.

[27] Ibid., 63.

Though the Soviet Union failed to beat the United States to the moon, it excelled at building and servicing space stations, skills which proved invaluable to the success of the International Space Station thirty years later. The *Salyut* Program of the early 1970s, the Soviet Union's first attempt to construct a space station, was, in large part, cover for the military Almaz series of orbital space stations, but it still had a very important scientific mission. NASA noted congruence between *Salyut*'s objectives and their own in a 1986 study:

> There is considerable similarity between the Soviet Salyut Space Stations and the proposed American Space Station… The goals of the Soviet Space Station missions are primarily governmentally sponsored and are scientific, experimental and/or military (though the extent of the military is not known).[28]

NASA seemed to be unaware of the split responsibilities of the *Salyut*/*Almaz* programs, and as such was unaware of just how closely *Salyut* approximated American goals in space. The two superpowers may have had identical reasons for pursuing space stations, but the Soviet Union was incomparably more experienced in their implementation; the USSR launched *Salyut* a total of six times, four successfully. By the time NASA published their evaluation of the Salyut Program, construction had begun on *Mir*, a spectacular, multi-component station over six times the mass of *Salyut*. The United States was falling uncomfortably behind the Soviet Union in what was becoming apparent as the future of the space race; however, an audacious plan, for which the iconic Space Transportation System, also known as the Space Shuttle, was explicitly designed, sought to overtake the Soviets in a single leap.

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[28] Bluth and Helppie, "Soviet Space Stations," 3.

The Space Transportation System, America's first manned space vehicle following the end of the Apollo Program, was designed with the implicit imperative of constructing and maintaining a massive, permanent space station. Political vacillations and efforts to overcome budgetary opposition expanded the vehicle's mission to an unrealistic and, ultimately, fatal degree, but its original purpose was to advance the state of the art in long-duration manned spaceflight. The Columbia Accident Investigation Board's (CAIB) final report endorses this evaluation, stating, "NASA centered its post-Apollo plans on developing increasingly larger outposts in Earth orbit… The space agency hoped to construct a 12-person space station by 1975; subsequent stations would support 50, then 100 people."[29]

Note that these ambitions seem completely unaffected by the bitter disillusionment of the Apollo Applications Program being pared down to a single *Skylab* article and three crewed Apollo visits; the CAIB adds that, "NASA's vision of a constellation of space stations and journeying to Mars had little connection with political realities of the time."[30]

Aeronautics: The Foundations of the Space Shuttle

The Space Shuttle's purpose, the construction of a large, permanent space laboratory, was a peaceful, civilian vision for space development; however, the Shuttle's physical manifestation was founded upon decades of aeronautical research, facilitated by military aid, but fundamentally administered by civilian agencies. The Space

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[29] *Columbia Accident Investigation Report: Volume 1* (Ontario: Apogee Books, 2003), 21

[30] Ibid., 22.

Transportation System was the final product of a research lineage exploring regimes "higher and faster," which began in in 1947 when Charles Yeager broke the sound barrier in level flight at the controls of the rocket-powered Bell X-1. Military involvement in this effort was absolutely essential at all stages, but most frequently took the form of enthusiastic offerings of both personnel and materiel aid.

The knowledge which laid the groundwork for Yeager's historic flight and which underpinned the pursuit of "higher and faster" was the result of advanced aerodynamic research which started prior to World War II. Transonic[31] research first began after World War I in several locations across Europe. England constructed a supersonic tunnel in 1922 at the National Physics Laboratory (NPL), and another in 1942 which the NPL claims, "…was the only sizeable supersonic tunnel possessed by the Allied Nations for most of the Second World War."[32] Meanwhile, Germany was rapidly expanding its mastery of aeronautics as well:

> The first supersonic wind tunnel based on the vacuum-storage principle in Germany was put into operation in 1936 at the TH Aachen by the Institute of Aerodynamics headed by *Wieselsberger*… In 1938, the AVA in Gottingen started operating the 'High-Speed Wind tunnel,' which… reached a Mach number of 3.2.[33]

This heavy investment in aeronautics benefitted Nazi Germany by fostering exotic aircraft designs which utilized advanced aerodynamic principles.[34] Though Germany's

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[31] Transonic refers to airspeeds approaching the speed of sound.

[32] "Aerodynamics," *NPL.co.uk*, http://www.npl.co.uk/about/history/research/wind-tunnels/ accessed 5/25/2015.

[33] Ernst Heinrich Hirschel et. al., *Aeronautical Research in Germany: From Lilienthal until Today*, p. 197

[34] The Go-229 flying wing interceptor, a refinement of research on blended-wing-body aerodynamics conducted by the Horton brothers in Germany, never saw combat and only a few examples were built. However, in addition to its radical shape (which granted it a smaller radar signature), the Go-229 featured engines mounted atop the fuselage, forward of rearward sloped bodywork which shielded the exhaust from observation from below. This inadvertent thermal stealthing was a central design component of the American B-2 bomber and the YF-23 prototype Advanced Tactical Fighter. The swept wings (which

industrial infrastructure was too vulnerable to allied bombing and the war was at too late a stage for these designs to alter the course of the conflict, much like German rocketry under Werner von Braun, the concepts immeasurably impacted post-war research efforts.

American aeronautical efforts capitalized on German progress during World War II and were aided by military involvement in many ways, but were administered by the National Advisory Committee for Aeronautics (NACA), a federal agency completely unaligned to any military body. NACA was established in 1915 with the intention of overseeing fragmentary work on aeronautics then taking place at academic institutions and research centers around the nation. Within a short time, NACA grew to directly administer many of these efforts. In 1947, NACA personnel oversaw Chuck Yeager's record-breaking flight in the X-1 at the Air Force's Muroc Field. Muroc Field was eventually redubbed Edwards Air Force Base, the moniker by which it became famous as the home of all major American aerospace research; as the direct predecessor of the National Aeronautics and Space Administration (NASA), NACA was behind many of these projects.

Undoubtedly, the primary reasons that NACA enjoyed extensive military backing were that its research projects translated seamlessly to military application (unlike later manned space equipment developed by NASA) and its research direction ran parallel to preexisting Air Force interests. The NACA cowling, for example, which helped cool and streamline radial engines in aircraft, was a simple and effective development which saw widespread adoption throughout the 1930s in both civilian and military aircraft. The

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mitigate compressibility effects) of both the Me-163 rocket interceptor and Me-262 fighter prompted crash efforts to incorporate the feature into all second-generation Allied jets being designed after the end of the war. By the early 1950s, the swept wing was ubiquitous throughout international military aviation.

achievement of greater speeds and altitudes translated directly to better performance and survivability for military aircraft. Each program possessed performance objectives it aimed to achieve or aerodynamic regimes it intended to explore; each new step in that succession expanded not only human knowledge of flight dynamics, but more importantly, it expanded the potential performance envelope of military aircraft.

The Air Force's commitment to NACA's work can be viewed much like the prospects of investors in the private sector who often have little concrete hope of witnessing return on their investment, so they strive to wisely and responsibly invest in businesses or industries that are promising and which relate to them. Like the National Laboratories, NACA was a research body established separately from any single military interest in order to broaden its usefulness. Money spent on NACA benefitted civilian aeronautics as well as other branches of the military, whereas that same money given to any one military branch for research purposes would yield far more specialized, more limited, and more controlled information. The Air Force frequently consulted with NACA and aircraft manufacturers throughout the development of experimental aircraft to ensure that its interests were the subject of research, but it did not, itself, possess the research capability to *ensure* the results of such developmental programs – that would have obviated the necessity of NACA. Instead, it simultaneously worked closely with NACA and aircraft design teams to ensure that the parameters of each project were tailored to its specific interests and concerted its efforts into studying how NACA's discoveries could be adapted to military usefulness. The USAF's role in this relationship *could* certainly be viewed as manipulative, but a more accurate portrayal is one of symbiosis; the Air Force was attempting to ensure a reasonable return on the massive investment they were making to civilian research. The Air Force's interest in NACA and cooperation with the organization, self-serving though it may have been, was responsible for enormous gains in aerospace knowledge in the two decades after Yeager's flight.

The pace of NACA's work accelerated dramatically in the 1950s and its progress continuously proved to be applicable to the armed services. Following Bell's success with the X-1, their swept-wing X-2 explored airspeeds from Mach 2 to Mach 3. In 1952, Douglas Aircraft's X-3 utilized a low-aspect ratio wing,[35] a central design tenet of the venerable F-104 interceptor, which made its first flight four years later. Bell's X-5 demonstrated a variable geometry wing[36] in 1951; in 1964, General Dynamics' F-111 became the first production aircraft with a variable geometry wing. Some concepts were direct testbeds for military systems, like the X-10 and X-11, which served as early examples of what would become the Navajo and Atlas missiles, respectively. The late 1950s saw investigation into vertical take-off and landing capability with the X-13 and X-14 test vehicles, but both aircraft preceded any military requirement and so were not pursued. Despite all these successes, however, the true revelatory moment in American aeronautical research occurred in 1959, with the first flight of the North American X-15.

X-15: Shuttle Prototype

North American's X-15 was the most ambitious, most scientifically rewarding research aircraft in history. Neil Armstrong, the first person to walk on the moon and

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[35] Aspect ratio, in aerodynamics, is the ratio of the length of the wing to its width. A high value indicates a long, thin wing; a low aspect ratio is indicative of a short, wide wing.

[36] Variable geometry wings are capable of varying their angle of sweep depending on conditions such as speed, altitude, and energy state.

former X-15 pilot, described its momentous influence on aerospace history in the forward to Milt Thompson's *At the Edge of Space*:

> It had taken half a century for aircraft to reach Mach 2 and 80,000 feet. Now one new design would attempt to triple those achievements… The X-15 would accomplish all its goals and more… it would become the most successful research airplane in history.[37]

Armstrong's claim may seem hyperbolic,[38] but the X-15 met, and in most cases vastly exceeded, all of its design goals. In *Hypersonic: The Story of the North American X-15*, authors Dennis Jenkins and Tony Landis claim, "The X-15 would ultimately exceed all of its performance goals. Instead of Mach 6.5… and 250,000 feet, the program would record Mach 6.7… and 354,200 feet."[39] Of course, achieving a speed of Mach 6.5 was itself a daring proposition; the last major milestone, Mach 3, had cost the pilot, Air Force Captain Milburn Apt, his life and ended the X-2 program.

The X-15 was not only extraordinary on paper; its construction necessitated brilliant solutions to very exotic and specific design challenges. The X-15 was a sleek, black aircraft 51 feet long. Its broad, stubby wings and vertical control surfaces were located toward the tail, which held the powerful XLR-99 rocket engine. Because the 57,000 lbf XLR-99 was capable of propelling the X-15 to nearly seven times the speed of sound, the X-15's skin was composed of a nickel superalloy known as Inconel-X to resist the great temperatures generated by friction. The X-15 also made highly parabolic flights

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[37] Thompson, *Edge of Space*, xii.

[38] There may certainly be some degree of personal affinity involved; prior to his selection by NASA as an astronaut, Armstrong was an X-15 pilot.

[39] Jenkins and Landis, *Hypersonic*, viii.

from launch altitude[40] to near-space where not enough air is present to affect changes in the aircraft's direction using aerodynamic control surfaces. As a result, the aircraft utilized a primitive reaction control system, similar to those eventually employed on Projects Mercury and Gemini in order to control itself at extreme altitudes.[41] Nearly every facet of the X-15's design anticipated broad, and sometimes very extreme operating conditions, which made the aircraft very versatile and ultimately allowed the program to have such a broad scientific bounty. This built-in versatility can be attributed to the unique circumstances and design philosophy under which the X-15 materialized.

The X-15 program benefitted from coming conveniently prior to major manned space efforts. Dennis R. Jenkins, who has authored numerous detailed histories of the X-15 program, writes:

> John Becker, arguably the father of the X-15, once stated that the project came along at 'the most propitious of all possible times for its promotion and approval.'… There were no 'glamorous and expensive' manned space projects to compete for funding, and the general feeling within the nation was one of trying to go faster, higher, or further.[42]

Preceding manned space efforts carried with it a great responsibility; the X-15 would essentially explore the last, transitory step between aeronautics and astronautics. It would have to bridge two vastly disparate operational regimes, and it was correspondingly over-

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[40] The X-15 could not take off from a runway; like its predecessors, it was carried aloft by a modified Air Force bomber and dropped. After several seconds of freefall, the engine was lit and the aircraft proceeded under its own power.

[41] One particular airframe, the X-15-3, possessed a very early flight control computer, the Minneapolis-Honeywell MH96, which anticipated this atmospheric gradient and proportionally decreased control authority of the aerodynamic surfaces whilst simultaneously blending in increasing control authority from the RCS. Thus, instead of switching from a central joystick controlling the air surfaces to a dedicated RCS joystick, pilots of the X-15-3 could continuously use a single joystick throughout all altitude regimes. As a result, the X-15-3 became NASA's high-altitude workhorse.

[42] Jenkins, *X-15*, 9. (Because this source is available in multiple formats, page numbers vary; those listed here will correspond to the PDF file available here: http://www.nasa.gov/pdf/601242main_X15ExtendingFrontiersFlight-ebook.pdf)

engineered in many ways. Rather than being built to represent an eventual future system, the X-15 was designed to be a fully capable, well-rounded research article, as Jenkins once again elucidates:

> [John] Becker once opined that proceeding with a general research configuration rather than a prototype of a vehicle designed to achieve a specific mission was critical to the ultimate success of the X-15. Had the prototype route been taken, Becker believed, 'we would have picked the wrong mission, the wrong structure, the wrong aerodynamic shapes, and the wrong propulsion.'… Indeed, several of the proposals for the X-15 sought to design a vehicle with some future application. Nevertheless, the original Langley concept of a vehicle optimized to collect the desired data as safely as possible ultimately won.[43]

Becker's remarks clearly satisfy a key component of this argument in a relevant framework of intimate cooperation between NACA/NASA (purely civilian agencies) and the United States Air Force. The X-15 was designed as a tool for fundamental scientific and aeronautical study, with absolutely no consideration for eventual military application. The relationship between the Department of Defense and civilian scientific bodies, in this case, was one of cooperation and mutual consultation. It was a highly improvisational and informal arrangement, as Jenkins further explains:

> …it is apparent that the most important lessons… from the X-15 concern not the hardware, but the culture… The Military and NACA initiated and funded the X-15 program without congressional approval or oversignt, although this was not an effort to hide the program or circumvent the appropriations process. The military services had contingency funds available to use as they saw fit. They ultimately needed to explain to Congress and the White House how they spent the funds, but there was little second-guessing from the politicians.[44]

It is important to note that while the X-15, developed in 1958 and flown throughout the 1960s, was not envisioned as a prototype for any future vehicle, the development of the Space Shuttle in the late 1970s established a retroactive design

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[43] Ibid., 7.

[44] Ibid., 8.

lineage with the X-15 at its genesis. Many of the techniques developed during the X-15 program were further developed for use in the Shuttle Program. The X-15 studied hypersonic heating and directional control throughout a broad altitude range, which the Shuttle would require due to its operation both within and outside of the atmosphere, and its deceleration from orbital velocity to hypersonic and eventually subsonic flight. The X-15 also developed techniques for precisely managing energy on approach to its final landing site; both the X-15 and the Shuttle performed what is called a "dead stick" landing, meaning they were unpowered. Because of this, their airspeed at landing had to be achieved on a single approach, as no additional propulsion was available in case of error. Like its unforeseen successor, the X-15 had a meager payload bay from which it conducted various scientific experiments to investigate the upper atmosphere and near-space throughout its 199 flights.

The Space Shuttle: Audacity and Compromise

From its inception, the Space Shuttle program (Space Transportation System, or STS, was its official name) was defined by compromise and the dichotomy between what NASA engineers and scientists intended the system to accomplish (the construction of a space station), and what external organizations would permit the architecture to do. The Shuttle was a grandiose, sweeping vision of what NASA intended its mission to be, but this mission was only nominally conscious of the economic and political factors which would ultimately circumscribe it. Public fervor for spaceflight had been misread; the political and social momentum for manned spaceflight had not been indicative of a general interest in science, but rather a public reaction to Soviet advances in the field.

This had become clear to the agency as the Apollo Program was prematurely curtailed, but, by that time, the initiative to build the Shuttle was well underway.

The Space Shuttle was undoubtedly one of the most ambitious and nationalistic technological efforts the United States ever undertook. Two of the primary reasons for its nationalistic implications were its sheer audacity and the radical departure it represented from any preexisting manned space system. Its most prominent feature, a massive delta wing planform, allowed it to land on conventional runways (albeit very long ones). The heat-resistant tiles which formed the Thermal Protection System permitted it to be refurbished and flown repeatedly – no other individual manned spacecraft had ever flown twice. Further aiding this reusability, the twin solid rocket motors which powered the first two minutes of the Shuttle's ascent were equipped with parachutes and flotation devices. After each flight, they too were collected and returned to flight-worthy status for future use. The Shuttle was an incredibly versatile system, capable of lifting and deploying massive payloads, returning them to earth inside its cargo bay, doing science in orbit. At once both a successor to and complete departure from the Saturn V which preceded it, the Space Transportation System lifted a mere 20% of the mighty Saturn's payload capacity, but did so regularly from 1981 until 2011. The Shuttle also possessed an expansive habitable area compared to Apollo; it was qualitatively unlike any other manned space system which had ever flown before. However, the Space Shuttle was not merely an accumulation of technological superlatives.

The Space Transportation System, as the Space Shuttle was formally known, was originally NASA's formulation of a future unfettered by military imperative. Since the organization's inception, NASA had been tasked with the explicit goal of fulfilling a military-political objective – landing men on the moon. Having completed that mission in spectacular fashion, NASA began to look for longer-term missions in order to ensure the security of the program and to direct future technological progress. To this end, NASA devised an ambitious plan which combined recent initiatives at settling near-Earth space with seemingly-unrelated yet highly successful aerospace research being conducted in the Mohave Desert.

During its early conceptual history, the physical configuration of the Space Shuttle was anything but definite. Even before the Apollo Program achieved its goal, a deluge of studies suggested than myriad design approaches could conceivably attain the performance goals and conduct the mission of constructing and servicing space stations:

> Since the early 1960s, shuttle advocates had been bedeviled by a multiplicity of reusable launch vehicle concepts, all of which could claim the name of a shuttle. In their day these had included boosters powered by scramjets or by LACE, horizontal-takeoff vehicles employing a rocket sled, and behemoths such as the Nexus that matched the weight of an ocean liner. These had fallen by the wayside, but the range of concepts had remained uncomfortably broad: expendable boosters with reusable upper stages, stage-and-a-half partially-reusable configurations such as Lockeed's Star-Clipper, two-stage, fully-reusables such as General Dynamics's Triamese. This was somewhat like having the Air Force propose to build a new military airplane, without specifying whether it would be a fighter, bomber, or transport.[45]

The first tentative steps toward solidifying NASA's idea of the Space Shuttle came in 1969, and were developed by an aerodynamicist who had roots in NACA and had worked on the X-15 program: Maxime Faget.[46] Faget proposed a two-stage configuration where both stages were winged vehicles, the larger one serving as a launch carrier for the orbiter, which would, at significant altitude and speed, separate and continue into orbit.

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[45] Heppenheimer, *Space Shuttle Decision*, 134.

[46] Ibid., 206.

The smaller orbiter possessed a small, straight, low-aspect ratio wing which would generate appreciable lift only *after* reentry. The bulk of the retarding force acting on the vehicle throughout atmospheric interface would be derived from blunt-body drag, which Heppenheimer describes:

> Even with thermal protection, he did not want to fly his shuttle during reentry, in the manner of an airplane: ‘It’s a hell of a lot eaiser to do a no-lift entry than a lifting entry...’ With airplane-style reentry, ‘you are stuck in the atmosphere, going fast for a long time.’ Rather than lose energy to a shock wave, the airplane would experience drag through friction with the atmosphere which would transfer heat to its surface.”[47]

Faget wanted a brief period of reentry which best protected the vehicle from the thermal effects of high velocity interface with the upper atmosphere. His proposal also ensured total reusability of the system, but envisioned an orbiter which would have exited atmospheric entry in an aerodynamic stall after which it would have conducted a nose-down dive to gain enough airspeed to land safely at the low speed Faget desired (approximately 130 knots).[48] The Air Force abhorred the notion of deliberately sacrificing aerodynamic control during such a crucial moment, and Air Force Flight Dynamics Laboratory (FDL) scientist Alfred Draper, among others at FDL, vehemently criticized Faget’s approach; “Draper preferred to have the shuttle enter its glide while still supersonic, thus maintaining much better control while continuing to avoid aerodynamic heating.”[49] Draper’s approach carried with it problems related to the shift in the wing’s center of lift, dependent upon the vehicle’s speed. The Air Force, however, due to its long history of supersonic and hypersonic research, was prepared to address this problem:

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[47] Ibid., 209.
[48] Ibid.
[49] Ibid., 210.

> The Air Force had extensive experience with supersonic fighters and bombers that had successfully addressed this problem, maintaining good control and handling characteristics from Mach 3 to touchdown… the preferred solution was a delta wing, triangular in shape… Such aircraft… relied instead on elevons, control surfaces resembling ailerons set at the wing's trailing edge. Small deflections of these elevons then compensated for the shift in the center of lift, maintaining proper trim and balance without imposing excessive drag.[50]

Faget disliked the weight such a system would impose through thermally protecting a larger wing, and the greater landing speed that weight demanded. Draper countered that the delta configuration offered both a smoother transition in flight characteristics between supersonic and subsonic flight regimes, and the thick wing roots of a delta offered ample room for landing gear. This early conflict between NASA and Air Force personalities, their competing ideas promising to shape the very structure of the Shuttle, was a far cry from the "good old days" of cooperation in earlier aeronautical efforts in the Mohave Desert. Ultimately, securing Air Force backing for the project was a stronger influence upon NASA's leadership than the safety and simplicity benefits Faget's proposal boasted. They were presented with a simple, yet agonizing, choice: to settle for a vehicle much larger and much more dangerous than the one they originally envisioned to court the United States Air Force, or to abandon manned spaceflight. NASA chose the former. It exerted its collective agency by forming a tenuous and very imbalanced (NASA *needed* the Air Force; the Air Force did not need NASA) political alliance in order to gain political support, and thus funding, for the Space Shuttle.

By the mid-1970s, it was clear even to NASA that their financial future was in jeopardy. An energy crisis, waning public and legislative interest, and growing sophistication and expense of competing military systems contributed to the rapid decline

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[50] Ibid., 211-12.

in NASA funding. That these budgetary issues prematurely ended the Apollo Program and severely curtailed the Apollo Applications Program were dire enough, but NASA was dreading the cancellation of the Shuttle. The only means of saving the program lay in decreasing its cost by broadening its usefulness. Jenkins adeptly conveys the urgency of the situation:

> It became clear, even during NASA's early in-house analyses, that any economic justification depended on the Space Shuttle being the only U.S. launch vehicle during the 1980s… In particular, it was crucial for NASA to gain agreement from the national security community to use the Space Shuttle to launch all military and intelligence payloads, which were projected to be roughly one third of all future space traffic. Thus, DoD support was crucial on both political and economic grounds.[51]

In order to appeal to the Air Force, the Shuttle was going to have to fulfill specific mission requirements, namely the ability to place reconnaissance satellites into polar orbit. Accommodating this requirement was no simple task, and it would fundamentally alter the structure of the Space Shuttle. Military satellites typically circle the earth in polar orbits, so called because their north-to-south (or conversely, south-to-north) orientation brings them over the poles; "While a satellite orbit remains fixed in orientation with respect to distant stars, the Earth rotates below this orbit. This permitted single reconnaissance missions to photograph much of the Soviet Union."[52] Though highly convenient for purposes of national defense, this arrangement made the hypothetical Shuttle's mission orders of magnitude more difficult. Launching into polar orbit meant the Shuttle would not only lack the speed benefits of launching with the

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[51] Jenkins, *Space Shuttle*, 99.

[52] Heppenheimer, *Space Shuttle Decision*, 214.

Earth's direction of rotation, but also that its landing point would move out from underneath it during flight.

The ability to traverse the distance between where the Shuttle launched, and where that location would be after a single orbit, was known as cross-range, and covering it was a monumental challenge. Jenkins corroborates that, "… the single requirement that had the most impact on the Space Shuttle design was that for high cross-range, which is the ability to maneuver to either side of the vehicle's ground track during reentry."[53] The Air Force once again believed it had the answer in the delta wing planform, as Heppenheimer notes, "Compared to a straight wing, it produced considerably more lift at hypersonic speeds. Using this lift, a reentering shuttle could achieve a substantial amount of crossrange…"[54] As noted by Faget and others, this exposed the orbiter to potentially very long periods of very severe heating, but it was the only way to cover the 1,200 mile cross-range the Air Force demanded for relevancy to its own missions.

Though nominally hesitant regarding the new system, as it promised to supplant several very reliable alternatives, the Air Force was prepared to enthusiastically support the Shuttle Program if its design was altered to fit its needs; indeed, the Air Force had grand plans for the Shuttle. With the assumption that the Shuttle could accomplish polar, once-around orbits, Cape Canaveral would have been an untenable launch site:

> It could not do this by firing its boosters from Cape Canaveral; geography dictated that these boosters would fly over populated territory. A launch to the north carried the hazard of impact in the Carolinas; a launch to the south would compromise security if the rocket fell on Cuba. Hence, the Air Force maintained its own space center at Vandenberg AFB, on the California coast. It offered a clear shot to the south, across thousands of miles of open ocean.[55]

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[53] Jenkins, *Space Shuttle*, 101.

[54] Heppenheimer, *Space Shuttle Decision*, 213.

[55] Ibid., 214.

The Air Force envisioned operating and maintaining its own fleet of Space Shuttles, housed, launched, landed, and processed at Vandenberg. To this end, it made extensive modifications to Space Launch Complex 6 (SLC-6 or, informally, "Slick Six"). The land on which SLC-6 was constructed was originally acquired to support Manned Orbital Laboratory launches using modified Titan boosters, "but almost as suddenly as it had started, MOL ended. When the MOL program was cancelled in 1969, the 'brick and mortar' construction at Vandenberg was about 90 percent complete."[56]

In 1972, SLC-6 was selected as the launch site for the Air Force's Shuttles,[57] and the Air Force began rapid construction of a massive support infrastructure whose scale and sophistication rivalled that of NASA's at the Kennedy Space Center. By 1986, the United States Air Force had constructed facilities for processing the orbiter, maintaining and storing external tanks, refurbishing solid rocket motors, stacking the Shuttle's components, and had greatly expanded the flame trench below the launch pad, all at great expense and engineering effort; "Construction of the Vandenberg facilities required approximately 250,000 cubic yards of concrete – that equivalent of a 25 mile, four-lane interstate – 9,000 tons of reinforcing steel, and 15,000 tons of structural steel."[58] It was apparent that the United States Air Force had fully committed to the Space Shuttle Program.

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[56] Jenkins, *Space Shuttle*, 469.

[57] Ibid.

[58] Ibid., 470-72.

Conclusions

The Space Shuttle Program was never altered forcefully from outside the agency; NASA merely succumbed to significant inter-agency pressure. T.A. Heppenheimer characterizes this devil's contract in the context of a January 1971 meeting in Williamsburg, Virginia whose purpose was to solidify design requirements for the space shuttle:

> NASA used the occasion to give the Air Force everything it wanted… One sometimes hears that when two parties are in a relationship, the one that wants it more is the weaker. NASA certainly had been pursuing support for the Shuttle with unmaidenly eagerness, and the Williamsburg rules were the result. The agency now was promising to build a bigger and heavier shuttle than it had wanted for its own uses, with considerably more thermal protection. It also was prepared to treat the Shuttle as a national asset – which meant the Air Force would not pay for its development or production and yet would receive the equivalent of exclusive use of one or more of these vehicles, entirely gratis… with the Air Force having by far the larger budget as well as greater political clout, the Williamsburg agreement resembled a treaty between a superpower and a small nation.[59]

In order to secure what it wanted, which was originally an ambitious and possibly naïve expectation divorced from social and political realities, NASA acted responsibly. It compromised with the Air Force, cooperating to develop a vehicle which satisfied its own needs, as well as those of the department of defense and commercial organizations. NASA sacrificed a significant portion of its own agency over the ultimate shape of the Space Shuttle, but did so voluntarily. Contending that NASA was now prepared to "reap its reward," Heppenheimer quotes Senate testimony by Air Force Secretary Robert Seamans in March 1971:

> The DOD supports its [the Space Shuttle] development if the results of current NASA Phase B studies and our own complementary studies show that such a system is feasible and can offer the desired performance and cost advantages over

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[59] Heppenheimer, *Space Shuttle Decision*, 233.

> current systems. Preliminary indications from these studies are that such a system can be developed. If the final study results confirm this, and we think they will, the Air Force will provide a strong recommendation that Shuttle development be authorized. When the operational system is achieved, we would expect to use it to orbit essentially all DOD payloads, "phasing out" our expandable booster inventory… The DOD investment over the next two to three years is planned to be small. However, in the future, we will require major funding to equip a DOD fleet and to provide unique DOD hardware, facilities and operational support.[60]

This endorsement was the ultimate goal of NASA's overtures to the United States Air Force, and the very reason the agency accepted tremendous expansion of the Shuttle's mission, complexity, and risk. However, it was this inter-agency support which was vital to sustained Office of Management and Budget (OMB) attempts to circumscribe the program; NASA agreed to Air Force pressure to *expand* its capabilities, considering such an imposition preferable to OMB attempts to *diminish* its capabilities. Heppenheimer's overall appraisal of the inter-agency compromises which led to the Shuttle's iconic shape and capabilities is surprisingly candid:

> Ironically, though it was a NASA project from the start, its main design features reflected pressures from outside that agency. The Air Force had pushed for the large payload capacity and the high crossrange that called for a delta wing; while NASA later accepted these features and made them its own, the initial impetus had come from the pentagon. Similarly, the solid boosters came from the OMB. Left to its own devices, NASA surely would have picked a liquid booster… In this fashion, the Air Force and OMB crafted a design that NASA would construct and operate.[61]

While this interpretation of events is certainly not *wrong* per se, it is certainly incomplete. That the Air Force-specific alterations to NASA's original vision for a manned, reusable space vehicle were ultimately pervasive and disastrous to the system's safety is not a point of contention; instead, it is vital to note that NASA deliberately solicited the

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[60] Ibid., 234.

[61] Ibid., 423.

USAF's political involvement in order to cement a future for a system that *only* NASA wanted. As Heppenheimer, himself, notes, "…while NASA needed the Air Force, the Air Force did not need NASA. That service was quite content with existing boosters such as the Titan III."[62] Throughout the design process, when a flurry of vastly divergent options were being studied, and indeed before that late stage, NASA had many opportunities to reassess its long-term commitments, particularly to a space station whose future was looking increasingly hypothetical. NASA could have settled for a smaller, cheaper version of the Shuttle (perhaps sacrificing major goals in interplanetary science and, of course, giving up on their space station). Alternatively, the agency could have developed a reusable capsule as a successor to Apollo, a contemporary analog to Soyuz; such an architecture has materialized today as the Multi-Purpose Crew Vehicle currently undergoing testing and which flew an unmanned test mission in December of 2014.[63] The interpretation that NASA was forced into radically altering the nature and capabilities of the Space Shuttle by outside sources is predicated on the counterfactual assumption that NASA had no other options and that the development of a winged, reusable vehicle was dictated to it. The truth is that NASA alone devised and promoted the concept, chose at great risk to the agency to pursue it, and engaged in its own maneuvers to avoid the attendant political hurdles to the project's completion.

In an ironic post-script to the Shuttle's tumultuous developmental history, the very vulnerabilities injected into its design by the Air Force and OMB ultimately ensured that it would never satisfy their operational requirements. It was neither safe enough nor

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[62] Ibid., 216.

[63] "'Nearly flawless': Orion passes 2-orbit test flight," *CNN*, May 14, 2015, http://www.cnn.com/2014/12/05/tech/innovation/nasa-orion-launch/.

reliable enough for military service, and was extraordinarily expensive to fly. From very early in flight testing, NASA was aware that the Shuttle Program was in serious trouble:

> On the surface, the program seemed to be progressing well. But those close to it realized that there were numerous proglems. The system was proving difficult to operate, with more maintenance required between flights than had been expected. Rather than needing the 10 working days projected in 1975 to process a returned Orbiter for its next flight, by the end of 1985 an average of 67 days elapsed before the Shuttle was ready for launch… Already, the goal of launching 50 flights per year had given way to a goal of 24 flights per year by 1989. The per-mission cost was more than $140 million, a figure that when adjusted for inflation was seven times greater than what NASA projected over a decade earlier.[64]

The Shuttle, though technologically sound, and undoubtedly a marvel of engineering, was failing to satisfy the conditions upon which its existence was predicated. Pressure to increase flight rate and lower costs led to safety oversights and regular acceptance of anomalous or unsafe vehicle behavior, a phenomenon which has come to be known as "normalization of deviation." These pressures culminated, in January 1986, in the breakup of the Shuttle *Challenger* and the loss of her crew.

The *Challenger* disaster's effects on the space program, including the eighteen-month pause in flights and massive safety evaluation, are well-documented elsewhere; what is instead pertinent here are the effects of *Challenger* on the Department of Defense's interest in the program. Though *Challenger* was a grievous personal injury to the Space Program, claiming seven lives, including one civilian, the effects on the future of the program were correspondingly momentous. In August, a mere seven months after the accident, the Space Shuttle was formally released from its commercial and military obligations. The CAIB states that, "On August 15, 1986, President Reagan announced that the Shuttle would no longer launch commercial satellites. As a result of the accident,

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[64] *CAIB*, 24.

the Department of Defense made a decision to launch all future military payloads on expendable launch vehicles, except the few remaining satellites that required the Shuttle's unique capabilities."[65] Though its cause was disastrous, this policy change effectively freed the program for purely scientific use. Throughout the 1990s, the Space Shuttle was a platform for myriad different orbital experiments, deploying highly valuable planetary science payloads such as the Hubble Space Telescope, the Galileo probe which visited Jupiter, the Ulysses mission to investigate the Sun, and the Chandra X-ray Observatory. The Shuttle Radar Topography Mission, conducted on STS-99, produced radar topography maps of a large portion of the Earth's surface. The Space Shuttle was a hugely successful platform for science and was instrumental in the construction of the International Space Station, the very purpose for which it was originally envisioned.

The Space Shuttle Program was, admittedly, subject to tremendous external pressure both financial, originating from the Office of Management and Budget, and technical, originating from the United States Air Force; however, it still stands as a powerful example of scientists and technical personnel wielding their agency through the manipulation of an understood policy apparatus. NASA committed itself to the creation of a Space Station and a Space Shuttle to service it long before military interest in the project materialized; it undertook the most audacious manned spaceflight effort in history during a time in which public and political support for the space program had dissolved. Not only did both of these programs take shape, more or less as originally intended, but they did so despite severe technical difficulty, withdrawal of Department of Defense support, and the loss of 14 astronauts. Whether the loss of life and material expense of the

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[65] Ibid., 25.

Shuttle Project and the International Space Station were worthwhile largely depends on individual views regarding the value of scientific pursuit. What has instead been argued, and hopefully proven to reasonable expectations, is that the nature (if not degree) of human utilization of near-Earth space achieved by the early 21st Century closely resembles that which was envisioned by scientists and artists during the early part of the 20th Century. NASA, and NACA before it, pursued research and operational objectives which were occasionally coincident with military interest, but not overtly for military benefit. This pursuit of knowledge culminated in a bold assertion of scientific interest in developing reusable vehicles and permanent habitation in space. Far from an enthusiastic front for military aerospace research, NASA has often stood alone, unsupported financially and politically, pursuing objectives with limited defense application; that it has realized the dreams of Tsiolkovsky, Goddard, and Clarke is nothing short of miraculous.

Chapter II: Science within the Nuclear Testing Program and the National Laboratories.

Secrecy

In stark contrast to the transparent and accessible documentary body NASA's achievements have produced, study of the United States nuclear arms development and testing activities have been impeded for decades by intense security. This is not surprising; in many cases, historical narrative is interspersed with scientific and technological achievements which may have long been surpassed, domestically, but still elude other nations or groups whose possession of such information the United States considers to be dangerous or otherwise undesirable. This jealous protection of "trade secrets" dates back to one of the most massive intelligence failures in United States History. During the height of the Manhattan Project, "from 1942 to 1949, [Manhattan Project physicist] Klaus Fuchs passed along secret information to the Soviet Union"[1] pertaining to the theory and design of nuclear weapons. This rapidly accelerated the Soviet program, which exploded its first device in August of 1949, a mere four years after the first atomic explosion in Alamogordo, New Mexico – far earlier than US intelligence agencies had expected.

Despite a redoubling of internal security, American advances in higher yields and smaller weapon size were mirrored, after nominal intervals, by their Soviet adversaries. *Ivy Mike*, the first proof of the radical Teller-Ulam radiation implosion principle, was detonated in the Marshall Islands in 1952. Teller-Ulam radiation implosion, named for physicists Edward Teller and Stanislaw Ulam, was a major breakthrough in thermonuclear explosive design. In a Teller-Ulam configuration, a standard, explosively

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[1] Rhodes, *Making of the Atomic Bomb*, 770.

compressed nuclear explosive (primary) is placed at the head of one or more cylindrical containers of fusion fuel, referred to as secondaries. These components are placed inside a heavy casing. When the primary detonates, powerful electromagnetic radiation (primarily X-rays) leaves the primary, travels down the length of the casing toward the secondaries (in some cases reflecting off of the inside of the casing) and superheats them. This causes their outer layers to ablate – essentially, to burn off – and the resultant opposing force compresses them, establishing the requisite conditions for fusion to occur. This explanation is highly abridged, but Rhodes[2] and Hansen[3] both present superb illustrations which fully convey the principle. As a key theoretical component to hydrogen-weapons, the Teller-Ulam principle was jealously guarded; despite this security, the Soviet Union exploded a device based upon Teller-Ulam radiation implosion in 1955. As the United States continued to design increasingly sophisticated weapons while aggressively promoting non-proliferation, the very concept suffered dramatic and regular defeats. In the decades after the first Soviet nuclear explosion, Joe-1, the United Kingdom, France, China, India, Pakistan, and North Korea all developed and tested nuclear weapons.[4]

The failure of nuclear states to control information pertaining to nuclear weapons did little to deter continued attempts to do so; this made study of the topic incredibly difficult until the waning years of the Cold War. Highly technical histories such as

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[2] Rhodes, *Dark Sun*, 506.

[3] Hansen, *U.S. Nuclear Weapons*, 22.

[4] Israel is widely suspected to possess approximately 200 nuclear weapons. South Africa developed a small number of nuclear weapons for use as a regional deterrent, but fully decommissioned its nuclear weapons and is now a signatory of the Treaty on the Non-Proliferation of Nuclear Weapons.

Glasstone and Dolan's *The Effects of Nuclear Weapons*,[5] first published in 1950 as *The Effects of Atomic Weapons*, constituted more scientific exposition than history and that work has only become as encyclopedic as it has through successive editions which have each benefitted from declassified information. Another notorious episode involved independent researcher Chuck Hansen's attempt to research nuclear bomb design and testing details. University of Maryland Head of Engineering and Physical Sciences Library Herbert Foerstel first describes the publication of an article which first appeared in *The Progressive* magazine in 1979,[6] written by Howard Morland, a freelance contributor:

> …the technical information in Morland's article, describing the nature, composition, and reaction processes of the hydrogen bomb, was derived from public sources like Dr. Edward Teller's 1975 article in the *Encyclopedia Americana*… Morland researched his article by reading encyclopedia and magazine articles, textbooks, and unclassified government reports.[7]

The government responded by instigating litigation against *The Progressive.* The publication successfully defended itself, but the government's actions mobilized Hansen:

> Hansen had written repeatedly to DOE officials challenging their suppression of Morland's article, and he infuriated them during the Spring of 1979 when he organized a nationwide 'H-bomb Design Contest.' The first entry that DOE felt compelled to classify would be declared the winner… Hansen wrote a lengthy letter to Senator Charles Percy (R-IL) in which he summarized the technical data from Morland's article and other sources. Hansen mailed copies of the letter to several newspapers and the DOE… On September 16, Hansen's letter to Senator Percy was published in a Madison, Wisconsin, newspaper. The next day, the Justice Department announced that it would seek dismissal of its case against *The Progressive*…[8]

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[5] Semuel Glasstone and Philip Dolan, *The Effects of Nuclear Weapons* (No location given: United States Department of Defense and United States Department of Energy, 1977).

[6] Morland, "The H-Bomb Secret," 3-12.

[7] Herbert Foerstel, *Secret Science: Federal Control of American Science and Technology* (Westport, Connecticut: Praeger, 1993), 82-83.

[8] Ibid., 88-89.

Hansen effectively demonstrated that the protection of information deemed "sensitive" was nearly impossible and that hopes for non-proliferation lay not in the control of knowledge, but instead in reliance upon the staggering industrial difficulty of creating nuclear weapons. Hansen eventually compiled one of the most illuminating documentary accounts of nuclear weapons and testing ever printed, *U.S. Nuclear Weapons: The Secret History.* Hansen's book draws on declassified documents, providing unprecedented detail of nuclear weapon design including technical drawings, schematics, and intricate diagrams. The author traced weapons' physical configurations and design specifications throughout history to modern systems; *U.S. Nuclear Weapons* is a pioneering technical history of the nuclear bomb.

One of the first major historical narratives of the arms race, and one which won the Pulitzer Prize, was Richard Rhodes' 1989 *The Making of the Atomic Bomb*. Rhodes' book was a comprehensive and foundational work which richly addressed both the personal stories of the men who created the bomb as well as the technological advances for which they were responsible. In three further monographs, *Dark Sun: The Making of the Hydrogen Bomb*, *Arsenals of Folly*,[9] and *Twilight of the Bomb*,[10] Rhodes describes the development of Edward Teller's hydrogen "Super" bomb and political efforts to eliminate nuclear stockpiles. Despite its status as a major milestone in the study of nuclear history, Rhodes' series is not without its shortcomings; each tells history "from the top," focusing on the perspectives and accomplishments of major scientific or

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[9] Richard Rhodes, *Arsenals of Folly: The Making of the Nuclear Arms Race* (New York: Vintage Books, 2007).

[10] Richard Rhodes, *The Twilight of the Bombs: Recent Challenges, New Dangers, and the Prospects for a World Without Nuclear Weapons* (New York: Random House, 2010).

political figures such as Ernest Lawrence, Edward Teller, Igor Kurchatov, Lavrenti Beria, Ronald Reagan, and Mikhail Gorbachev. While there is nothing wrong with this approach, *per se*, Rhodes only superficially addresses (with the exception of *Dark Sun*) the scientific programs which were deeply embedded in the nuclear arms design and testing processes, as well as the personnel involved in those peripheral initiatives.

Unsurprisingly, the overwhelming bias in the historiography of the nuclear age is toward arms and arms production, while one of the major components of that era, scientific investigation and the cutting-edge research institutions which underpinned the aforementioned effort, has been largely ignored. One attempt to shed light upon and humanize the researchers behind "big science," University of Maryland University College professor of Humanities and Philosophy Debra Rosenthal's *At the Heart of the Bomb: The Dangerous Allure of Weapons Work*, falls short of its implied objective. The book, released in 1990 and, "…based on over 260 hours of interviews with eighty-five people who work or have worked in the two nuclear weapons design facilities in New Mexico," ostensibly aims to expand the agency of scientists, but repeatedly reduces research institutions to mere "bomb factories" and victimizes researchers.[11] Rosenthal's hyperbolic, inaccurate, and overly-editorial agenda is clearly articulated on the fifth page:

> But what motivates contemporary nuclear weapons designers?... Are the scientists and engineers expanding and enhancing our nuclear arsenal today the last cold warriors, standing on the "shoulders of giants," the ultimate true believers? Or, like Paul, are they simply self-absorbed tinkerers, content so long as they are allowed to play with their sophisticated but deadly toys?
>
> I have always thought of scientists as representing, however awkwardly, the height of Enlightenment values. They use the scientific method to dispel superstition. They do not fear creativity and can challenge even the most entrenched orthodoxy. They think logically, and they have an unrelenting passion

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[11] Debra Rosenthal, *At the Heart of the Bomb* (New York: Addison-Wesley Publishing Company, Inc., 1990), ix.

> for truth. Their knowledge allows them to manipulate and control the capricious forces of nature, thus making us all less vulnerable. So why, I wondered, did Paul sound more like a victim than a master?[12]

Rosenthal's characterization of scientific personnel is simplistic and patronizing. She consistently exhibits a misunderstanding of the interconnected yet simultaneously discrete nature of scientific work, reducing all activity at Los Alamos National Laboratory and Sandia National Laboratory to the term "weapons work." It is laudable that Rosenthal saw fit to examine the National Laboratories, but regrettable that she chose to wrestle her interviewees' testimony into a contrived exposé into the morality of scientists *vis-à-vis* one small section of their expansive work. Rosenthal's work is an implicit endorsement of Gillispie's original charge that scientists are political non-entities – or perhaps more insultingly, that they hold political and moral views, but lack the constitution to act in accordance with them.

Rosenthal's disservice to laboratory personnel was rectified in 2003 by Peter J. Westwick, "a Senior Research Fellow in Humanities at the California Institute of Technology"[13] in his book *The National Labs: Science in an American System, 1947-1974*. Westwick's work is a fantastically in-depth, objective analysis of the origins, purpose, method, and structure of the National Laboratories drawing on a wealth of secondary works, government reports, and personal correspondences, both recorded and written. Westwick is a renowned scholar of the history of science with several other works pertaining to aerospace and defense science; *The National Labs* is vital to this paper in providing mileposts in the proceeding section.

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[12] Rosenthal, *Heart of the Bomb*, 5.

[13] Peter J. Westwick, *The National Labs: Science in an American System, 1947-1974* (Cambridge, Massachusetts: Harvard University Press, 2003), inside cover.

National Laboratories

Much like NACA, the National Laboratories came into being as the result of a will to consolidate, collate, and direct research. Prior to their existence, research was conducted on an individual basis and in a highly informal manner at academic institutions dispersed throughout the country. Though Los Alamos, the first of these national laboratories, was organized and assembled in a crash effort to influence American military progress in World War II, Westwick contends that it is indicative of a larger, preexisting social phenomenon:

> The emergence of the national labs expresses a central theme in American history. Industrialization in the late nineteenth and early twentieth centuries spurred the reconstitution of American society, from small-scale, informal, local groups to large-scale, formal, bureaucratic organizations… The national lab system brought these same characteristics to a large segment of American science.[14]

Not only was this restructuring not an unforeseen or alien concept, but it was also not one which was unilaterally imposed upon academics by General Leslie Groves or the United States government. The initiative held the full endorsement of the scientific director of the Manhattan Project, Robert Oppenheimer, who saw it as a way to eliminate many of the communicative difficulties posed by a distributed research network:

> …I became convinced, as did others, that a major change was called for in the work on the bomb itself. We needed a central laboratory devoted wholly to this purpose, where people could talk freely with each other, where theoretical ideas and experimental findings could affect each other, where the waste and frustration and error of the many compartmentalized experimental studies could be

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[14] Westwick, *National Labs*, 6.

> eliminated, where we could begin to come to grips with chemical, metallurgical, engineering, and ordnance problems that had so far received no consideration.[15]

Of course, the nature of the research conducted at Los Alamos (and at many other laboratories and academic institutions) from 1943 to 1945 was overwhelmingly military-oriented, directed specifically toward designing and then weaponizing a nuclear explosive. With the end of the war, however, a relationship began to take shape which forms a major part of the narrative of this paper. In the case of aerospace research, civilian agencies progressed the state of the art while defense-related groups opportunistically invested and attempted to alter or adapt the architecture for their own purposes. With regards to nuclear science and technology, the facilities, personnel, and other infrastructure were developed with defense as a primary motivation, but increasingly came to be appropriated for fundamental science work. Not only was this non-defense related work tolerated, but it came to be accepted as a fundamental part of a reimagined, government-sponsored national scientific community. Los Alamos was the prototype for this arrangement, as well as one of the most distinctive and productive of the national labs.

Postwar Crisis and Reorganization

As previously stated, Los Alamos' work during World War II was centered around designing a nuclear bomb, but almost immediately after the war, it became obvious that the entire community faced an inevitable transformation. The strict,

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[15] "In the Matter of J. Robert Oppenheimer," *United States Atomic Energy Commission Personnel Security Board*, United States Atomic Energy Commission, April 12, 1954. Vol. 1, 32-33. Hosted by U.S. Department of Energy OpenNet, https://www.osti.gov/opennet/hearing.jsp.

centralized wartime conditions were anathema to the civilian scientific staff, which tolerated the hardship in order to fulfill their moral obligation of protecting the free world from Axis aggression. With the conclusion of the war, it proved nearly impossible to retain scientific staff:

> Here was Los Alamos in September, 1945. The senior civilian scientists, weary of living under wartime conditions, under wartime security, on a wartime Army post, and under conditions of wartime urgency, thought longingly of their academic laboratories and classrooms. The more junior civilians thought of the academic degrees they did not have and the further education they ought to have…[16]

The geographical isolation of Los Alamos, a primary component of wartime security, further compounded threats to the institution's sustainability after the war. Westwick notes, "The lack of long-term certainty over the future of the labs encouraged the flight of the scientific staff. Clinton and Los Alamos suffered from their distance from academic and metropolitan centers. Of the three thousand employees at Los Alamos at the end of July, only a third remained by the end of the year."[17]

Much like NASA in the wake of its Apollo Program success, the nascent national laboratories could not survive without an immediate and dramatic expansion of their duties. Because they were staffed by civilian scientific personnel, their long-term survival could not be guaranteed though pursuit of military technology alone. Space Shuttle's identity and role was altered to garner political support; the national laboratories shifted away from their original mission toward fundamental scientific advancement to court civilian interest. The change was informally enacted very shortly after the war:

> When the Advisory Committee for Research and Development met on 8-9 March 1946, it took the national name and applied it to the regional concept. National

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[16] Rhodes, *Making of the Atomic Bomb*, 755. Rhodes cites Norris E. Bradbury, Peace and the atomic bomb, *Pomona College Bulletin*, Feb. 1949.

[17] Westwick, *National Labs*, 35.

> laboratories, it suggested, should provide equipment too expensive for universities or private labs; the national labs would pursue programs of unclassified basic research guided by a board of directors selected from participating universities and other institutions.[18]

A mere three years after its inception as a vessel for centralizing scientific effort for military benefit, Los Alamos became the first in a network of distinct, individual research campuses across the country involved in a breadth of work spanning nearly every scientific discipline. Westwick notes that, "The history of the labs demonstrates that this process of adaptation began in the earliest days of the system, and that the structure of the system thenceforth encouraged it."[19] Ensuring their own future meant Los Alamos and the other national laboratories would never be strictly military-oriented research facilities, and their work would never be solely defense-related.

Laboratory Atmosphere

Aside from its primacy, Los Alamos was unique in other ways: the laboratory, and eventually the community which coalesced around it developed an informal, collegial atmosphere which emulated the environment in which the civilian staff felt most comfortable and found themselves most productive. Considering the intolerance exhibited by Los Alamos staff to a more isolated, rigid military atmosphere following World War II, this was likely either a conscious effort by planners or a result of subtle, prolonged pressure from scientists. Rosenthal's vivid description of Los Alamos instills a pedestrian impression of the location which completely belies the nature of its work:

> Convoys of joggers hug the roadsides at lunchtime. But there are no gleaming white towers, no inspiring statues of the great physicists, no American version of

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[18] Ibid., 39.

[19] Ibid., 299.

> the Arc de Triomphe. There is no visible sign of the bomb at all… Visitors and employees compare the Los Alamos Laboratory complex to a huge college campus without students… No one cares if you ride around and indulge your curiosity. Visitors are free to ogle the lab architecture… to sit under the pines near the administration building…[20]

The very demeanor of the community was carefully engineered to attract civilian scientists; however, the concessions went far deeper than mere aesthetics. As the Cold War deepened, the mission and nature of the work undertaken at the national laboratories shifted paradoxically away from military activities in order to attract and retain leading scientific personnel. Researchers were granted an oft-overlooked but critical degree of freedom in the scientific questions they pursued. Much of this pressure to enact this change came from within the organizations:

> The persistent agitation of the labs for basic research helped to overcome opposition to it in the AEC and GAC… Argonne, Los Alamos, and Oak Ridge claimed that they needed basic research 'to attract and hold research personnel of the highest qualifications,' since 'first-class men will not remain on the laboratory staff unless they have reasonable freedom to carry on research in fields of their own choosing.'[21]

So it came to be that this autonomy formed a central tenet of the laboratory system. Personnel were actively sought from civilian universities to staff the national laboratories. They contributed a broad range of individual talents and specialties and were free, within reason and as budgetary realities allowed, to pursue their own objectives and occasionally lend their help to specific security-related projects.

Among the many itinerant researchers who contributed to the research effort at the national laboratories, was Dr. Raymond Fawcett of Farmville, Virginia. Dr. Fawcett, a former Professor of Physics at Longwood College, holds the distinction of having

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[20] Rosenthal, *Heart of the Bomb*, 28-29.
[21] Westwick, *National Labs*, 154.

conducted research activities at two separate national laboratories, Argonne National Laboratory (AGL) and Los Alamos National Laboratory (LANL). Recruited by LANL staff after a presentation of his research at the International Conference on Nuclear Cross Sections for Technology in Knoxville, Tennessee in October of 1979, Dr. Fawcett subsequently spent "thirteen consecutive summers, first in 1981 and the last in 1993"[22] doing science at LANL. His insights into the national laboratories are invaluable.

Speaking in person with the author, Dr. Fawcett clearly reinforced and elaborated upon the Los Alamos National Laboratory's unique place within the national laboratory network, a status which is hinted at by Rosenthal and Westwick. Argonne National Laboratory was apparently far more mission- and specifically task-oriented than LANL, with Dr. Fawcett remarking that, "in the division where I worked… there was some pressure for production. A meeting was held every week where the division leader addressed each of the physicists in our division… and he would point to each one of us – 'what are you doing this week and what progress are you making?'"[23] Despite the administrative pressure, Dr. Fawcett did not believe it impacted the quality of the Laboratory's work: "Certainly, I did not see it in the three months I was there, but there was more of a competitive spirit…"[24]

In stark contrast to the strict management of time and research direction at Argonne National Laboratory, LANL granted an unprecedented degree of latitude to its scientific personnel. This freedom seems to have had a positive impact on morale, as well, judging from the sentimental tone of Dr. Fawcett's recollection:

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[22] Personal interview with Dr. L. Raymond Fawcett 27-28 October, 2014.
[23] Ibid.
[24] Ibid.

> At Los Alamos, the atmosphere was more relaxed. It was as if you were working at your own rate, and I certainly was, without the pressure of hurrying to get the work done. It was, I guess one would say a more laid-back atmosphere and there was great respect among the physicists for one-another… I didn't sense any competitive feeling. It was, I thought, a more respectful feeling of fellow staff members, because there wasn't a pressure that I observed to hurry to get things done.[25]

The difference was not one of professionalism, he was sure to point out, but merely one of attitude: "…the physicists at both places were highly knowledgeable and accomplished; personalities were involved that were different by my likes at the two laboratories."[26] The contrasting characterizations of Argonne and Los Alamos once again clearly highlight scientists' preference to determine the direction and cadence of their work, as well as their disdain for micromanagement; that LANL vigorously embraced those characteristics does much to explain its success as a research institution.

From Dr. Fawcett's testimony, the extent of the scientific personnel's autonomy at Los Alamos is startling and strongly substantiates the argument posited – that scientists were given enormous responsibility and freedom in determining research direction. He describes the title of Laboratory Fellow, a status reserved for the most long-standing, valuable members of the scientific community:

> Persons, physicists that I respected greatly, who had been at the laboratory, Los Alamos, for a number of years, who were not interested in becoming administrators, they were given the title 'Laboratory Fellow' and allowed to work on anything they wanted to. There may have been pet projects that they had had in mind since long before they came to the laboratory and now they had the time and the freedom to work on those projects all they wanted.[27]

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[25] Ibid.

[26] Ibid.

[27] Ibid.

This status may have been reserved for certain tenured personnel, but it illustrates that Los Alamos was interested in diversification of research and placed a high priority of the individual initiative of its staff in shaping its overall body of work.

Of course, the national laboratories could only afford to extend so much latitude to their scientists; like any government-funded institution, account had to be made of the money expended on research and that it had been invested responsibly. This, however, is another area in which Los Alamos exhibits a startling informality, and in which its rank-and-file researchers exerted an enormous individual influence. Los Alamos' funding seems to have been highly discretionary, as Dr. Fawcett explains, "…as far as I know, while I was not involved in any way with budgets, the laboratory funding was… predominantly for the purpose of weapons work. However, it certainly, as best I could tell, in no way [was] required to be used totally for weapons work."[28] Funding was apparently regularly manipulated or redistributed to support the breadth of work in which the laboratory engaged. This practice seems to have been tacitly endorsed by management and only occasionally attracted mild objection:

> …a million dollars taken for a pure physics project would be a million dollars not available for weapons work. So, while most of the division leaders of the several divisions at Los Alamos were in support of spending some money that was not used for weapons development and weapons work, one or two of them, for the experiment [in which] I became involved, expressed some reluctance to spend the amount of money that was going to be required… that's all – just simply expressed reluctance. So, there was no hesitation by the experimenters that were involved to spend what was necessary to get this particular experiment done.[29]

The preceding paragraph is remarkable considering the scale of funding entrusted to the national laboratories – that scientists had so much influence over its eventual use is

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[28] Ibid.

[29] Ibid.

unequivocal evidence of the relationship between the government and its national laboratories: the former advanced, essentially, massive research grants in the form of yearly budgets. A portion of each budget was specifically intended for weapons concepts and the rest was an accepted investment in fundamental scientific research. This investment, it was recognized, promoted the nation's scientific and technological base by keeping in top intellectual form scholars who eventually dispersed back into academia. A requisite compromise demanded of the laboratories in this symbiotic relationships was granting scientists the freedom they needed to be productive.

Science Within the Nuclear Testing Program

One of the least visible but most powerful manifestations of researchers' priorities was through a practice Dr. Fawcett refers to as a "hang-on" experiment – an experiment envisioned to take advantage of the unique and extreme conditions produced by a nuclear explosive. Intended to be conducted alongside a nuclear detonation, "hang-on" experiments have a long history, dating back to the very first series of post-war nuclear tests – Operation *Crossroads*. "Hang-on" experiments explore a variety of scientific interests, some defense-oriented, some purely scientific, and others an indistinguishable combination of the two.

Operation *Crossroads*, "…was the first [nuclear testing series], consisting of an air and underwater detonation at Bikini atoll using an array of target ships…"[30] and saw the fourth and fifth nuclear detonations in human history. The weapons employed were, essentially, copies of Fat Man, the implosion-based, plutonium bomb dropped on

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[30] Hansen, *U.S. Nuclear Weapons*, 50.

Nagasaki in August of the preceding year. Hansen notes that, "*Crossroads* was the first large-scale weapons effects test…,"[31] an early iteration of "hang-on" experiments which grew steadily in frequency throughout the United States' nuclear testing program. This first attempt set a clear precedent of scale, involving, "…42,000 men, 156 airplanes, and 242 ships."[32] In addition to measuring the impact of blast effects and radiation on military hardware, the test involved a biological experiment – "On the decks of the ships were cages containing goats, sheep, pigs, and rats."[33] The question of radiation's effects on biological organisms stemmed from accounts of severe radiation injuries in the wake of the Hiroshima and Nagasaki bombings, but despite its origin, curiosity regarding the topic held far-reaching, non-military significance. In that sense, the extensive range of experimentation conducted in parallel with Operation *Crossroads* was a synthesis of defense and fundamental scientific interest. *Crossroads*' scientific bounty lay primarily in the insights it provided into how disastrously harmful radiation is toward biological organisms; its military benefit was that it illustrated the ruinous effects of radiation on machinery. The flotilla anchored in Bikini Atoll was rendered incredibly radioactive, and the fallout was exceedingly difficult to wash off of ships as it seeped into every crevice and infiltrated wooden decks.

An enormous number of nuclear tests were conducted in the continental United States from 1951 until 1962; many of them involved military exercises to assess the ability of a fighting force to operate in the immediate aftermath of a nuclear engagement or to analyze the psychological effects imposed on soldiers by witnessing a nuclear

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[31] Ibid.

[32] Welsome, *Plutonium Files*, 171.

[33] Ibid.

exchange. Pulitzer Prize-winning journalist Eileen Welsome elaborates on these activities:

> Radiation experiments on soldiers began in 1951, the year atomic bomb tests began in Nevada. They continued until 1962, when above-ground tests were halted. Military troops were used in psychological tests, decontamination experiments, flashblindness studies, research involving flights through radioactive clouds, and studies aimed at measuring radiosotopes in their body fluids.[34]

In these military biological and material testing programs, a disturbing generalization becomes apparent: military regard for human and non-human health and safety were not significant concerns compared to the desire to collect data. This observation is one major distinguishing factor which differentiated later, smaller scale "hang-on" experimentation modules designed and implemented by the national laboratories.

During the early 1950s, as development of the hydrogen "Super"[35] bomb began to gain serious momentum, pure scientific experiments which did not result in harm to living beings were undertaken alongside nuclear tests. Experimenters grew increasingly curious as to the nature of fusion and the bizarre conditions inside hydrogen weapons at the moment of detonation. To better understand this phenomenon, they continued to construct increasingly elaborate experimental setups in conjunction with thermonuclear tests. The first example of this was during Operation Ivy, shot *Mike*,[36] which took place in the Bikini Atoll in November 1952. *Mike* was the first full-scale test of the Teller-

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[34] Ibid., 9.

[35] "Super" was a term frequently used in internal references to a hypothetical fusion weapon because its predicted explosive yield was to be orders of magnitude greater than existing atomic weapons.

[36] Nuclear tests were designated with names corresponding to the order in which they took place, e.g. *Able* and *Baker* for the first and second explosions in a group. These individual tests, known as shots, were grouped into named series. So, just like NASA's nomenclature of payload-booster combinations (*Mercury-Redstone, Gemini-Titan II*), nuclear tests were referred to by their test series' name followed by their individual name, hence *Crossroads Baker, Ivy Mike,* and *Castle Bravo*.

Ulam radiation implosion principle and was the first true thermonuclear detonation in history. *Mike* also stood out for the extensive degree to which it was instrumented:

> The islet was ssurrounded by diagnostic and test instruments and experients radiating from the *Mike* shelter. Most notable of these was an 8 ft. square, 9,000 ft. long aluminum-sheathed plywood shed running from Elugelab to the adjacent islet Bogon. The shed enclosed a number of helium-filled plastic ballonets (to minimize atmospheric absorption) used to direct gamma rays and neutrons from the exploding test device into various detectors and counters, whose data would be telemetered to other stations.[37]

The common misconception of Cold War nuclear testing is that all activities were intended to prove and refine weapon designs, and while certainly much of this occurred, many tests existed primarily to prove a scientific principle that it was hoped could be weaponized – *Mike* fell firmly into that category. Of course, it had been suspected for over a decade by military and scientific minds that fusion could be harnessed and employed in the form of a bomb, but *Mike* was not a weapon test; it was a science experiment of truly extraordinary scale. As part of the *Mike* shot, "Over 500 scientific stations were constructed on 30 separate islands in addition to reef locations on Eniwetok atoll."[38] Of course, these scientific observation posts and devices yielded information which could be used to construct a bomb, but that is the nature of basic research: as more of the physical world is understood, it can better be manipulated for belligerent ends. What *Mike* elucidated on a most fundamental level was that humans could instigate a large-scale fusion reaction in cryogenically-stored deuterium fuel.

While *Mike* was arguably a fundamental physics experiment, the 1954 test series Operation *Castle* was undoubtedly intended to investigate methods to weaponize the

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[37] Hansen, *U.S. Nuclear Weapons*, 56.

[38] Ibid., 57.

principles observed 16 months earlier. The original cryogenic configuration in which *Mike* was detonated was far too large and cumbersome to have been dropped from an airplane or packaged into any other conceivable delivery system. Hansen explains the infeasibility of directly weaponizing *Mike:*

> The *Mike* device was erected in a large, hangar-like corrugated aluminum-covered shelter 88 ft. wide, 46 ft. deep, and 61 ft. high, topped with a 300 ft. radio and television control signal tower… The *Mike* device was essentially a large thermos bottle, standing 20 ft. high and measuring 6 ft. in in diameter. The device… weighed between 62 and 65 tons.[39]

Clearly, something was going to have to be done to reduce the size and weight of the fusion experiment for it to have *any* military application. The answer, which was to be proven during *Castle*, was solid fuel: "The Teller-Ulam principles had been demonstrated in a liquid-fueled device; the next step… was to determine the characteristics of radiation implosion in large-scale solid-fueled devices."[40]

Just as in the *Ivy* series, Operation *Castle* saw extensive construction of scientific facilities to monitor the tests. Despite having proven the concept of radiation implosion, experimenters had questions regarding new explosive configurations and the vastly different materials utilized within them. This construction effort dwarfed that which was undertaken for *Ivy Mike*:

> For the *Bravo* shot, a twelve-pipe, 7,500 ft. array was erected running from the shot cab to a station on Namu; for *Koon*, a two-pipe array, 5,600 ft. long, and for shot *Echo* at Eniwetok, a two-pipe array, 2,700 ft. long, was constructed from the shot cab on Eberiru island to recording stations at Aomon island… An array of twelve mirror towers were also built in an arc on the artificial one-acre shot island created for the *Bravo* test. These mirrors would reflect early bomb explosion light to a series of remote high-speed cameras several miles away.[41]

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[39] Ibid., 56.

[40] Ibid., 61.

[41] Ibid., 64.

Despite all the preparation Operation *Castle* entailed, an oversight by theoretical personnel at Los Alamos[42] led to it yielding approximately three times its expected explosive potential. This resulted in a radiological catastrophe for the people of the Marshall Islands and an embarrassing political episode due to the presence of a Japanese fishing trawler in the fallout zone. Scientific staff played an enormous role in Cold War nuclear testing, opportunistically employing elaborate and often large-scale experimental devices to investigate the exotic conditions within nuclear detonations. Their successes often went unnoticed, quietly expanding human knowledge of the physical world or contributing to future weapon and test configurations; as proven by *Castle Bravo*, however, their mistakes could have global environmental and political ramifications.

“Hang-On” Experiments: Mighty Oak and EMZY

In most cases, “hang-on” tests were of a much smaller scale than the primary experimental packages seen in cases like *Ivy Mike* and *Castle Bravo*. There were often multiple, modest experiments arranged around each explosive, not unlike the way NASA carried many small-scale experimental payloads on each Shuttle flight. Dr. Fawcett happened to have participated indirectly in one such “hang-on” test, called EMZY. Operation *Charioteer* was a series of nuclear detonations conducted at the Nevada Test Site from 1985 to 1986[43] and one of the explosions, *Mighty Oak*, provided an opportunity

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[42] Rhodes writes in *Dark Sun*: “It was expected to yield about five megatons, but the group at Los Alamos that had measured lithium fusion cross sections had used a technique that missed an important fusion reaction in lithium7, the other 60 percent of the Shrimp lithium fuel component.” 541

[43] “United States Nuclear Tests July 1945 through September 1992.” U.S. Department of Energy Nevada Operations Office, http://www.nv.doe.gov/library/publications/historical/DOENV_209_REV15.pdf, 104.

for LANL researchers to pursue a question of fundamental scientific significance – what is the size of a neutron?:

> Technically speaking, it was to measure the S-wave scattering length, which, in common terms, is the cross-section of the neutron. And why is that the size? Well, think of taking an orange, chopping it in half, and looking at the half. What are you looking at? The cross-section. So, if you know the cross-section and you assume spherical in design, then you know the diameter.[44]

Despite the simplicity of the question, determining the S-wave scattering length of a neutron required elaborate test articles as well as extensive excavation.

Like all nuclear tests following the cessation of atmospheric testing, *Mighty Oak* was to be exploded in an enormous underground tunnel complex. This tunnel was intended to both contain radioactive material and permit study of the blast effects, housing multiple arrays of experimental setups. Within the cavern housing *Mighty Oak,* Dr. Fawcett claims "…there is a good deal of space available to do ancillary experiments that are dependent on that nuclear explosion."[45] In fact, *Mighty Oak* was apparently host to a multitude of experiments, some intended to determine the effects of radiation on electronics, as Dr. Fawcett observed, "at the far end of the tunnel, racks of electronics equipment were set up to be exposed to the X- and gamma radiation that left the site of the explosion at the speed of light…"[46]

As for EMZY, the test apparatus which was to determine the size of a neutron, its design was strange but ingenious. Dr. Fawcett described a pair of "bowlers" which bracketed the nuclear explosive, each "…just a glass sphere filled with compressed tritium and deuterium." The necessity for placing them in proximity to a nuclear device

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[44] Personal interview with Dr. Raymond Fawcett 27-28 October, 2014.
[45] Ibid.
[46] Ibid.

lay in the fact that nuclear explosions generate an enormous amount of neutrons, far more than can be produced inside commercial or experimental reactors – it was these neutrons which were key to activating the "bowlers." When *Mighty Oak* detonated, radiation caused fusion reactions in the two "bowlers" which, in turn, released their own neutrons which collided in a carefully predetermined manner, eventually scattering to detectors placed around the experiment.

Unfortunately, EMZY failed to fulfill its objective due to a minor technical failure which had enormous consequences for the scientific personnel involved. Blast doors which were designed to shut immediately after detonation malfunctioned:

> …we wanted only neutrons, so in order to stop the debris, which would follow the neutrons out more slowly – by debris I am including alpha and beta and vision fragments – …you had to get these blast doors closed immediately after the bowlers went off… The blast doors didn't close in time and all of our recording instrumentation downstream in the tunnel… the debris got down there, exposed all the film, and ruined the recording devices…[47]

The failure of the EMZY experiment was costly from both a budgetary and materiel standpoint, but its loss also carried with it more severe yet less tangible consequences. The scientists at Los Alamos who had hoped to examine a physical principle nobody had yet uncovered, who had acted on their own initiative to bring the investigation to fruition, were devastated:

> …it was a disappointment for all of us. You see, we may have made the measurement. That is, we may have gotten the data that, when analyzed, produced the results that we were looking for, but because the alcove with the recording equipment was not sufficiently protected, it was all lost. We really expected the blast doors to close. They had been closing in time for other experiments in tunnel shots over the years, but they failed for us.[48]

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[47] Ibid.

[48] Ibid.

Furthermore, EMZY was a clear articulation of purely scientific curiosity, of the agency scientists exerted to pursue concepts which fascinated them. Dr. Fawcett clarified the greater physical meaning of EMZY, remarking:

> …it was new knowledge. To learn the size of the neutron would be something that people, over the years, have wanted to knmow simply because neutrons exist and we want to know more about them… It's just basic knowledge and man just wants to know.[49]

EMZY may have been a bitterly disappointing missed opportunity for the research staff involved in planning and preparing it, but it was in many ways a decisive victory, as were the other myriad such events which have not yet entered the historical discourse of the nuclear age. EMZY was a tangible representation of the agency of scientists within a system originally established to support defense. Westwick argues that the considerable freedom extended to researchers was, from a very early stage, vital to the laboratories' diversity and potency:

> To attract and retain top scientists, the labs adopted the scientists-on-tap model, which, for example, allowed Frederick Reines and Clyde Cowan at Los Alamos to take a break from weapons work for basic research that would detect the neutrino and win a Nobel Prize… Lab scientists could also justify their research as a contribution to the storehouse of knowledge, to the apprenticeship of visiting scientists, and to the training of a new generation of scientists.[50]

This organizational model facilitated the recruitment of Dr. Fawcett, who returned to Longwood after his summers at Los Alamos having benefitted from pursuing physics questions alongside some of the most brilliant minds from across the country.

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[49] Ibid.

[50] Westwick, *National Labs*, 301.

Answering Gillispie

The triumph of scientists and basic human inquisitiveness within a system and during an era dominated by questions of military equivalency and competition is one of the most significant untold stories of the post-Cold War narrative. Westwick's conclusion to *National Labs* contains a section which bears great significance to this thesis as it explicitly emphasizes the agency of scientists within the national laboratory structure:

> A central question for historians of postwar American science, and of physics in particular, concerns the alignment of scientific research with the goals of national security. Paul Forman has advanced the thesis that American physicists in the Cold War became captives of military patronage… The work of the national labs for national security has led some historians, including Forman, to categorize them as military institutions. Lab scientists did devote much effort to national security programs and proved willing to orient their programs even more toward military needs in times of emergency. But they also provide evidence for the other side of the argument: that postware physicists found 'intellectually compelling areas of inquiry' whether relevant or not to national security… …detailed examination of the national labs demonstrates that, despite their extensive work for national security, lab scientists proved adept at attaining scientific independence… Lab scientists did not merely trim their sails to prevailing social and political winds; they could influence the wind direction.[51]

The preceding paragraph is a powerful assertion of scientists' agency and one which this paper fully endorses. Scientists found myriad ways to express their vivid individuality and creativity in the programs in which they participated. 'Big science' was not a governmental call-to-arms as it has far too often been portrayed. Rather, the research investments made by America in the last half of the 20th century were a broad and conscious investment, an acknowledgement of the nation's heritage of strength through diversity. Of course, military relevancy had to be nominally observed – in NASA's case, this was in order to secure funding, and in nuclear research, it was to preserve the latitude

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[51] Ibid., 300-01.

that was already granted to scientific personnel. There was an implicit understanding in both fields that defense was important – Cold War anxiety was never lost, especially on such an intelligent and politically-informed group – but researchers were simultaneously aware of an important axiom: knowledge has no fundamental morality.

Knowledge can be used for widely morally-accepted, constructive endeavors or for heinous acts of violence and inhumanity. The likelihood of any given discovery being applied to one category over the other depends very much upon the nature of the people who choose to apply it – there is no innate, assumed moral character in any physical law or principle. Knowledge of biology can be utilized to cure virulent, harmful diseases or to create and promulgate them; rocketry can aid in peering further into the galaxy or it can deliver guided explosives; nuclear fusion promises to solve the greatest problem the human species has ever faced, that of generating clean energy and minimizing destruction of the environment, or to permanently end our existence.

Great minds recognize this gamble and continue to do science, not because they are ignorant of the potential catastrophic misapplication of their work, but because inactivity forfeits positive progress. The 20th century saw the United States climb to a position of unprecedented and still-unrivaled military supremacy built almost entirely upon technological and scientific progress. In the same span of time, however, the United States pioneered civil aviation on a massive scale, built a telecommunications network to connect the globe, eradicated or marginalized a host of diseases, and created a permanent laboratory for the pursuit of science in space.

The freedom scientists enjoyed within the research programs that produced such feats was an intentional compromise by their patrons. Far from holding unlimited power over their research “servants,” governmental funding bodies repeatedly tried to overtly centralize research organizations and direct them toward defense-related goals – each attempt either failed or jeopardized the health of the research body in question. The political motivation behind the Apollo Program rendered the space program extremely vulnerable when, after having achieved its goal, both public and political support vanished; later, orienting the design of the Space Shuttle toward military objectives fundamentally compromised its safety. Likewise, post-war attempts to maintain research centralization and military security in the national laboratories failed spectacularly as they hemorrhaged skilled scientific personnel – only after the national labs underwent a fundamental tonal shift to a more academic and open framework did they become the robust, integral research institutions they are today.

Gillispie may have been correct – scientists do often cooperate with the state – but they do so because the state is a vehicle for cooperative, mutual action. The state exists as a means to accomplishing what the individual cannot do alone. In conducting research efforts on behalf of or with the aid of the state, scientists are ultimately endowed with the means of realizing their own dreams.

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Appendix I: Interview with Dr. L. Raymond Fawcett

RM: This is Ryan Mooney interviewing Dr. Raymond Fawcett at the, uh, interviewer's residence for the Youngstown State University Oral History Program on October 27, 2014. Thank you very much for being here today.

RF: Glad to do it.

RM: Can I ask you, uh, a couple of brief things – just, uh, you know, where were you born, where have you lived?

RF: Yes. I was born in Richmond, Virginia in late 1936. Lived in the Richmond, Virginia area until the early 50s and my family moved to West Point, Virginia. And from there, of course, college began and I was in many places after that. So, um, I don't know how deep we need to go into this. Ask me what you need to know about the question you just asked. Do we need to extend it? IT seems to me we'll get into colleges and universities in a little bit. What would you like me to address next?

RM: Um, now would be as good a time as any to discuss your educational background. What got you into science?

RF: In elementary school, in the fifth grade, when our teacher was teaching world history, while this wasn't science I found it very interesting one of the things she taught was that two of the earliest written languages were Cuneiform, which was used in the Mesopotamia area and Hieroglyphics in Egypt. And I remember that all these years, in the sixth grade, we had a very large class but our teacher introduced us to the word research and she would give us subjects and we would have to look up the name, say it was George Washington Carver and what is he known for? And we'd go to encyclopedias – there weren't computers in those days – reference books to be able to write a paragraph about each of the list of names that she asked us to do research on. The point of mentioning that is the word research. Most of my high school was in our home town of West Point, VA and we were in a very small class. Our senior class had seventeen students in it and I was a hard working student so I was able to get into the college of William and Mary which was only 25 miles from our home but my father's business had just gone bankrupt and there wasn't any money to help me go to college so I worked at the local Safeway store and saved money the previous two years and the summer before college and got a small 300 dollar scholarship at the college of William and Mary and that was almost enough to get me through the first year and fortunately my advisor paid off the final bill and of course when I worked I went back and paid him as quickly as possible. But there was no money to begin a school year so I went back to work at the Safeway and while working at the Safeway I investigated possible opening at companies in Richmond, Virginia and was fortunate enough to get a job as a laboratory technician at Phillip Morris Tobacco Company, what was *then* Phillip Morris Tobacco company. And I made 235 dollars a month and got a promotion at the end of the third month to 270 dollars a month and lived in a boarding house across town with three other guys in a room for 1250 a week we got two meals five days a week and a place to sleep. Rode the bus to work and saved money like mad. And saved a great deal of money enough to go to college. Where would I go and now what would I study? I had certainly enjoyed physics in high school I knew it was going to be in science and I also liked biology and chemistry. But I lit on physics and I visited the university of Richmond, I was working in Richmond at that time, talked to the chairman of the department and he invited me to apply to the university of Richmond and I did so, got in, and got a small scholarship in the alumni office and with money I had saved that year, I was able to live off-campus very inexpensively and complete my sophomore year. But I had lost a whole year in saving money to get back in school at the University of Richmond. I did reasonably well then, and my advisor was able to get me placed ta what was then the naval weapons lab, a facility on the Potomac River where naval ordnance was tested and I made enough money living there in one of the barracks to have enough money to complete the next year and then the next year. So, in five years, I was able to get my master's degree and my advisor at the University of Richmond encouraged me to go on to graduate school. So, I made a number of applications and the University of North Carolina at Chapel Hill offered me an 1800 dollar per year teaching assistantship so I went there. I found out very quickly that graduate school was going to be extremely difficult for me. The competition was like nothing I had ever seen before. And, I did very poorly. I had hoped to obtain a PhD in physics there, but at the end of the first year, my grades were so poor that I was told I should go to work on a Master's degree, that I would not be eligible to get a PhD at the University of North Carolina. So, finally having passed the German reading exam after many attempts, I did my research, wrote a thesis, and was done. What to do now? To teach, or to go to work in industry? Well, I interviewed both. In those days, you could teach at a college or small university with just a master's degree in physics, physicists were in demand in the early sixties – that changed very quickly in the late sixties – but I was able to get a teaching job at Furman University in Greenville south Carolina and was there for three years and moved back to Virginia to what was then Longwood college in 1965. As an assistant professor of physics there and I was urged to, if I wanted to stay in teaching, that it would be in my best interests to try to obtain a PhD so after two years at Longwood college, I applied to Virginia Tech University and was admitted. And between some help from Longwood College and an assistantship at Virginia Tech, money was long no longer a problem. Being able to stay in school because of my poor grades was a problem and I found rather quickly that I needed to go back and stay some undergraduate courses, at least one, probably two, working on my PhD. I'm going through all this grief with you to tell you that it does not take brains to get a PhD; it takes guts determination, of keeping on, keeping on, no matter what the obstacles are. So, finally, I did get far enough along to take the PhD written examination. And I managed to fail with the highest tying score of failure on that examination, but the other gentleman who failed, unfortunately it was his second try and you don't get a third try so after many years of attempting and for him he was older it was many years of attempting to get a PhD he was washed out. That scared me terribly so I did nothing for the next six months but study how to solve fairly difficult physics problems and fortunately was able to pass the PhD writtens the second time around. And returned to Longwood College where I stayed for another subsequent 24 years and retired in 1992. But in the summers, rather than teach summer school, I made some applications to go away and was fortunate enough in 1979 to get a summer of doing research in what was then my field, which was measuring neutron capture cross-sections and I had done well enough with my PhD research that I learned fairly well how to measure those cross-sections on several elements but when I got to Argonne national laboratory in, just outside of Chicago, on this summer-long research grant, my mentor there felt that there needed to be a re-measurement of uranium 238 capture cross section in the KeV range, although it had been measured many, many times, there was a part of that cross section that was not well defined because so many researchers had gotten different values so we did that experiment very, very carefully and my mentor had just finished some other work and he asked if I had been, would be interested in presenting our summer work, the result of our summer work on uranium 238 at the international nuclear physics convention, which, fortunately that year was being held in Knoxville TN, a mere 350 mile drive from where I was teaching. I believe the previous year it had been held in japan and there is was no kind of way I would have been able to go to Japan. Went there and made the presentation and it happened that Dr. Raymond Hunter from Los Alamos and another gentleman from Lawrence Livermore Laboratory were interested in the work that I had presented and talked to me afterwards and I gave them a copy of the result of that work and they both asked would I be interested in doing some work for them. And Los Alamos was able to get a position together for me prior to Lawrence Livermore so in 1981 I spent the summer at Los Alamos Laboratory working on a problem that, the experiment had been done but the mathematical, analytics results did not anywhere match the actual experimental results and considerable effort had been put into trying to make the theoretical results match the experimental results. And that was an experiment that was particularly important to Dr. Raymond Hunter and he asked me, as an experimental physicist, to learn the theory, to analyze the results. Why did he want an experimental physicist to solve a theoretical problem? Because having had a good deal of experimental measurements made by then, I knew that side things that often theorists would not realize needed to be taken into account as to where neutrons would scatter, this was a problem on neutron capture but not measuring cross-sections. It was measuring neutron capture in a large sphere of lithium deuteride in which capsules of lithium 6 and li7 had been placed and lithium 6 and 7 both will capture neutrons and produce tritium. So, it was the tritium production that was critical to match the experimental results with theoretical results. It was thought I could solve that problem in the summer. It took six summers and the final result was published in Nuclear Science and Engineering. I think it was 1990. And while there, I worked on other problems, too, but why don't we get into those tomorrow. Why don't you ask me questions about what I've already said?

RM: Um, we covered a couple of things I'd already listed here. Um, earlier in your education career, when you were running into some difficulties you had spoken about what kept you motivated to persevere? What questions in physics captivated you and made you want to continue with that field?

RF: Well, it was not so much the physics as, since I had chosen teaching, teaching by then, I had to put beans on the table. We had a daughter and although my wife had been a teacher, we decided at the birth of our daughter that she was to be a stay-at-home mother. Her responsibility would be to raise that daughter and any other children we would have and mine would be to provide the financial resources. So, I had to get a job, keep a job, and get tenure. That was my driving force. It's just fortunate that I enjoyed physics.

RM: You had mentioned, um, earlier, I believe that at a very early age, you were already interested in higher education and achieving a higher level of education. I think you said, in, what was it, Kindergarten or-

RF: When I was five years old, I recall my parents saying to me, son you will go to college. Of course, they thought that they would have the financial resources to help me. But they very much wanted me to be the first person in the Fawcett family to achieve an actual undergraduate college degree. No one else had. And they were interested in education because they felt that, while their education was limited, with an education, one could do better at making money. When you don't have very much, money becomes very, very important. I don't much give a damn about it now because I have enough to maintain my lifestyle which is not an expensive lifestyle. Next?

RM: Actually think we can have a break here. Would that be ok?

RF: Ok.

RM: This is Ryan Mooney interviewing Dr. Raymond Fawcett on the 28th of October, 2014 at the interviewer's place of residency. I'd like to take this opportunity to thank you again, Dr. Fawcett. In our last session you mentioned working at Argonne and Los Alamos National Laboratories. Would you mind describing the nature of the work that you did at these facilities?

RF: Yes, in 198- 9, no 1979, I was fortunate enough to be able to go to Argonne national laboratory for a summer of research and I was assigned to work with a physicist who was involved in the kind of work that I did for my PhD dissertation. The measurement of neutron capture cross sections. And my mentor at Argonne Laboratory was interested in re-measuring the neutron capture cross-section of uranium isotope 238 in the KeV energy range. While it had been measured many times before, there was considerable disparity in the results of measurements that had occurred over the years by a number of different experimentalists in different laboratories throughout the world. So, I worked with my mentor in re-determining the capture cross-section of uranium 238 and at the end of that summer, my mentor had been working on another project, so he invited me to present our results on uranium 238 at the annual NuCon physics conference which happened to be in Knoxville, TN and I presented our paper and at the end of my talk, there just happened to be one of the physicists from Los Alamos National Laboratory and one from Lawrence Livermore Laboratory and I chatted with them at the end of that talk and offered them a copy of the paper. And they wondered if I would be interested in doing some work with them. And the physicist from Los Alamos made arrangements first for me to do some work with him so I was invited to Los Alamos in the summer of 1981 to perform a theoretical analysis of an experiment that had been done which involved the production of tritium in a lithium deuteride sphere. So, during the time that I did that, theoretical analysis, which took a number of summers, I became involved in work that was not related to weapons production. You had earlier mentioned that you wondered if any work went on at Los Alamos that was not directly weapons-related. So, I'm going to address that next, but first let me say something about the working atmosphere at Argonne. At Argonne, in the division where I worked and I can't right now recall the name of that division, there was some pressure for production. A meeting was held every week where the division leader addressed each of the physicists in our division and we all sat around and he would point to each one of us "what are you doing this week and what progress are you making?" At Los Alamos, the atmosphere was more relaxed. It was as if you were working at your own rate, and I certainly was, without the pressure of hurrying to get the work done. It was, I guess one would say a more laid-back atmosphere and there was great respect among the physicists for one-another in the applied theoretical physics division, where I was invited to work. So, if you like now, we can go on and, we can go on and talk about work done other than that directly involved with weapons work, analysis, and production.

RM: Well, do you care if I ask a couple of other questions-

RF: Please

RM: You mentioned the difference between the tone at Argonne and Los Alamos; did that affect the way, um, that the respective personnel at these facilities interact with each other. Was there a difference in the community? Was it felt? Could you tell by your colleagues, did it affect their work, either negatively or positively?

RF: I don't feel that it affected their work at Argonne. Certainly, I did not see it in the three months I was there, but there was more of a competitive spirit even though each of the staff members in the division where I worked were working on differing projects. It's difficult to explain. It's more of a feeling that I had and at Los Alamos, I didn't sense any competitive feeling. It was, I thought, a more respectful feeling of fellow staff members, because there wasn't a pressure that I observed to hurry to get things done. And because the physicists at both places were highly knowledgeable and accomplished; personalities were involved that were different by my likes at the two laboratories.

RM: And how long did you spend at Los – you mentioned you spent one summer at Argonne; how long did you spend at Los Alamos?

RF: Thirteen consecutive summers, first in 1981 and the last in 1993. I had retired from my teaching work at Longwood College in the spring of 1992 and there was no longer any nuclear weapons testing going on at Los Alamos and I felt ready, then, to enjoy my retirement.

RM: Can you describe the work that you did at Los Alamos?

RF: Yes. An experiment had been done some years before and there was a particular theoretician who was assigned to do the theoretical analysis of that experiment with the hope that the theoretical analysis would match the known experimental results. It did not. It did not even come close and there were a number of reasons for that. The reason I was invited to Los Alamos is because I was an experimental physicist all of my previous life and it was felt by those particularly interested in getting a theoretical match to the experimental results, felt that an experimentalist would just, from experience, know that many side, small effects occur during an experiment that a theoretician, not having done experimental work, may well not think of. And secondly, I felt no pressure to quickly get the results. So, I was allowed to make several changes in the way that the analysis was being done by using what I'll call ancillary data involving nuclear cross-sections of materials that were directly related to the theoretical analysis materials that were used in the experiment itself. And while the cross-sections, the neutron capture cross-sections for these other materials, such as I'll just pick some – tungsten, lead, copper, gold – these cross-sections had been measured by a number of laboratories previously and I was in a position to have access to, as far as I could tell, the best measurements that had been done on those cross-sections and I was in a position to be able to use several values of each kind of cross-section in the analysis. Hoping that I would use the most accurate of the more recent cross-section measurements of the ancillary materials. So, after several summers, I was fortunate enough to be able to very closely match the analytical, theoretical analytical results, with the already-having-been-measured experimental results. Now, the details of the work, the analysis, the title as best I can recall was "The analysis of tritium production in a lithium deuteride sphere," was published in nuclear science and engineering, I believe it was late 19 – uh, 90. I wrote up the results for publication in that journal. So, you can easily get your hands on that paper and read it for yourself. Where should we go from here?

RM: Um, what – you said that took several years to complete, that work?

RF: Several summers, because I would just drop it for the spring and fall semesters, going back to teach at Longwood and then I would pick it up when I would return for the following summer and I became involved in some other experiments that were not directly related to weapons production. Would you like for me to talk on that subject? Generally at first about laboratory work not weapons-involved, and then concentrate on a particular experiment that I was involved in.

RM: Well, the experiment you were conducting initially – that was related to weapon design? Or at least principles that would contribute-

RF: At Los Alamos, now?

RM: The one you just described, regarding neutron capture cross-section?

RF: Now, the experiment involving neutron capture cross-section occurred at Argonne laboratory. That was re-measuring the neutron capture cross-section of uranium 238 in the KeV energy range. Kilo-electron volt energy range. And it was that result that eventually led to my being invited to Los Alamos as a visiting staff member.

RM: Ok, so the analysis of tritium production was done at-

RF: Los Alamos

RM: And that was related to principles that would contribute to weapon design?

RF: Yes, without going into detail, I'm sure you are well aware that thermonuclear weapons involve the fusion of deuterium and tritium and so tritium production was certainly related to weapons design and development.

RM: You mentioned that there was work that was being done at the National Laboratories that was not related to weapon design?

RF: Yes. Now, Argonne's work, as far as I know, had no relationship to weapons design. It was, the part I was involved in, or the vision I was involved in, was experimental work. Experimental work in physics, in general, not just the kind of work that I did involving neutron capture cross-sections. Whereas, at Los Alamos, during the years that I was there and I suspect before and certainly afterward, a good deal of work went on that, from my point of view, had nothing to do with weapons design and development. I'll give you some examples of that. Persons, physicists that I respected greatly, who had been at the laboratory, Los Alamos, for a number of years, who were not interested in becoming administrators, they were given the title of 'Laboratory Fellow' and allowed to work on anything they wanted to. There may have been pet projects that they had had in mind since long before they came to the laboratory and now they had the time and the freedom to work on those projects all they wanted. I recall one day, one of the fellows came into my office and asked me if I knew anything about a particular kind of mathematical theory. Well, I had never heard anything about it and told him so, but for me, that was an example of a staff member working on something that I couldn't imagine had any relationship to weapons development. There were a number of other divisions, the physics division for example, where certainly a good deal of work went on with regard to weapons. A good deal of work also went on without regard to weapons and in the Los Alamos magazines that your father, Dr. Mooney, has, you can find more information about work that was not weapons related that was done over the years at Los Alamos. Now, if you like, I can talk to you about one of the particular projects I was involved in that was not related to weapons design.

RM: Before you do that, can I have you discuss real quick – you had mentioned earlier, off tape, about a meeting that had occurred where some people had voiced concern over the research that had been proposed-

RF: Oh yes! Now, as far as I know, while I was not involved in any way with budgets, the laboratory funding was, at least by my likes, predominantly for the purpose of weapons work. However, it certainly, as best I could tell, in no way required to be used totally for weapons work. However, a million dollars taken for a pure physics project would be a million dollars not available for weapons work. So, while most of the division leaders of the several divisions at Los Alamos were in support of spending some money that was not used for weapons development and weapons work, one or two of them, for the experiment that I became involved, expressed some reluctance to spend the amount of money that was going to be required to do that experiment, but that's all – just simply expressed reluctance. So, there was no hesitation by the experimenters that were involved to spend what was necessary to get this particular experiment done. Should I say something about the experiment?

RM: That would be wonderful.

RF: I was involved in a non-weapons kind of experiment that had been suggested a number of years before by people who were not at Los Alamos. I had first heard of it as a graduate student at Virginia Tech and that experiment was to come up with a way to measure the S-wave scattering length of a neutron. In simple terms, it is to measure the size of a neutron and it was quickly realized that huge numbers of neutrons would be necessary to do such an experiment and that nuclear power reactors, while they produced untold numbers of neutrons, were not nearly enough for this kind of experiment whereas a nuclear fusion explosion would produce huge numbers of neutrons and produce the data for the measurement experiment instantaneously. So, we went to work setting up how to do the experiment and doing the theoretical work with regard to the geometry of how the experiment had to be done and I was involved in the geometry part. So, I offered to look up the volume of intersection of two cones. I won't explain right now why that was a piece of information that was absolutely necessary for the gathering of data and analysis of the neutron scattering experiment – that is, the experiment to measure the size of the neutron. And I found that that volume was not known, that if it ever had been known, it was lost and extensive research was done, for me, looking for that volume of intersection and when we finally concluded that it was not known, I set to work with a physicist from the theoretical division to develop the geometrical theory and produce the answer for the volume of intersection of two cones. Well, we were able to make that determination, the algebraic formula, we were fortunate enough to be able to determine it in closed form – that is, where no approximation was necessary. So, we published that as a side bit of information in the journal Nature and I forget the year we published it. I'm going to say probably 1985 or 1986. Then, we also needed to know what is the volume of two not completely overlapping but intersecting cones and we went to work on that and, while most of that work was beyond what I knew how to do, I was involved to the extent that I kept up with what two other physic- mathematicians in the theoretical division were doing until that formulation was determined and it was not in closed form. It involved something called iteration, however, that was new information also and we published that and I believe we published that in Nature also. Now, I don't know how much detail I should go into as to how we went about attempting to measure the size of the neutron.

RM: As much as you'd like.

RF: Ok. To get a huge number of neutrons from two separate beams, we did what was called a hang-on experiment. When an experiment whose major purpose is to test a nuclear weapon, not only the results of what that test turns out to be, is certainly important, of major importance, however, there is a good deal of space available to do ancillary experiments that are dependent on that nuclear explosion. So, our group – or the, the work was mostly done, the grunt work of actually going there and setting up the experiment and getting the materials necessary together was done mostly by people in the physics division because when they heard about the possibility of making the measurement of the size of a neutron a number of people from the physics division were very interested in that. It was- would be new information that no one had ever measured and as far as we knew, never attempted to measure because very few people had access to nuclear explosions. So, our group, and I say our group, people from applied theoretical physics, where I worked, people from physics, and, I don't remember now but probably others from other divisions, we went about doing the experiment this way. At one end – this was a tunnel shot – at one end of the tunnel was the primary device, which was a fission device and on each side of that fission device, we were allowed to put what was called a "bowler." As far as I can remember, it was just a glass sphere filled with compressed tritium and deuterium. One bowler was placed on one side of the primary and the other bowler on the other side and, of course, when the primary went off, the radiation from it would set off the fusion reaction in the two bowlers and neutrons from the bowlers would go in all directions. Well, from our point of view, we needed to block out all of the neutrons except those that were collimated. What is a collimator? A collimator is a circular hole in a material that blocks out the neutrons except those that go through the hole and while I'm talking here, I can do some hand motion to show you what I mean. You'll just have to remember that so you can do hand motion later. So, here is this big device. It's got a hole here, it's got a hole here, and it's a few meters away from this bowler and this bowler. Alright, when these bowlers go off, having been set off by the primary, here, neutrons, fragments, alpha beta gamma x-rays, fly in all directions. We weren't interested in anything other than the neutrons, so those neutrons that were hitting in the direction of our experiment, we blocked those with a neutron-absorbing material except for the hole here and the hole here. Well, you know that if you start with a point source, here, and you have a hole, here, then what you get is a diverging beam of neutrons in a cone and another diverging beam of neutrons in a cone and you have this volume of intersection. Well, the neutrons that bump one another will be scattered in all directions and we had detectors placed all around here, neutrons detectors, to detect those neutrons. Well, of course, we wanted only neutrons, so in order to stop the debris, which would follow the neutrons out more slowly – by debris I am including alpha and beta and fission fragments – they follow the X- and Gamma radiation coming out first, then comes the neutrons, then come all the other debris - you had to get these blast doors closed immediately after the bowlers went off in order to stop the debris and have only neutrons coming through. The blast doors didn't close in time and all of our recording instrumentation downstream in the tunnel, well off from the tunnel in an alcove dug into the side of the tunnel, that did have, it was closed off but it should have been closed off in a more sophisticated way, the debris got down there, exposed all the film, and ruined the recording devices and as far as I know, to this day, in 2014, the experiment has never been attempted again. It certainly hasn't been attempted in this country since 1989 when the then-president terminated nuclear underground testing.

RM: The mishap that occurred that prevented any data from being collected, or preserved, did that – was that a source of anxiety or disappointment for people at Los Alamos who had worked on this experiment?

RF: Oh yes, it was a disappointment for all of us. You see, we may have made the measurement. That is, we may have gotten the data that, when analyzed, produced the results that we were looking for, but because the alcove with the recording equipment was not sufficiently protected, it was all lost. We really expected the blast doors to close. They had been closing in time for other experiments in tunnel shots over the years, but they failed for us.

RM: So, were these hang-on experiments common? Did other science that was conducted at the national laboratories, yours and others, depend upon nuclear devices?

RF: Yes. Radiation effects experiments. As far as I remember, at the far end of the tunnel, racks of electronics equipment were set up to be exposed to the X and gamma radiation that left the site of the explosion at the speed of light and flew down the tunnel to the racks of electronics materials in the far end of the tunnel. So, radiation effects was then and is still now an important consideration on electronics equipment.

RM: So, the experimentation that was associated with Might Oak was the shot the test explosion, if I'm correct, and you said the experimental apparatus was named EMZY?

RF: The part of the explosion that dealt with our concern, as best I remember, was named EMZY. When I say part of the explosion, the two bowlers, it was a long time ago, and I'm thinking back now, I think the yield from each bowler was small, about a kiloton. But still, a thousand tons of TNT is a good size explosion.

RM: I'd say so. So EMZY, this experimentation package, was it prepared at Los Alamos prior to being set up? Did personnel from Los Alamos accompany the equipment and set it up or was it prepared before being sent? What was the general logistical picture?

RF: The experimental setup was fabricated in the tunnel. It was big and it was heavy and a lot of materials were necessary so those materials, as best I can recall, were hauled from Los Alamos out to NTS, the nuclear testing site north of Las Vegas in Nevada, and most of the fabrication work, the setup work, was done by physicists from the physics division. I was not involved in going to the test site to help setup the experiment. My part had to do with helping with the geometry that was necessary, as I mentioned before, the volume of intersection.

RM: If I think I heard you correctly, earlier you said that the objective of this experiment was to measure the size of the neutron?

RF: Yes

RM: Thank you

RF: Technically speaking, it was to measure the S-wave scattering length, which, in common terms, is the cross-section of the neutron. And why is that the size? Well, think of taking an orange, chopping it in half, and looking at the half. What are you looking at? The cross section. So, if you know the cross section and you assume spherical in design, then you know the diameter.

RM: What greater scientific impact did this experimentation have? Why was it being investigated? What impact would it have had on our concept of the world around us? What scientific importance did it hold?

RF: That's an interesting question. It – the first thing that comes to my mind is that it was new knowledge. To learn the size of the neutron would be something that people, over the years, have wanted to know simply because neutrons exist and we want to know more about them. One of the factors of knowing more is how big they are. We know their mass from far simpler considerations we know their mass. But we don't know their size. It is simple to assume that they're spherical, but suppose you've got fewer neutrons scattered this way than you've got scattered this way – wouldn't that imply not spherical, but maybe, what, elliptical in cross section? Or maybe they don't have a well-defined size. Maybe from quantum mechanics we would think of them as being sort of fuzzy masses. I don't know. Nobody knows because if they're like hard balls, we would have gotten one neutron scattering result, if they are like ellipses, we would have gotten another, and if they're fuzzy, we would have gotten another. It's just basic knowledge and man just wants to know.

RM: And this experiment still can't be conducted with reactors? It would still require, to be replicated, it would require the detonation of a nuclear primary?

RF: Yeah. I believe that to be true. Even from a high powered power reactor, I think it would take years to get the streams of overlapping neutron cones necessary to get enough data to make a measurement that you would have any confidence in.

RM: Thank you so much. I think we're going to stop here and take a break if that's alright with you

RF: Oh, yes.