2020

The Emergence of Modernity in the Early Aerospace Industry 1950-1970

Lachlan Warwick Sands
Claremont Graduate University

The Emergence of Modernity in the Early Aerospace Industry 1950-1970

By

Lachlan W. Sands

Claremont Graduate University

2020

Approval of the Dissertation Committee

This dissertation has been duly read, reviewed, and critiqued by the Committee listed below, which hereby approves the manuscript of Lachlan W. Sands as fulfilling the scope and quality requirements for meriting the degree of Doctor of Philosophy in History.

Daniel Lewis, Chair
Claremont Graduate University
Research Associate Professor of History
&
The Huntington Library
Dibner Senior Curator for the History of Science and Technology

Joshua Goode
Claremont Graduate University
Associate Professor of Cultural Studies and History

Richard G. Olson
Professor of History Emeritus
Harvey Mudd College

Abstract

The Emergence of Modernism in the Early Aerospace Industry 1950-1970

By

Lachlan W. Sands

Claremont Graduate University: 2020

The literature regarding the management of science during the early aerospace industry between 1950 and 1970, and more specifically the period of Lockheed's transition from aeronautics into aerospace, is sparse and the subject is insufficiently studied. In this dissertation I examine how the practice of science changed in the United States in the decades after World War II. The scientific endeavor in the United States manifested a transformation from an enlightenment-based set of norms to a new modern ethos. For all intents and purposes, the traditional intellectual boundaries between basic science, applied science, and technology dissolved across the majority of the American scientific endeavor in the from 1950 to 1970 as science became objective oriented. This is in contrast to the common acceptance among historians of science who place this change in the 1970's and 1980's concomitant with the development of the entrepreneurial university.

The majority of original research presented here comes from the archives of Lockheed executives Willis Hawkins, who started as an engineer and would ultimately ascend to company President, and Ben Rich who was a staff scientist who also rose to lead Lockheed's semi-autonomous Advanced Development Program or Skunk Works.

The dissertation is grounded in Etzkowitz and Leydesdorff's Triple-Helix Model of science and demonstrates that the early aerospace industry conducted and managed science with the modern characteristics of human resources circulation, the development of innovation networks, reflexive

output circulation, and non-linear innovation. Additionally, I amend the Triple-Helix model by proposing that the original version, which is scaled to describe large organizational and national science policy, can be used to model modern science management at three discrete scales – macro, meso, and micro. Finally, the study identifies as outcomes the role of industry associations, the loaning of human resources, and of capitalism during the period, and in the persistence of the Linear Model.

Acknowledgements

Thank you to Dr. Daniel Lewis. Without Dan's patience, guidance, and thoughtfulness, this dissertation would never have been finished. Also thank you Dr. David Edens, my friend of many years who pressed me week after week to submit my drafts. And finally, to Dr. Jennifer Padilla, who has ever been an inspiration, a comfort, and a soul-mate, thank you for always being with me. I love you.

Contents

Chapter 1

Introduction

This chapter describes the current understanding of modern scientific practice, and how intellectual and practical norms have modernized in the American scientific endeavor since the mid-Nineteenth Century. It also establishes a roadmap for this study including an explication of the central thesis. In the second half, this chapter examines how enlightenment-based scientific norms changed so significantly in the 1930's. This sets a context for the remainder of the dissertation which studies the practice of science in the aerospace industry the period of 1950-1970.

In this dissertation I examine how the practice of science changed in the United States in the twenty years after World War II, from an enlightenment-based set of norms to a new modern ethos. I suggest that, for all intents and purposes, the traditional intellectual boundaries between basic science, applied science, and technology dissolved across the majority of the American scientific endeavor in the from 1950 to 1970. This is in contrast to the common acceptance among historians of science who place this change in the 1970's and 1980's with the development of the entrepreneurial university.

To demonstrate the dynamism of scientific norms, this dissertation traces the evolution of enlightenment-based science norms into WWII, then examines the practice of modern science in the early aerospace industry through the lens of the Triple-Helix Model of modern scientific practice.

World War II was an inflexion point in the practice of what historian of science Thomas Kuhn dubbed 'normal science.' In his *The Structure of Scientific Revolutions,* Kuhn described 'normal science' as the day to day mopping up of the details within a 'paradigm,' or constellation of scientific thought. Normal science follows a paradigm shift which occurs when a contemporary scientific model is unable cope with anomalies identified during normal science, and a new, more illuminating model is adopted as a foundation for a scientific field. Newtonian physics, natural selection, mendelian genetics, and quantum mechanics are all examples of paradigm shifts which transformed fields of scientific study, and under which scientists practice 'normal' day-to-day science.[1]

However, the scientific norms related to the practice of normal science, as differentiated from the ontological framework a paradigm supplied, were rooted in the enlightenment and did not change significantly until the 20th century[2]. From the end of the 19th century through to the end of WWII these enlightenment-based norms became insufficient to cope with the realities of radical new scientific paradigms and cultural shifts punctuated by two world wars. Norms such as ultimate causality, scientific disinterestedness, objectivity, communism, and the primacy of observation lost their practical value to normal science since they were progressively less able to elucidate the scientific problems in a modern world. By the interwar period, scientists needed a new intellectual language, and almost a century after the same shift had occurred in art, literature, and philosophy, science shook off the shackles of enlightenment scientific norms and adopted a modernist ethic, and implemented modern scientific practice.[3]

---

[1] Normal science refers to both the fact that the majority of scientists practiced science the same way, and that they shared a common series of intellectual norms. Sociologist Robert Merton first identified the norms of what he called 'normative' science in 1942 in his article *Science and Technology in a Democratic Order.* Merton's norms were communism, universalism, disinterestedness, and organized skepticism. See Robert King Merton, "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology* 1, no. 1 (1942): 115–26; Robert King Merton, *The Sociology of Science : Theoretical and Empirical Investigations* (Chicago : University of Chicago Press, 1973).

[2] To clarify, a generally agreed-to set of principles which bound and illuminate a scientific field comprise the 'paradigm' of the field. On the other hand the norms of day-to-day science are a shared set of beliefs about the practice of science itself and are independent of paradigms. These norms were established during the enlightenment and I argue here that they changed significantly in the 20th century.

[3] Thomas S Kuhn, *The Structure of Scientific Revolutions: 50th Anniversary Edition*, Fourth edition (Chicago, IL,: University of Chicago Press, 2012).

Throughout this work, modern and modernist are differentiated. By modern, I simply mean contemporary. Modernism, on the other hand, is cultural reaction to having an insufficient language and analytical toolkit to cope with contemporary phenomena.

Contemporary models of science describe these normative changes as taking hold of the larger scientific endeavor in the late 20th Century. Historian Paul Forman suggested the moment was circa 1980 when applied science gained primacy over "pure," or undirected science[4]. Henry Etzkowitz and Loet Leydesdorff, originators of the Triple-Helix Model of modern science, added to Forman's thesis and identify four characteristics of modern science – a focus on human resources, the development of innovation networks (or communities), reflexive output circulation, and non-linear innovation.[5]

The models of science which have gained historiographical credibility describe the relative slowness of change of scientific norms. Indeed, the influential description of enlightenment-based normative science made by Robert Merton remained unchanged between his first paper in 1942, and his final reprint in 1973. A partial explanation for this is that academic science (that which is performed in the college or university setting) changes practice slower than industrial or governmental science. However, academic science represents only a fraction of all the science occurring in industrial, and governmental environments. Despite this, academic science is by far the most historically studied form of scientific endeavor. Historians of science have for many years based their descriptions on the shaky assumption that the normal science practiced in universities represented the entire American scientific endeavor. After WWII, it simply did not. Looking at the American scientific endeavor through the lens of

---

[4] Forman was describing this change in both the social and public policy perspectives. It is also important to note that Forman had defined the enlightenment-based practice of science as 'modern' and the newer form as 'post-modern.' My definitions are based on intellectual modernism rather than the broader modern historical era. For the purposes of comparison, Forman described the characteristics post-modernism in the same way I describe modernist science i.e. when basic science, applied science, and technology conflate to prioritize outcomes.

[5] Paul Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," *History and Technology* 23, no. 1–2 (March 2007): 1–152; Henry Etzkowitz, *The Triple Helix : University-Industry-Government Innovation in Action* (New York : Routledge, 2008).
For a detailed explanation of the Triple-Helix model, see Chapter 2.

the largest scientific community – industry – I argue that the norms of science changed to a modern praxis decades earlier than historians and sociologists of science generally accept.[6]

The Structure of this Study

Foundationally, the Triple-Helix model, which is described in detail in Chapter 2, achieves a functional description of the practice and management of modern science, which differentiates it from the enlightenment-based norms which Merton described. In this dissertation, I show how the early aerospace industry, in particular Lockheed Corporation from 1950-1970, exemplifies the Triple-Helix model by applying modernist norms to the management of science. It is important to note that while modern norms gained primacy, enlightenment-based norms continued to exist as ethical goals. Additionally, I amend the Triple-Helix model by proposing that the original version, which is scaled to describe large organizational and national science policy, can be used to model modern science management at three discrete scales – macro, meso, and micro.

The majority of original research presented here comes from the archives of Lockheed executives Willis Hawkins, who started as an engineer and would ultimately ascend to company President, and Ben Rich who was a staff scientist who also rose to lead Lockheed's semi-autonomous Advanced Development Program or Skunk Works. Both archives are large, and contain correspondence, diaries, speeches, notes, and research results which offer a broad insight into the management and practice of industrial science in the twenty years following WWII. Additionally, I consulted the Huntington-USC Institute on California and the West's oral histories from aerospace employees which were collected as part of their Aerospace History Project.

---

[6] Merton, "Science and Technology in a Democratic Order"; Merton, *The Sociology of Science : Theoretical and Empirical Investigations*.

This dissertation is arranged in seven chapters. This introduction sets a context for the intellectual environment in the American scientific endeavor following World War II. It begins with a review of the influence of modernist thought[7], scientific discovery, and warfare on the norms of science at the individual and intellectual level leading up to WWII. It is necessary to start with a discussion of the intellectual context which led to the changes in science as practiced. It was new modernist intellectual norms that set in place foundation in which a partnership between academia, industry, and government (modeled by the triple-helix) could thrive. The research for this chapter comes primarily from published articles and books during the Nineteenth and Twentieth Centuries which illuminate the changing philosophies of individual scientists.

The second chapter is a review of related literature within the purview of this dissertation. The early development of modern American science, as modeled by the triple-helix, and through the lens of the early aerospace industry, has very little directly written about it. However, obliquely related work on cold-war science, post-WWII science policy, the history of aerospace, and historiographical analyses of models of scientific endeavor do exist in enough quantity to give a supporting context to this work.

The third chapter begins a series of chapters which explore the four key Triple-Helix based characteristics of modern science management. The first of these is an examination of the importance of human resources circulation. Topically, this chapter breaks into a discussion of macro scale issues such as the movement of human resources between the key actors of academia, industry and government, and international recruiting. The meso stratum of human resources issues can be seen in the enormous efforts placed in recruiting top talent. Another meso-level instance I examine is the

---

[7] The term modern and modernist occur throughout this dissertation and refer to the set of intellectual and practical norms which developed in science as a response to the insufficiency of enlightenment-based norms to solve contemporary scientific problems. The terms do not refer to the "modern era" which is often described as the enlightenment through to the post-modernism of the 20th century.

“loaning” of people on a temporary basis to other key actors for consulting and teaching. The final set of examples are at the micro level. They are about individual collaborations and permanently sharing individuals between key actors.

Chapter Four examines innovation networks. These are communities developed to encourage invention and innovation. These communities are comprised of different intellectual and physical spaces which codependently established strong communication systems between Triple-Helix stakeholders, promoted undirected thinking and research with a minimal regard to development, and ensured that there were ample resources budgeted so researchers did not get distracted.

In Chapter Five I discuss examples of reflexive output circulation. These are characteristic of modern science in which knowledge can move forward and backward on the triple-helix progression toward technology. This chapter looks at the concomitant modern norm of secrecy which developed as a response both to the climate of the cold war, and to the growing influence of capitalism on science. The chapter ends with an examination of how the ideal of open communication can exist and thrive in an environment where confidentiality is also a priority.

Chapter Six shows examples of non-linear innovation within the scientific enterprise through the lens of Project Hindsight. The Linear Model, while insufficient to describe the practice of modern science, has remained remarkably resilient as a model at the national policy level. Project Hindsight was the government’s attempt to justify and quantify the Linear Model of innovation. All three Triple-Helix stakeholders pushed back on the results of Hindsight and their objections demonstrated the non-linearity of modern innovation.

The final chapter, the conclusion, includes a summary of findings, a discussion of the implications of this research, opportunities for further analyses and research, and potential arguments against the points in this work.

The Rise of Modernism in Science in the 1930's

The normalization of modernist scientific ethos manifested in the American scientific endeavor primarily in the interwar period, but this manifestation was the culmination of long cultural and intellectual processes rooted in the Nineteenth Century. World War I highlighted the interaction of science on warfare internationally. The public perception of this "Chemist's War" revolved around technological achievements such as poisoned gas and advanced explosives, but the war was also notable for the introduction of aerial bombing, submarines, tanks, and highly portable mechanized guns. From a cultural perspective there was an ambivalence toward the science behind these technologies. Industrial science provided wartime advances such as Fritz Haber's and Carl Bosch's process of catalyzed nitrogen fixation which revolutionized both the manufacture of fertilizer and explosives. It allowed Germany to continue making gunpowder without imported Chilean saltpeter (unavailable due to the British blockade) but was also a necessary component of the first Green Revolution. Nevertheless, scientific complicity in warfare was a jarring counterpoint to the humanism of the enlightenment scientific ideal.[8]

Prior to World War I, the norms of science held closely to four longstanding enlightenment traditions – the primacy of direct observation, objectivity, causality, and the neutrality ideal. Historian Dorothy Ross in *Modernist Impulses in the Human Science, 1870-1930* credited H. Stuart Hughes with the identification of the "intellectual revolution" which occurred at the turn of the turn of the 19th century. Ross defined a common focus on the subjectivity of perception and cognition as *cognitive modernism*.[9] Enlightenment-era epistemological systems were insufficient to cope with modernity and new cognitive and representational systems were needed. The transition to modernist scientific norms

---

[8] For a long synopsis of the contribution of chemistry as a field to World War One, see Michael Freemantle, *The Chemists' War: 1914-1918* (Royal Society of Chemistry, 2015); Dorothy Ross, *Modernist Impulses in the Human Sciences, 1870-1930* (Baltimore: Johns Hopkins University Press, 1994).
[9] Ross, *Modernist Impulses in the Human Sciences, 1870-1930*, 3–4.

demonstrated a set of four dominions of change which worked across the borders of scientific endeavor: the abstraction of science into mathematical models, a new hierarchy of scientific endeavor, the loss of key enlightenment scientific philosophical tenets[10], and new application of social praxis.[11]

The Primacy of Observation

The first of these traditions to demonstrate insufficiency was the primacy of observation. Enlightenment scientific practice and philosophy was established in the 17th century scientific revolution. It was a blend of the scientific methods of Francis Bacon (empiricism and observation, causality, universality, and improvement of mankind's state) and the *Discourse on Method* of Rene Descartes (deduction, objectivity, application). Bacon's first aphorism in *The New Organon* set the tone for the imperative of observation stating that the interpreter of nature can only know what he has observed; "beyond this, he neither knows anything nor can do anything." However, by the middle of the nineteenth century, developments within science, especially electro-magnetic experimentation and evolutionary biology, presented examples of reality which could not be observed. To cope with these anomalies, scientists began to abstract science from raw observation to mathematical modeling for verification of conjecture.[12]

Abstracting simple observation to first or second order experimentation was as old as the scientific revolution itself.[13] Francis Bacon referred to manipulation of nature as "twisting the lion's tail to make her cry out her secrets." Robert Boyle carried this one step further with his air-pump by

---

[10] Objectivity, causality, observation.
[11] Olivier Zunz, "Producers, Broker, and Users of Knowledge: The Institutional Matrix," in *Modernist Impulses in the Human Sciences, 1870-1930*, ed. Dorothy Ross (Johns Hopkins University Press, 1994), 290–309.
[12] Steven Shapin, *The Scientific Revolution* (University Of Chicago Press, 1998); Francis Bacon, *Francis Bacon: The New Organon and Related Writings*, ed. Fulton H. Anderson, First edition thus. (Prentice Hall, 1960), 39.
[13] First order being the manipulation of nature. Second order being a symbolic experiment immediately verifiable in nature.

examining situations which could not occur in nature and extrapolating his observations to generalizations about universal law. Even so, the ultimate test of these observations was to determine congruity with nature, and to be able to predict natural behaviors by applying the discovered laws. Newton and the Newtonians, whose scientific method was the polyglot used throughout the 18th and 19th centuries allowed for greater latitude with abstract experimentation. Newton abstracted optics, astronomy, and physics into the symbolic language of mathematics to aid in prediction, but like Bacon and Boyle, he verified with direct observation.[14]

Two developments in the mid-eighteenth century brought into question the reliability of observational verification and deductive reasoning in science. Initially, the development of non-Euclidean geometries by Bolyai, Lobachevsky, and Poincaré. That a new definition of space was mathematically possible demonstrated the insufficiency of enlightenment scientific practice. This was because Euclidean geometry was the basis for the axiomatic logic which Descartes had popularized.[15] In order to function properly, non-Euclidean geometries had to negate Euclid's 5th axiom (the parallel postulate).[16] Since the axiomatic logic that was foundational to the enlightenment scientific epistemology was based on Euclid's axioms, negating Euclidean geometry also implied negation axiomatic mathematical logic. This was the first significant logical challenge to the foundations of enlightenment scientific method.

---

[14] Thomas S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (University Of Chicago Press, 1977), 50; Bacon, *Francis Bacon*, 163; Margaret J. Osler, "The New Newtonian Scholarship and the Fate of the Scientific Revolution," in *Newton and Newtonianism: New Studies*, ed. James E. Force and Sarah Hutton (Dordrecht: Springer Netherlands, 2004), 1–13; Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump* (Princeton University Press, 1989). The work is filled with examples of Boyle abstracting to second order experiment. Perhaps the most famous, the void-in-a-void experiment is detail in pp. 40-41

[15] Descartes famous *cogito ergo sum* being the first principle from which he built a systematic worldview.

[16] The Parallel Postulate is independent of the other four postulates and defines a flat plane. Non-Euclidian geometries obey the other four postulates, but require a different postulate for parallel lines. Therefore, new spaces were mathematically possible which could not be visually observed.

The second challenge to the enlightenment scientific method was the publication of Darwin's *Origin of Species* in 1859. Darwin both supported and negated the ideal of observation as a foundation for scientific method. While his exhaustive observations of nature led him to develop the theory of natural selection, the work required the scientific establishment to recognize the concept of reason outside of observational experience; one could not directly observe natural selection, only its outcomes. Darwin extended his observations to cover vast stretches of time so Newtonian observational verification was by definition impossible. *Origin* also was about the study of relationships between objects, not the objects themselves. Again, the lack of observable process was a challenge for the enlightenment scientific understanding.

The electron posed another transformative frustration for scientists trying to work exclusively through an enlightenment lens. The existence of an electron had been demonstrated conclusively in 1900, and in 1909, Robert Millikan conducted his famous oil-drop experiment to determine an electron's exact charge. The philosophical implications of mathematically proving an atom's divisibility were hard to ignore because the results were not directly observable but mathematically indisputable. Foremost it was clear that to continue experiments on objects impossible to observe one must entirely dispense with direct observation for verification and rely entirely on mathematics and inference.[17]

The Mathematization of Science

Darwin and Millikan were not the only scientists to cut at the enlightenment cord which held axiomatic logic and experience together: Mendel's work on statistical probabilism in genetics was rediscovered in 1900, Planck established the field of quantum mechanics in 1901, and Einstein published

[17] Karl T. Compton, "The Electron: Its Intellectual and Social Significance," *Science*, New Series, 85, no. 2193 (January 8, 1937): 30–31.

his work on relativity and the photoelectric effect in 1905. This observation-independent science applied a pressure across all fields of science and resulted in a new focus on mathematics. Not only did intangible concepts like the electron and natural selection need representation, but *relationships* between objects needed description. This was a basis for the development of a new scientific method. The scientific community adopted mathematical modeling and verification as a descriptive and predictive language to cope with these modern challenges.

Historian Theodore Porter traced the roots of many of these changes to the influence of Ernst Mach and Karl Pearson. Mach, heavily influenced by Hertz's verifications of Maxwell's electromagnetic theory, developed a method which would satisfy the need of a new descriptive language for science. Mach espoused a positivist philosophy which had elements of both modernism and the enlightenment ideal. He emphasized both the importance of mathematical modeling, and the essential act of observation, but he accepted inference as evidence in the verification of postulates. Mach also believed that truth was unknowable since all knowledge was subjective. Porter credits Karl Pearson, Mach's protégé, with a far greater practical influence through his book *The Grammar of Science (1892)* which established a new scientific method built on statistical probabilism.[18]

Pearson's work was a universal scientific methodology; it was an adjunct to contemporary scientific thought rather than a replacement. Despite his enlightenment views on the importance of observation, Pearson suggested that the universe was in no possible arrangement homogeneous, and that all observation was statistical in nature. Since pure objectivity was not possible, Pearson suggested that all observations were in fact "correlations" between observation and expectation. Physics tended to have exceptionally high correlations while biology and social sciences displayed progressively lower

---

[18] Theodore M. Porter, "The Death of the Object: Fin de Siecle Philosophy of Physics," in *Modernist Impulses in the Human Sciences, 1870-1930*, ed. Dorothy Ross (Johns Hopkins University Press, 1994), 128–51.

correlations. From another perspective, Pearson said that *relationships* were as real as tangible objects, the study of relationships was at the heart of the new scientific endeavor.[19]

Probabilism also led Pearson to explore the concept of assent as a substitute for objectivity. He suggested that with the concept of objective truth in doubt, it might be enough that an educated majority agree on what is truth. The concept of coherence theory of truth replacing the enlightenment concept (and religiously supported) correspondence theory of truth struggled to take root.[20] In practice, peer-review, higher education, the scientific publishing system, the conference system, and scientific organizations by their existence demonstrated a structural acceptance of assent-based truth, but individuals within science struggled to give up the idea of an objective truth as the goal of their researches.[21]

The historiography of truth identification in the twentieth century has demonstrated a shift towards accepting the subjective. In 1936, Polish biologist Ludwig Fleck called the assent-based system "thought collectives" and regarded them as a natural response to hedge against relativism; for if there is no objective truth, all things might be relatively true. The major criticism of Thomas Kuhn's *The Structure of Scientific Revolutions (1962)* was that his model did not have an oppositional force to relativism. Present-day historian Sandra Harding suggested the epistemological relativism described by Kuhn and Fleck demonstrated that institutional relativism and cultural objectivism are "twins" and she agreed

---

[19] Porter, 143–47.
[20] Harold Joachim, *The Nature of Truth : An Essay*, Facsim. ed. (Wesport: Greenwood press, 1977). Correspondence theory is a classical notion of truth that says that truth is determined by how closely our thoughts and statements correspond to the real world. This concept is predicated on the assumption that the real world is both rational, and concrete. As a logical outcome of this system, there is only one truth and all else is fallacy. This therefore is a dualist, or binary system.
Truth in coherence theory is the description of an object that is most supportive of the system of conceptions already surrounding that object. Instead of comparing a statement to a physical object to determine truth, coherence theory compares a statement to all other related statements. The key outcome of this system is that no one truth exists since truth is based on individual (and sometimes group) perception.
[21] W. V. Houston, "The Philosophy of Physics," *Science*, New Series, 85, no. 2209 (April 30, 1937): 419.

with Dorothy Ross that the individual struggle to maintain opposing philosophies and remain functional is a characteristic of a modernist epistemology.[22]

The Abstraction of Science

This dualism can be seen clearly in the case of the electron paradox. Here was a "thing" which was both real and its effects were measurable, but the electron itself was a mathematical construct. Ultimately, Heisenberg explained this paradox in 1929 with his uncertainty principle.[23] Since relativity by 1930 had been around for 25 years, many of the contributors to the journal *Science* were already in a semi-modernist frame of mind; they had accepted some positions necessary to keep them current, but enlightenment norms remained a priority. In the late 1920's the work of Niels Bohr and ultimately Heisenberg (who was a member of Bohr's lab when he developed the uncertainty principle), prompted several articles in *Science* which addressed issues of the modernist epistemology and came to accept more modernist perspectives as the decade wore on.[24]

Tracing the acceptance of mathematical modeling and mathematical training for scientists is a way to determine the general acceptance an important aspect of modernist thought. As a baseline example, in 1924 Columbia physicist I. M. Pupin declared that scientific training should not be

---

[22] Robert Sonné Cohen, Thomas Schnelle, and Ludwik Fleck, *Cognition and Fact: Materials on Ludwik Fleck* (Springer, 1986); Sandra Harding, "After the Neutrality Ideal: Science, Politics and 'Strong Objectivity,'" in *The Politics of Western Science, 1640-1990*, ed. Margaret C. Jacob (Humanities Press, 1994), 81–101; Ross, *Modernist Impulses in the Human Sciences, 1870-1930*, 5–9.

[23] As with so much of quantum mechanics, the math was often highly developed prior to an application being developed. The concept of uncertainty was presaged by Joseph Fourier in 1822 when his work with harmonics led him to develop a series of transforms (now called Fourier Transforms) which held frequency as a variable independent of time. Heisenberg adapted Fourier's equations to demonstrate that when either location or velocity was concentrated, the certainly of the other was diluted.

[24] For a more detailed discussion of Bohr's philosophy, see David Lindley, *Uncertainty: Einstein, Heisenberg, Bohr, and the Struggle for the Soul of Science*, 1st ed (New York: Doubleday, 2007); Also see the review of this Lindley's work: Paul Forman, "David Lindley. Uncertainty: Einstein, Heisenberg, Bohr, and the Struggle for the Soul of Science.," *Isis* 100, no. 1 (March 1, 2009): 180–81.

mathematical, but one of discipline and logic applied to the observation of nature. Pupin, nearing the end of his career, represented an old-guard of nineteenth century physicists for whom science and technology were one and the same. The mathematician George Birkhoff, writing the same year, but of a later generation than Pupin, recognized that the role of mathematics had expanded to the point where in his scientific hierarchy, he placed mathematics at the pinnacle. Mathematics, Birkhoff claimed, was the only objective part of the scientific project.[25]

The scientific establishment did not generally accept Birkhoff's position in 1929, and another mathematician, G.A. Miller challenged the aura of perfection around math. He wrote that while math was a valuable modeling system, progressions of logical analysis are only as strong as their foundational assertions. Since we have seen the foundational assertions of Euclidean geometry proven wrong, math was never, and should not ever be mistaken for truth.[26]

Nobel Laureate P.W. Bridgman agreed with Miller and his concerns with the over-emphasis on mathematics. A respected philosopher of science, Bridgman in 1930 wrote a clear and thoughtful analysis of both the position of natural sciences, and the direction in which science must head using the tools of mathematization and abstraction. At that time Bridgman was unable to fully commit to mathematical modeling. He wanted science to keep a contact with the real world by focusing on measurements of mathematical predictions. Bridgman was wary of the structural and social ramifications of developing a specialized mathematical language, and he also lamented the loss of logical deduction as a modern tool since axiomatic proofs had fallen out of modernist favor. In 1930, his

---

[25] M. I. Pupin, "The Meaning of Scientific Research," *Science*, New Series, 61, no. 1567 (January 9, 1925): 26–30; George D. Birkhoff, "The Origin, Nature and Influence of Relativity," *The Scientific Monthly* 19, no. 2 (1924): 180–87.
[26] G. A. Miller, "Mathematics and the Truth," *Science*, New Series, 70, no. 1825 (December 20, 1929): 608–9.

hybrid approach was idiosyncratic but not unique. He was fully able to come to terms with the advances in sub-atomic physics but warned against a wholesale loss of the enlightenment ideal.[27]

Physics was not the only field in which mathematics became the language of abstraction. Chemist Irving Langmuir in 1929 traced a clear line of influence between recent scientific discovery and the loss of confidence in the intellectual tools supplied by the enlightenment. His article was a call to all scientists to "pave the way for the coming revolutionary changes…we must be prepared to modify our methods of thinking, probably along lines now so prevalent in physics. But, above all, we must urge young chemists in the universities and after graduation to become thoroughly well trained in mathematics and in modern physics." Like Langmuir, Stanford pathologist W. H. Manwaring also urged his audience to dispense with what he called the "Victorian model" or "mechanical model" of science and instead implement a mathematical modeling system. Manwaring asserted that an abstracted mathematical model was not only more accurate than a mechanical (observational) model, but was predictive as well.[28]

Outside of physics, and to some degree chemistry, the mathematical language was not higher order math, but statistics. This phenomenon had begun with the laborious work of Darwin and his contemporary Mendel who demonstrated the substance of their theories with statistical analysis. By 1930, the concept of a statistic (a number symbolic of an observation of a group of objects) had finally taken root. The comparison of statistics and the probabilism of quantum theory convinced mathematician and physicist Warren Weaver that the twentieth century "should be known as the reign

---

[27] P. W. Bridgman, "Permanent Elements in the Flux of Present-Day Physics," *Science*, New Series, 71, no. 1828 (January 10, 1930): 19–23.
[28] Irving Langmuir, "Modern Concepts in Physics and Their Relation to Chemistry," *Science*, New Series, 70, no. 1817 (October 25, 1929): 385–96; W. H. Manwaring, "Biochemical Relativity," *Science*, New Series, 72, no. 1854 (July 11, 1930): 390.

of probability." University of Iowa president, and mathematician H.L. Rietz took a more balanced approach in 1931, but one that still promoted the mathematical abstraction of science. He wrote "Along with our reverence for the mechanistic view and its achievements, it seems appropriate to recognize its limitations, and to develop also an appreciation for the rationale as well as for the convenience of the statistical view."[29]

The study of natural selection and heredity were studies of relationships between objects. These intangible relationships became the focus of scientific investigation in modernist science. We can see the growing importance of relational observation in Langmuir's long discourse on what he refers to as "meaningless questions" which were essentially questions without validating comparisons; or questions not including relationships. Langmuir wrote that

> To ask whether an electron is a particle or a wave is a meaningless question; the same is true of the question whether light consists of corpuscles or waves. One must answer that both of these are particles or waves according to the kind of operations that we may perform in observing them. If we make an experiment which proves that an electron has a very definite position then it would seem to prove that it is a particle. In that case, however, according to the uncertainty principle, we are not able to determine accurately the velocity and therefore cannot predict where the particle will go.[30]

---

[29] Warren Weaver, "The Reign of Probability," *The Scientific Monthly* 31, no. 5 (November 1, 1930): 457–66; H. L. Rietz, "Some Remarks on Mathematical Statistics," *Science*, New Series, 74, no. 1908 (July 24, 1931): 86.

[30] Langmuir, "Modern Concepts in Physics and Their Relation to Chemistry," 391, 394. Langmuir, 391. A question such as "how fast does a bird fly?" has no logical meaning since time and space are relative so no answer is available without a context and a reference. A more meaningful phrasing would be "at what velocity does X bird fly in comparison to Y point on the earth." Again, it is a study of relationships has taken primacy in science.

By combining the concept of meaningless questions with Bohr's Principle of Complementarity[31] Langmuir dispensed with the argument that two correct descriptive theories could not exist for the same observation.[32]

By 1937, concept of abstracting science into the symbolic language of mathematics was so ingrained that historical treatments of the process began to appear. Physicist and president of the Massachusetts Institute of Technology, Karl Compton, gave credit to the electron for establishing once and for all the need for abstract mathematical modeling. He also credited the electron with rise of quantum physics, confronting issues of measurement and subjectivity, the separation of observation from knowledge, the exploration of the subatomic structure, accidental x-rays (by Roentgen), Heisenberg's uncertainty principle, the loss of causality, and the differentiation of pure and applied science. This may be a lot to ascribe to a the study of a single phenomenon, but the significance of the article is that Compton recognized that all of these characteristics of modernist science were present and embedded in undirected scientific practice by 1937.[33]

Applied Science

Part of the enlightenment scientific system was to conflate the realms of science and technology. This was in part a result of the enlightenment goal of applying science to the betterment of humankind. Social improvement was a staple of the public conception of scientific thought well into the

---

[31] In 1926, Bohr published a full exploration of this principle which effectively said that the behavior of objects in sub-atomic scales (and other very specialized circumstances) is too far from our experience to be able to develop effective mechanistic models. Instead, multiple model may be needed in which each works under specific conditions. An example is the wave particle theory of light.

[32] Cecil H. Desch, "Pure and Applied Science," *Science*, New Series, 74, no. 1925 (November 20, 1931): 495–502. Desch was an applied scientist and he summed up this logical argument against Heisenberg's uncertainty principle by declaring that all uncertainty in science is tantamount to failure and Heisenberg simply redefined failure as success.

[33] Compton, "The Electron: Its Intellectual and Social Significance."

1930's. However, the move to mathematical abstraction across all fields of science created a hierarchy within science which modified the science-technology relationship. Those scientists who moved in a mathematical, abstract, and subjective direction were able to control the discourse between themselves and those who remained predominantly in the enlightenment ideals of objectivity, observation, and causality. The distinction between 'pure' and 'applied' science[34] was not entirely a result of the compulsion toward modernism, but a mixture of competing influences within the scientific establishment including funding, the economy, national security, political influence, tradition, and the focus of traditional science on preserving the status quo. There are two themes evident in the journal articles of the period: pure science defending its primacy, and applied science resisting subjugation.[35]

In 1928, William Bragg, president of the British Association for the Advancement of Science, wrote of the interplay between craftsmanship (applied science) and science (the pure variety). He made it clear that the goal of science, ultimately was a practical one, and while there are differences in approach, "no clear line can be drawn between pure science and applied science: they are but two phases which meld into one another, and either loses virtue if dissociated from the other." This emphasis on science as a public good is a common theme after the role of science in the development of weapons for World War I.[36]

In 1927, P.L.K. Gross published a statistical study on the publication of chemistry papers before, during, and after WWI. His conclusions were that war has an immediate negative effect on research but

---

[34] Many sources refer to applied science and pure/basic/undirected/fundamental science. This is usually to discriminated between science practiced in an academic setting versus and industrial setting. It can also mean the different between practicing science with production of outcomes in mind versus science for the sake of discovery alone.

[35] Zunz, "Producers, Broker, and Users of Knowledge: The Institutional Matrix"; Charles E. Rosenberg, *No Other Gods* (Johns Hopkins University Press, 1978), 185–95.

[36] William Bragg, "Craftsmanship and Science," *Science*, New Series, 68, no. 1758 (1928): 213–23.

a very positive long-term effect, especially in the United States. Far from praising the effects of war, Gross concluded that American and European pure science were so reduced during the war, that no relationship existed between wartime and postwar chemistry. Ironically, with respect to the war, he quoted Calvin Coolidge who, in reference to *applied* science, said "Wherever we look the work of the chemist has raised the level of our civilization, and increased the productive capacity of our nation."[37]

There was disagreement among academics regarding the role of directed science within universities. In 1930, R.L. Sackett, the Dean of Engineering at Penn State, attempted to reestablish the primacy of technology and applied science by reiterating the long history of scientific invention; starting with the pyramids and ending with the steam engine of James Watt. Throughout, he stated that all these applications of science *were* science in its purest form: in the betterment of humankind.[38]

The theme of a return to social praxis came up regularly in science journals, but were authored predominantly by applied scientists. As late as 1937, professor of electrical engineering E. Weber declared that the mission of science was both "truly objective," and for the service of humanity. By 1938, applied scientists like R.C. Wallace (a research physician) were searching for justifications for accepting the discourse as applied by pure science. Wallace ultimately determined that for scientific roles which interact with humanity (as applied scientists are expected to do), the language of mathematics, the losses of causality, objectivity, and the acceptance of uncertainty are detrimental, and the traditional enlightenment scientific norms fit this role more comprehensively. He wrote of the pure scientist, "one contemplates with disquiet and apprehension the increasing stream of narrow specialists

[37] P. L. K. Gross, "Fundamental Science and War," *Science*, New Series, 66, no. 1722 (December 30, 1927): 640.
[38] R. L. Sackett, "A New Science," *Science*, New Series, 71, no. 1829 (January 17, 1930): 57–60.

who issue from the institutions of learning into a world that is seeking for other counsel than they can give."[39]

In this competition for the control of the discourse within science, pure scientists had the advantage of a specialized language, and had made cultural choices which had differentiated them from their applied colleagues. The influential sub-atomic physicist E. O. Lawrence in 1937 gave a speech praising the work and mission of applied science. Lawrence, as representative of the basic science realm, defined a space for applied science to dwell in. The speech both set the rules of discourse between these realms of science, and subjugated applied science to a lesser idealistic role; that of interpreting pure science for the use of the public. He did this deftly by employing great compliments to the field and to the relationship between pure and applied science. He called this relationship "an absorbing romance," and said,

> The applied sciences are devoted to perfecting instruments and means already available, a procedure which in some quarters has been regarded as the best attack on practical problems. On the other hand, the pure scientist is guided by curiosity to learn more about the facts of nature.[40]

The same year, in an attempt to rally chemists to political action, academic chemist Harold Urey wrote a similar speech which also defined the role of applied scientists, and of industrial science at large by praising the great influence of applied chemistry on everyday life. He patronized applied scientists by

---

[39] Harvey Cushing, "The Humanizing of Science," *Science*, New Series, 81, no. 2093 (February 8, 1935): 137–43; Dayton C. Miller, "The Spirit and Service of Science," *Science*, New Series, 84, no. 2179 (October 2, 1936): 297–304; Desch, "Pure and Applied Science"; E. Weber, "The International Mission of Science," *The Scientific Monthly* 44, no. 2 (February 1, 1937): 171–73; R. C. Wallace, "The Changing Values of Science," *Science*, New Series, 88, no. 2282 (1938): 271.
[40] Ernest O. Lawrence, "Science and Technology," *Science*, New Series, 86, no. 2231 (October 1, 1937): 295.

calling upon them to focus on developing products to better the human experience. He broadened this theme carried into fields beyond chemistry and physics into social sciences.[41]

The conversation about pure vs. applied science spilled over into social sciences as well. In 1936, anthropologist Melville Herskovits proposed a similar bifurcation of anthropology into both pure and applied forms. Since the field had not traditionally been tied to technology Melville took it upon himself to develop a series of opportunities for anthropologists who wanted to come "out of their ivory towers." He saw a need for both styles of anthropology because "the stress of the application of anthropology to the practical problems of the administration of primitive peoples that has such currency at the present time is a pragmatic problem in the extreme, and the anthropologist must be realist enough to see it as such."[42]

The difficult requirements of a higher math were an agent of change, but not the only agent. Many of the contributors to *Science* took time to emphasize the education of the newer generations of scientists, some emphasizing math, others the principles of scientific thought. But both applied and pure scientists had been deeply affected by the memory of science's complicity in the First World War, and potential for both harm and good that could come from both the enlightenment and modernist scientific projects. Scientists prepared for praxis, along a continuum; at one pole was traditional application, the other pole was the adoption of political action.

[41] Harold C. Urey, "Chemistry and the Future," *Science*, New Series, 88, no. 2276 (1938): 133–39.
[42] Melville J. Herskovits, "Applied Anthropology and the American Anthropologists," *Science*, New Series, 83, no. 2149 (March 6, 1936): 2161.

Causality and an Indeterminate Universe

The scientific community's intellectual commitment to causality remained in place until the 1930's when Heisenberg's uncertainty principle was expanded to deny causality. In its place came a general acceptance of an indeterminate universe where both random action and free will needed to be accounted for in the scientific mindset.

Dorothy Ross credited Mach (model theory), Henri Poincaré (non-Euclidean geometry and relativity), and Karl Pearson (mathematical scientific method) with destroying the "deterministic conception of scientific law that had undergirded both positivist science and historical progress." However, these men worked primarily in the late 19th century and it was not until the 1930's when the changes to the scientific process they proposed were addressed by the scientific establishment as a whole. The event which forced the issue was Werner Heisenberg's uncertainty principle (also translated as indeterminacy principle), which carried implications to foundational scientific thought far beyond its immediate application to electron motion. Philosopher John Dewey saw the principle as entirely invalidating scientific claims of absolute knowledge.[43]

With the traditional principles of scientific method at stake, a chain of debate within the pages of *Science* began in 1931 when Harvard physicist P.W. Bridgman wrote an extensive article both explaining in simpler language the meaning of Heisenberg's Principle, but also exploring its less obvious consequences. Bridgman stated that traditional cause and effect is not applicable at quantum scales; quantum mechanics loaned itself only to statistical analysis. Heisenberg's principle also indicated that

---

[43] John Dewey, *The Quest for Certainty: A Study of the Relation of Knowledge and Action,* Gifford Lectures ; 1929 (New York: Minton, Balch, 1929); Dorothy Ross, "Modernist Social Science in the Land of New/Old," in *Modernist Impulses in the Human Sciences, 1870-1930*, ed. Dorothy Ross (Johns Hopkins University Press, 1994), 174; James T. Kloppenberg, "Democracy and Disenchantment: From Weber and Dewey to Habermas and Rorty," in *Modernist Impulses in the Human Sciences, 1870-1930*, ed. Dorothy Ross (Johns Hopkins University Press, 1994), 75.

the universe contains true random action and may well be discrete since causality also infers continuity.[44] Bridgman hedged on the link to causality and took pains to clarify his disagreement with Dewey was limited to scientific inquiry, not matters of the human condition. He wrote:

> A word of warning may be interjected here. Many will be tempted to see a connection between the question of the predictability of the behavior of organic systems and those questions which have always exercised the human race, determinism and free will. It seems to me that there is no connection. The former is a question of physical fact, while the latter are predominantly questions of a subjective character which involve those emotional experiences which the subject goes through when on the point of making a decision.[45]

Physicists Charles Galton Darwin and Paul Heyl agreed with Bridgman in distancing science from what they saw as philosophy. Both confirmed that causality was not applicable to either quantum or human behavior. While the evidence of discontinuity as a fundamental principle of the universe was hailed in philosophical circles as evidence of free will instead of a religious or philosophical determinism, Darwin objected to this use of the uncertainty principle saying "The question is a philosophic one outside the region of thought of physics." Moreover he wrote that predictability was possible with "matter-in-bulk" so the theory did not "free us from determinism." Heyl agreed with both Darwin and

---

[44] Causality simply means that every effect has an underlying cause. In theory, these causes could be traced backward in a continuous manner to the birth of the universe. Continuity and causality are therefore linked, and if we know the rules of a particular effect, we can predict future actions of certain objects. Heisenberg conclusively demonstrated that at quantum scales, we are unable to know both the velocity and location of an electron simultaneously since the measuring of either will by definition change them. The behavior of any individual electron is therefore impossible to predict, and in turn both discrete (non-continuous) and non-causal. Instead, behavior at a quantum level can only be measured using probability applied to some number of electrons greater than a reliability threshold.
[45] P. W. Bridgman, "The Recent Change of Attitude toward the Law of Cause and Effect," *Science*, New Series, 73, no. 1899 (May 22, 1931): 546.

Bridgman and proposed that there was no clear way to scale-up the discrete physical properties of quantum world into the macro world we see around us. This was another example of a transition between traditional sciences looking to preserve an enlightenment perspective of macro-scale continuity in the face of modernist pressure to declare that the entire universe was discontinuous.[46]

MIT physicist Karl Compton immediately published a rebuttal to this logic by devising a thought experiment to allow a very simply upscaling of a random quantum event into a macro-scale event. In the spirit of protecting the epistemology of macro-scale continuity, prominent physicists Henry Margenau and W.A. Noyes lambasted Compton's suggestions in two letters to the editor. Margenau contended that Compton did not logically demonstrate free-will, only the creeping accumulation of acausal nature between quantum and macroscopic scales. Margenau, like Darwin, contended that this was philosophy, and not physics. Noyes argued that Compton simply misunderstood the difference between *indeterminable*, and *indeterminate*. Noyes did however, accept that there is a space in modernist science for philosophy.

> Darwin may be right when he says that the problem of "free will" "is a philosophic one outside the thought of physics," but such a statement depends on one's definition of physics and of science. It is true if we include in science only those things which are fully known and can be mathematically demonstrated. Science would be a very poor affair if it rigidly excluded all ideas for which this process is incomplete—indeed, is it possible to say of any fundamental idea that the process is complete? Science and philosophy, in their higher reaches, should be identical.[47]

---

[46] Charles Galton Darwin, "The Uncertainty Principle," *Science*, New Series, 73, no. 1903 (June 19, 1931): 660; Paul R. Heyl, "Cause or Chance?," *The Scientific Monthly* 34, no. 3 (March 1, 1932): 273–77.
[47] Arthur H. Compton, "The Uncertainty Principle and Free Will," *Science*, New Series, 74, no. 1911 (1931): 596.

Compton put his finger on the nub of the issue. With the loss of determinism, causality, the absolute nature of space and time, and objectivity, the epistemology behind the enlightenment scientific method was broken. Darwin et al. assumed that they could continue to base their scientific method on a macro-scaled causality, but the Noyes saw this as intellectually dishonest; a far greater interaction between modernist science and philosophy was needed to compensate for the ethical and epistemological void left by the loss of enlightenment scientific philosophy.

A New Scientific Method

With the loss of the philosophical pillars of support supplied by the enlightenment tradition, the scientific community moved to replace the previous norms which comprised traditional scientific method. An alternative method already existed in the work of Ernst Mach and Karl Pearson. While Mach paved the intellectual path to make abstract modeling and a focus on relationships between object and subject acceptable techniques in mathematical physics, it was his protégé Pearson who gave practical tools for a new scientific method.[48]

By 1935, the debate about free will had been subsumed by a larger debate about ontology. The loss of objectivism and the concomitant rise of relativism gave a greater freedom to the scientific community in both the practice of science and the determination of truth. In 1935 Edwin Wilson of the Harvard School of Public Health wrote an appeal for assistance in determining a system of proof which was applicable to sciences outside of physics. The response over the next few years was the

[48] Porter, “The Death of the Object: Fin de Siecle Philosophy of Physics,” 138, 149–51.

development of a probabilistic approach to science based on the tools published in Pearson's *The Grammar of Science*.[49]

Scientists were also aware that perception was social and cultural in nature. In 1937, W. V. Houston wrote a comprehensive 'state of the field' analysis. He suggested that even though it was broadly acknowledged that concrete truth was not achievable he criticized physicists for only paying lip service to positivism. He wrote: "I am inclined to believe that those most effectively active in physics to-day have the very naive view which I mentioned at the beginning. They tend to believe that there is a real world which can be discovered, and they propose to discover it."[50]

R.C. Wallace also acknowledged the strength of the enlightenment project as a cultural influence on modernist science writing about the application of probabilism to social sciences:

> Such a procedure may be useful in relating together the laws of the physical world, derived as statistical averages, with the laws of human behavior; but it does not strengthen the hands of the scientist who has been accustomed to work on the implicit assumption of rigid causality in the phenomena of nature.[51]

It is this very system of simultaneously holding and applying mutually exclusive intellectual systems that Dorothy Ross and Stuart Hughes say is characteristic of the modernist ethic. This allowed scientists to accept what Charles Galton Darwin had called the "slight fuzziness inherent in all the facts of the world." In 1938, George Birkhoff wrote that this quote by Darwin is indicative of a need for

---

[49] Edwin B. Wilson, "What Is a Proof?," *Science*, New Series, 81, no. 2103 (April 19, 1935): 371–73; Allan Ferguson, "Trends in Modern Physics," *Science*, New Series, 84, no. 2184 (November 6, 1936): 401–7. Excellent description of the rise of probabilism in the exploration of the electron. Ferguson notes that there are two key differentiations of modern physics from traditional physics. This first of these is the mathematical and quantitative nature of it, and the second is the rise of probabilism and the decline of causality.

[50] Houston, "The Philosophy of Physics," 419.

[51] Wallace, "The Changing Values of Science," 268.

scientific ‘faith’. Since observation was now secondary to mathematical proof, Birkhoff suggested we fundamentally have to believe that the object no longer exists in anything but the symbols of higher math. With Darwin's fuzziness also came a new scientific reasoning process which combined attributes of probabilism, induction, deduction, and intuition applied in systematic forms and appropriate times. Birkhoff believed this was enough structure to provide faith within the scientific community that finally abandoning the absolutes of the past would allow science a greater vista of truth.[52]

Other authors described Birkhoff’s ‘faith’ in different ways, but the concept demonstrated a collective recognition that by late in the decade the scientific method had fundamentally changed. It also generated an intellectual elitism demonstrated by the bifurcation of pure and applied realms, and of social praxis. By the end of the 1930’s, scientists had determined that the world needed their ‘scientific spirit’.[53]

Praxis and the Decline of the Neutrality Ideal

> During the dark days of the World War, I once spoke to a distinguished scientist of some major event in the course of the war and he looked up from his work and said sharply, "What war?"
>
> -E. G. Conklin[54]

The publicly perceived complicity of the scientific establishment with the war machines of World War I was antithetical to the humanism of the enlightenment. This did not mean, however, that

---

[52] Ross, *Modernist Impulses in the Human Sciences, 1870-1930*, 5–6, 9; Charles Galton Darwin, “Logic and Probability in Physics,” *Science*, New Series, 88, no. 2277 (1938): 159; George D. Birkhoff, “Intuition, Reason and Faith in Science,” *Science*, New Series, 88, no. 2296 (December 30, 1938): 609.
[53] Wallace, “The Changing Values of Science.”
[54] Edwin Grant Conklin, “Science and Ethics,” *Science*, New Series, 86, no. 2244 (December 31, 1937): 595.

insulation between science and politics was characteristic of modernist science specifically; instead it was a byproduct of the enlightenment conflation of objectivity and disinterest. It was also a result of an academic research system which supplied no advantage for political or social praxis. To further complicate matters the loss of objectivity in science, and its concomitant ideal of scientific neutrality, emphasized a relativism for which a solid ethical foundation had not yet been developed. This was a dilemma in the modernist movement since the humanist aspect of enlightenment science remained generally popular among scientists but conflicted with the differentiating between applied and pure science. The solution was twofold. First, the modernists encouraged the social sciences and applied sciences to devote *their* time to addressing social need; then as individuals, pure modernist scientists took personal political action. This again differentiated them from the enlightenment scientists who strove for the betterment of humanity, but also for political neutrality.[55]

Historian Sandra Harding recognized two definitions of politics involved with the science of the 1930's. The first of these was defining politics as the overt influence of outside interest groups bending science to their ends. As an example, the Nazis of the 1930's sought control over the scientific establishment, its research directions, and ultimately its results by replacing the administrators in scientific institutions with party loyalists. However, there is an inversion of influence as political institutions were then scientized by the nature and language of the scientific establishment.[56]

---

[55] Alan D. Beyerchen, *Scientists Under Hitler: Politics and the Physics Community in the Third Reich* (Yale University Press, 1981), 209–10; Ross, *Modernist Impulses in the Human Sciences, 1870-1930*, 2; Ferguson, "Trends in Modern Physics," 401–7. Enlightenment science supported the status quo and encouraged an insulation between it and society's immediate needs.

[56] Harding, "After the Neutrality Ideal: Science, Politics and 'Strong Objectivity,'" 81–83.

Historian Robert Procter said that these are simply reverse concepts of each other. The Nazis politicized science through direct influence of the scientific project, but in turn depoliticized it by scientizing political and social institutions. He wrote:

> The Nazis depoliticized problems of vital human interest by reducing these to scientific or medical problems, conceived in the narrow, reductionist sense of the terms. The Nazis depoliticized questions of crime, poverty, and sexual or political deviance by casting them in surgical or otherwise medical (and seemingly apolitical) terms.[57]

Modernist scientists sought the scientization of the political endeavor. From within the pure scientific community, a political praxis developed under the pressure of worldwide conflict and the clouds of an imminent World War II. Academic scientists had already rejected the concept of applied science as not part of their purview, so in 1937, Robert S. Milliken clarified how academic scientists were going to engage the world at large. In *Science and the Scientific Attitude,* he defined the mindset with which modernist scientists go about their research. He suggested that this mindset must be applied to all sciences and forms of human endeavor in which thinking is involved. He also suggested that the meritocratic nature of science was similar to the goals of political democracy; a stance Robert Merton took five years later when he defined his norms.[58]

This ideal was supported in journals and expanded to encourage all scientists (who had expertise in scientific thought) to engage in "the promotion of international amity", ethics, politics, education, social sciences, philosophy, and economics.[59]

---

[57] Robert Proctor, *Racial Hygiene: Medicine under the Nazis* (Harvard University Press, 1988), 290.
[58] Robert S. Milliken, "Science and the Scientific Attitude," *Science*, New Series, 86, no. 2221 (July 23, 1937): 65–68; Merton, "Science and Technology in a Democratic Order."
[59] Birkhoff, "Intuition, Reason and Faith in Science," 609.

On the eve of WWII Richard Gregory, editor of *Nature*, wrote:

> Men of science can no longer stand aside from the social and political questions involved in the structure which has been built up from the materials provided by them, and which their discoveries may be used to destroy… Both rightly and wrongly, science has been blamed for much of the wastage of life which has been brought about by the rapid applications of scientific knowledge to purposes of peace and of war. Men of science…can no longer remain indifferent to the social consequences of discovery and invention. [60]

Despite the calls for social praxis, the brief moment between the establishment of it as a characteristic of modernist science, and the wholesale disruption of the scientific project by WWII, did not give the movement time to take root. There were several letters to Science condemning Nazi science and the treatment of Jewish scientists over the course of the decade, but only one scientist actually recorded an example of his applied praxis in the journal *Science.* P. W. Bridgman, who had been at the forefront of the modernist movement within science, publish his *Manifesto.* In it, Bridgman officially closed his laboratory to all scientists from all totalitarian states. By doing so, he engaged with society at large, but at the risk of damaging the façade of neutrality that enlightenment science had so carefully constructed. He wrote:

> Science has been rightly recognized as probably the one human activity which knows no nationalisms; for this reason it has been a potent factor making for universal

---

[60] Richard Gregory, "Science, Religion and Social Ethics," *Science*, New Series, 89, no. 2304 (February 24, 1939): 166, 163–66; Birkhoff, "Intuition, Reason and Faith in Science"; Wesley C. Mitchell, "Science and the State of Mind," *Science*, New Series, 89, no. 2297 (January 6, 1939): 1–4; Edward L. Thorndike, "Science and Values," *Science*, New Series, 83, no. 2140 (January 3, 1936): 1–8; V. F. Lenzen, "Philosophy of Physics," *Science*, New Series, 85, no. 2216 (June 18, 1937): 583; Wallace, "The Changing Values of Science"; Milliken, "Science and the Scientific Attitude."

> civilization. Action such as this is therefore to be deeply deplored and to be undertaken only after the gravest consideration. But it seems to me that the possibility of an idealistic conception of the present function of science has been already destroyed, and the stark issues of self-survival are, being forced upon us…Here I think is one of the few conceivable situations in which the popular conception of the social "responsibility" of "science" can touch at all closely the individual scientist.[61]

This a far cry from Conklin's anecdote from the First World War. The following month Columbia's Douglas Johnson published a letter chastising Bridgman, Johnson's long argument is summarized as: "it is wrong to engage in political discourse as a scientist; politics should remain outside the halls of academe". Regardless of the merit of either Bridgman or Johnson, the fact that this kind of debate over praxis was held in the pages of *Science* again demonstrated how much the character of science had changed in the course of the 1930's, and how praxis was now a characteristic difference between modernist and enlightenment science.[62]

The move toward social praxis was paused in the context of Big Science and WWII. Prior to the war, there were few examples of Big Science in the academic realm. E.O. Lawrence's cyclotron at its peak in the late 1930's employed more than 85 scientists and technicians. Modest by today's standards, this was by far the largest pre-war laboratory in the United States. During the war, however, Lawrence's work on the Manhattan project swelled his lab to more than 1200 scientists and technicians. Other national laboratories, such as Oak Ridge in Tennessee, Los Alamos in New Mexico, and Argonne in Illinois were similarly engorged by the wartime nuclear project. Enormous resources were poured into the Manhattan Project and its success in developing nuclear weapons demonstrated a need for the

---

[61] P. W. Bridgman, "'Manifesto' by a Physicist," *Science*, New Series, 89, no. 2304 (February 24, 1939): 179.
[62] Douglas Johnson, "The Manifesto by a Physicist," *Science*, New Series, 89, no. 2307 (March 17, 1939): 246–48.

United States to maintain large-scale science under the guise of a national defense measure. In these environments, there was no opportunity for individual scientists to practice personal social praxis.

Conclusion

The same fundamental influences which pushed art movements toward modernism in the 19th century also applied to normal science. The scientific project had a greater structural interdependence, and a culture promoting the status quo, was thus slower to implement a modernist ethic than literature, art, or music. Nevertheless, the scientific issues of late nineteenth century and early twentieth century science were inexplicable, inconceivable, and incommunicable in the language of enlightenment science, and a new language was needed. This language did not take manifest in scientific practice until the 1930's.

The new norms of individual practice took preeminence over the previous norms. In place of objectivity came a mathematical system of abstract models. When a scientific field was not as mathematically suited as physics or chemistry, statistical probabilism was employed. With no object remaining, the subject became the focus. The concept of truth became a coherence-based form of epistemological relativism instead of the traditional concrete goal. The modernist scientific aesthetic gave strength to applied science in contention with academic science. These were differentiated primarily in the use of higher mathematics, and presumed prestige. Finally, the walls between science, philosophy, and praxis crumbled in the decade before the Second World War with the government's Big Science projects.

This chapter described the context of intellectual norms used in science leading into the Second World War. The establishment of Big Science as a normal part of the overall scientific endeavor also required new norms to govern good management of science. These were grounded in the individual modernist norms which developed through the 1930's but were more practical in nature. The next chapter reviews the existing literature on these topics and grounds this dissertation in theory.

Chapter 2

Literature Review

This chapter is in two parts. The first is a literature review to examine the current state of the field. This includes an examination of Cold War science, the place of aerospace in regional and national context, and the challenges facing industrial science before and after WWII. The second half of the chapter is dedicated to examining three models of scientific progress: The Linear Model, The Mode 1/Mode 2 Model, and the Triple-Helix Model.

The literature regarding the management of science during the early aerospace industry between 1950 and 1970, and more specifically the period of Lockheed's transition from aeronautics into aerospace, is sparse and the subject is insufficiently studied. As such, I have had to take an oblique path to describe how this topic is currently viewed by historians and sociologists of science and technology. The first half of this literature review describes this oblique approach. There are thorough analyses available in the science of the Cold War and its related fields of historical inquiry: governmental science policy and its development after World War II, the growth of the military-industrial complex, the normalization of Big Science, and the role of universities as public research institutions. The role of universities is also examined in the literature as part of a body of work examining how science was practiced in universities in conjunction with both industry and government. More recent work has shifted away from institutional history towards an examination of the space the aerospace industry occupied physically, intellectually, and societally.

These topics represent the strongest and most academic historical work. Unfortunately, there are also some systemic historiological issues in this field. There is a body of popular history which describes the development of individual institutions and products, but these have limited analytical content and are predominantly descriptive. Another issue is what is missing from the literature. There few historical analyses of social issues surrounding gender and race in aerospace. Similarly, a surprising gap exists in the historical narrative regarding the history of science conducted in military labs. The final issue is the bias toward the university perspective when examining the history of science. There are several reasons for this bias, and they are visible in the critiques historians have levelled against the universities for partnering with industry and government in their scientific endeavors.

Some of the tropes which influence historians of science toward the academic bias are bound up in the traditional Linear Model of knowledge production. An examination of the literature analyzing the Linear Model begins the second half of this literature review. This is followed by examinations of the Mode 1/Mode 2 model and the Triple-Helix model.

Macro-Scale Science Policy in the Post-War Period

A theme in the literature is that the overall academic scientific endeavor changed in many ways in the period around WWII. The Post-War era brought to colleges Cold War cultural issues, Big Science, the normalization of secrecy, and entrepreneurial capitalistic opportunities for academic scientists (and entire colleges) to trade some of their academic freedom for financial resources. The historical questions addressed in the literature revolve around which of these influences was the single greatest, and how these influences interacted with each other.

Historian of science Paul Forman identified the influence of WWII as the genitive impulse starting the gradual transition of primacy of applied science over basic science. Forman wrote that "Between 1940 and 1945 the convergence of science with engineering that characterizes our contemporary world was effectively launched in its primarily military direction with the mobilization of U.S. scientists, most especially physicists, by the Manhattan Project and by the OSRD, the Office of Scientific Research and Development." The OSRD's chief, Vannevar Bush, recognized that the independence of academic research was threatened by industrial, military, and political influence after

WWII. With what Forman called Bush's "elitist allies," Bush developed a Post-War scientific policy which give academic science a choice in how much external influence it was willing to accept. A key part of this policy was the establishment of the National Science Foundation and its management by civilian scientists. [63]

Unlike Forman, Historians Peter Galison, Bruce Hevly, and Rebecca Lowen, placed the roots of change in the prewar period. In their study of the Stanford Linear Accelerator (SLAC) they proposed that WWII merely interrupted, and then accelerated, a course of events which had begun with Ernest Lawrence's cyclotron and led to Big Science on campus. They suggested that the plan for Big Science at Stanford was in the mind of provost Frederick Terman not at the end of the war, but at the start of it. However, they did agree with Forman that the primary policy concern at the time was who was to direct the research: academia, industry, or government. Historian Robert Seidel agreed with Galison et al. that the impetus for Big Science began with Lawrence in the prewar period.[64]

However, Seidel also agreed with Forman that in the Post-War period there was a conflation of science and technology. He wrote about MIT's Radiation Laboratory that "As the Radiation Laboratory grew, the distinction between science and technology became harder to draw. If operationally defined, that is, as what scientists do, the Big Science of the Radiation Laboratory progressively became a cyclotron technology." He went on to suggest that this combination of disciplines produced not just knowledge, but an entirely new field of study – nuclear science. W. K. H. Panofsky also agreed that the

---

[63] Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *Historical Studies in the Physical and Biological Sciences* 18, no. 1 (1987): 152; Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," March 2007.
[64] Peter Galison, Bruce William Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935-1962," in *Big Science : The Growth of Large-Scale Research*, ed. Bruce William. Hevly and Peter Galison (Stanford, Calif. : Stanford University Press, 1992), 47. See also Peter Galison and Bruce William Hevly, *Big Science: The Growth of Large-Scale Research* (Stanford University Press, 1992), 8.

change in science occurred before the war, but he proposed that it was based in the partnerships between industry and science where industry was funding "small science" by which he meant modestly sized labs performing contract research. [65]

Another area of agreement between Forman and Seidel was that a defining characteristic of the new Post-War scientific endeavor was a conflation of applied and basic sciences. This occurred at all scales from the institutional to national scale (macro-scale), the project or lab scale (meso-scale), and the individual scale (micro-scale). Seidel even traced five key areas in which universities had to change systems to cope with the applied orientation of Big Science: 1) recognition of individual contributors in large projects, 2) that the tenure clock should be delayed for young faculty working on Big Science, 3) adjusting thesis standards to cope with a portion of a team project instead of individual work, 4) absenteeism from regular collegiate duties subject to the needs of the project, and 5) the quality of the colleges support facilities which often needed expansion for large and complex projects.

Stuart Leslie, perhaps the most prolific examiner of the relationship between industrial and academic science, suggested that "the obvious military orientation and application potential of nearly all of Stanford's electronics work rapidly eroded any significant separation between the "fundamental" and "applied" research." Similarly, Stanford's approach on Big Science projects eroded the barriers between disciplines and "brought together most of Stanford's best science and engineering, electrical, chemical,

[65] Bruce William Hevly, "Reflections on Big Science and Big History," in *Big Science : The Growth of Large-Scale Research*, ed. Bruce William. Hevly and Peter Galison (Stanford, Calif. : Stanford University Press, 1992), 31; W. K. H. Panofsky, "SLAC and Big Science: Stanford University," in *Big Science : The Growth of Large-Scale Research*, ed. Bruce William. Hevly and Peter Galison (Stanford, Calif. : Stanford University Press, 1992), 145.

and aeronautical engineering, chemistry, physics, and the interdepartmental laboratories at the initiative of and funding from the Department of Defense." [66]

When Leslie approached the question of who should direct Big Science in an academic setting, he tended to be sympathetic to Vannevar Bush's ideal of keeping direction in the hands of academic scientists. During the Cold War this impulse was very strong within universities. At Stanford, Leslie wrote that the faculty "worried openly about the long-range consequences for the department of cooperative research of the kind pioneered on the klystron and at Sperry. They were concerned that such a program would skew the department toward engineering problems and corporate interests."[67]

While these concerns were common at the national science policy (macro) scale, and at the faculty research (micro) scale, there was a middle, or meso scale, where science management welcomed funding from the industry and the military. Rebecca Slayton suggested that the administrators of these universities, particularly Julius Stratton at MIT and Frederick Terman at Stanford, played a high risk game "seeking to use military funding to gain autonomy and control over relations with industry during and after WWII." Ultimately, these colleges wanted a complimentary, not competitive relationship with industry, but wanted even more to control their own research.[68]

More recently, academic historians have moved away from the premise that political, military, or industrial direction in academic science has negative consequences. Forman, who took a more traditional approach to the topic, condemned the influence of military and industrial resources in

---

[66] Stuart W. Leslie, "Playing the Education Game to Win: The Military and Interdisciplinary Research at Stanford," *Historical Studies in the Physical and Biological Sciences* 18, no. 1 (1987): 71. Leslie, 84. See Also Panofsky, "SLAC and Big Science: Stanford University," 145; Stuart W. Leslie, *The Cold War and American Science : The Military-Industrial-Academic Complex at MIT and Stanford* (New York : Columbia University Press, 1993), 61.
[67] Leslie, "Playing the Education Game to Win: The Military and Interdisciplinary Research at Stanford," 63.
[68] Rebecca Slayton, "From a 'Dead Albatross' to Lincoln Labs: Applied Research and the Making of a Normal Cold War University," *Historical Studies in the Natural Sciences* 42, no. 4 (2012): 11, 258, 260.

academic science. He focused on the negative aspects of collaboration and wrote of the academic physicists, “though they have maintained the illusion of autonomy with pertinacity, the physicists had lost control of their discipline. They were now far more used by than using American society, far more exploited by than exploiting the new forms and terms of their social integration.” Panofsky disagreed with Forman and wrote that the recent popularity of "big-science bashing" was unfair because "we simply do not know how to obtain information on the most minute structure of matter (high-energy physics), on the grandest scale of the universe (astronomy and cosmology), or on statistically elusive results (systematic genetics) without large efforts and large tools.” Hevly lays the condemnation of Big Science at the feet of historians who have written poor institutional histories, and institutional histories in general. He wrote

> Whereas past studies of Big Science typically counted dollars and personnel, and tabulated the funding sources that nourished large-scale research, we can now see more of the causes and consequences of the growth of science. Those interested in the influence of research sponsors - industrial, military, and eleemosynary - cannot simply assume that the outside financial support subverts research in some pure, ideal state.[69]

The Cold War was also identified as an integral influence on the change in the scientific endeavor. Historian Margaret O’Mara argued that WWII and the early Cold War emphasis on nuclear weapons opened academia to Big Science, but it was the launch of Sputnik in 1957 which reestablished the importance of basic research in both academia and industry. The launch was a stunning moment for the United States. It both shocked and horrified the country by demonstrating how far behind the USSR

[69] Forman, “Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960,” 229; Panofsky, “SLAC and Big Science: Stanford University,” 145; Hevly, “Reflections on Big Science and Big History,” 357.

the United States was in the space race. O'Mara wrote that in the post-Sputnik world, "basic research won new respect and political support…More than ever before, national leaders saw scientific excellence as key to winning the Cold War."[70]

Historian Zuoyue Wang proposed a similar thesis to O'Mara that Sputnik's launch galvanized American science but he suggested it prompted an emphasis on technology, not basic science. The President's Science Advisory Committee (PSAC), founded by Eisenhower and comprised of accomplished scientists from several fields, played an important role in defining the potential limits of scientific endeavor. Wang wrote that…

> to accomplish their dual goal of controlling the arms race and promoting basic research, PSAC, following a long tradition of American public scientists, engaged in what the sociologists of science called "boundary work": they negotiated the boundary between science and technology (or between basic and applied research), and that between expertise and politics or policy.

According to Wang, it was this group more than any other was responsible for the tremendous investment in Cold War science.[71]

Jessica Wang in *American Science in an Age of Anxiety* suggested that while Sputnik may have had a later influence on science funding, the tremendous social and political influence of the Cold War and its concurrent anticommunism dominated the direction of science at a personal and intellectual

---

70 Margaret Pugh O'Mara, *Cities of Knowledge : Cold War Science and the Search for the next Silicon Valley*, Politics and Society in Twentieth-Century America; Politics and Society in Twentieth-Century America. (Princeton, N.J. : Princeton University Press, 2005), 47.

71 Zuoyue Wang, *In Sputnik's Shadow : The President's Science Advisory Committee and Cold War America* (New Brunswick, N.J. : Rutgers University Press, 2008), 8.

level. Science came of age during WWII but toward the end of the war "scientists turned away from a progressive left rhetorical style that emphasized fundamental civil libertarian principles." It was this transition which concerned and influenced Vannevar Bush and his colleagues Robert Oppenheimer and James Conant (the president of Harvard), to press for civilian control of the National Science Foundation. Wang was one of the few historians to examine the social history of scientists in this era, although her analysis crosses regularly into the large-scale arguments surrounding national science policy in the 1950's.[72]

Human Resources, Industrial Science, and the University

I have already touched on some of the challenges that universities faced with Big Science moving into the academy during and after WWII. But many research universities had long-standing financial relationships with industrial firms like Lockheed, Du Pont, and Bell Labs. One of the drivers for these relationships was the scarcity of scientists and engineers in the immediate post war period. Partnerships between industry, academia, and government meant that these human resources could be shared and as an added benefit, partnerships could be used as a system of communication in fields where secrecy was normal. Forman identified the shortage of scientists and engineers as characteristic of the Post-War period. He proposed that demand for researchers outweighed supply, and this became the limiting factor in industrial science. The money was there, but the people were not. He wrote that "From 1949 to 1955 the number of ads for industrial scientists in the pages of the New York Times increased by a factor of ten, and by another factor of two by 1959. As throughout the war, so in the

[72] Jessica Wang, *American Science in an Age of Anxiety : Scientists, Anticommunism, and the Cold War* (Chapel Hill, NC : University of North Carolina Press, 1999), 8.

decade following, the principal preoccupation of those responsible for managing the expansion of military R&D was where to find the necessary qualified men."[73]

This positioned the three key stakeholders, industry, academia, and government, in direct competition for researchers. Academia used inculcation as a strategy to keep the brightest students. MIT, whose Radio Lab is an exhaustively explored example of academic Big Science, implemented a strategy of keeping its own best and brightest. Leslie wrote that at MIT "scores of future professors who received their graduate instruction in these laboratories later taught their own students with textbooks written by their former professors." Leslie went on to propose that the government recognized that while universities were a relatively small expense compared to military research, "only the universities could both create and replicate knowledge, and in the process, train the next generation of scientists and engineers. The universities provided most of the basic research and all the manpower for the defense industry." As such, the universities were seen by the government as vital to the defense effort as the country's sole supplier of scientists.[74]

Industry, on the other hand, struggled to find enough researchers throughout the post-war period and had to develop several strategies to compete with academia and government labs.[75] Panofsky's examination of Du Pont's wartime construction of the massive Hanford Engineering Works in Washington state showed that aerospace learned from its mistakes. Hanford was a cutting-edge nuclear research and engineering facility designed to spearhead America's nuclear science project in the 20th century. But after the war, Du Pont could not find nuclear scientists to staff it. The academic physicists

---

[73] Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," 166.
[74] Stuart W. Leslie, "Profit and Loss: The Military and MIT in the Postwar Era," *Historical Studies in the Physical and Biological Sciences* 21, no. 1 (1990): 69; Leslie, *The Cold War and American Science : The Military-Industrial-Academic Complex at MIT and Stanford*, 2.
[75] See Chapter 3. Industry's greatest asset was financial. They could pay more than academic to government jobs.

feared that it was a step down in prestige, a change in lifestyle, and a move to an ugly industrial complex in the middle of rural Washington. The issue, according to Panofsky, was that immediately after the war, physicists wanted Big Science, which involved big technology, but did not recognize that it had traditionally, and successfully, been performed by industrial companies like Du Pont. He wrote that "The bleak prospect for being able to recruit physicists was one of important reasons why Du Pont opted to stay out of the nuclear energy field in the post-war era."[76]

Amateur historian and former Lockheed executive Sherm Mullin identified an industrial strategy to combat Du Pont's undesirable location in HR recruitment – move the company to where the scientists are. When the Lockheed Missile and Space Company (LMSC) began in Van Nuys, and struggled to recruit researchers, Lockheed moved the company to Sunnyvale in Northern California so it could share researchers with Stanford and NACA (the predecessor to NASA). Mullin described the example of Nicholas J. Hoff, a professor at the Polytechnic Institute of Brooklyn. Hoff initially declined Lockheed's offer of employment, so Lockheed offered him a position at Stanford where they would pay his full salary if he consulted for Lockheed part-time. Both Hoff and Stanford readily accepted. Kargon, Leslie, and Schoenburger suggested that Lockheed had no other choice but to innovate with human resources since as airframe building transitioned into a knowledge industry, they were terribly understaffed. In this case, it was Lockheed who drove the relationship with Stanford and the university "was demonstrably not leading but responding directly to industry requirements."[77]

---

[76] Panofsky, "SLAC and Big Science: Stanford University," 250–51.

[77] Sherman N. Mullin, "Robert E. Gross and the Rise of Lockheed: The Creative Tension Between Engineering and Finance," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 127–58; Robert H. Kargon, Stuart W. Leslie, and Erica J. Schoenberger, "Far Beyond Big Science: Science Regions and the Organization of Research and Development," in *Big Science : The Growth of Large-Scale Research*, ed. Bruce William. Hevly and Peter Galison (Stanford, Calif. : Stanford University Press, 1992), 353, 347; Glenn E. Burgos, "Aerospance in the Rest of California: The View from Silicon Valley," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 238.

Leslie also identified another strategy of industry to attract researchers, and that was to engage in the founding of new university departments which had sprung from the interdisciplinary approach of Big Science: nuclear science, solid state electronics, computer science, and materials science. Leslie wrote that in the most extreme cases industry even helped found new universities. Gifted recruiter and nuclear scientist Frederic de Hoffmann struggled to bring nuclear scientists to San Diego to work at the General Dynamics subsidiary General Atomics. In partnership with Robert Sproul at the University of California, de Hoffmann drove the founding of U.C. San Diego three miles away from General Atomics headquarters in Torrey Pines, California. Like Stanford and Lockheed, UC San Diego and General Atomics shared researchers.[78]

The third response to the human resources problem was to promote the study of basic science to appeal to scientists who were leery of applied science. Historian Steven Shapin demonstrated that despite the stereotypes surrounding industrial science, by the 1950's industrial scientists had many of the freedoms of academic scientists and many freedoms not found in the university. Shapin pointed to the basic science successes performed at Eastman-Kodak, Dow Chemicals, GE, RAND, and Bell Labs. Individual scientists in these companies felt that they were making equal contributions to science to academic researchers, and "by the end of the 1950's Bell Labs had the Nobel Prizes to support their claims." Other companies also engaged in basic research. The Radio Corporation of America, used this approach to recruiting and was able to keep its federal funding without external directional pressure provided that all its innovations were shared royalty-free with domestic competitors. The explicit goal at

[78] Stuart L. Leslie, "Spaces for the Space Age: William Pereira's Aerospace Modernism," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 15–36.

National Atomic was to “enable these senior men to delve into basic science, give some of their time to defense problems, and also to train others.” [79]

The Place of Aerospace

In the late 1990’s and throughout the 2000’s, historians in aerospace field began to look beyond large-scale institutional histories, and instead focus on the industry’s interaction with its environment. At the macro level, there were several studies of why and how aerospace found a home in Southern California, the diffusion of aerospace norms into local society, and the legacy left behind after aerospace moved away. Historians of place and space, examined the work and living environments of aerospace researchers, and how the norms of the aerospace industry affected the thought patterns of researchers in social life.

The aircraft industry was attracted to Southern California very early on. Historian Peter Westwick suggested this was for reasons of weather (good flying conditions year-round), safety (large open spaces allowed for safer accidents), and local boosters who recognized that choices for national-scale industry were limited by Southern California’s relatively remote location. In Southern California in the early nineteenth century, the cost of transnational freight was high, so boosters looked for industries which had low shipping costs like the Hollywood film industry, the aircraft industry (whose products could deliver themselves), and ultimately research-based knowledge industries. Boosterism was also a factor in the move of aerospace companies to San Diego, and the Bay Area. The accessibility

---

[79] Christophe Lécuyer, David C. Brock, and Jay Last, *Makers of the Microchip: A Documentary History of Fairchild Semiconductor* (MIT Press, 2010), 9; Steven Shapin, “Who Is the Industrial Scientist,” in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 345; Ann Johnson, “What If We Wrote the History of Science from the Perspective of Applied Science?,” ed. Christophe Lécuyer et al., *Historical Studies in the Natural Sciences* 38, no. 4 (2008): 610–20; Leslie, “Spaces for the Space Age: William Pereira’s Aerospace Modernism,” 140.

of military testing sites and colleges (especially UCLA and Caltech) also contributed to the industry spending its formative years in Southern California. Leslie, Mullin, and Westwick agree that this transition from aeronautics to aerospace also corresponded with a shift towards a knowledge industry where innovation carried more value that physical products. As Leslie pointed out when discussing Convair, “Like most laboratories, Convair Astronautics primary product turned out to be paper. As one executive astutely observed, "the product of this plant is isn't hardware, it's knowledge. The essential template for future success.”[80]

Unfortunately, there were less beneficial aspects of the aerospace industry settling in Southern California. Wade Graham and D. J. Waldie showed that one of the most obvious aspects of aerospace growth was suburban crawl. The city of Lakewood in South Central Los Angeles, for example, began as 17,500 homes built by Lockheed to house factory workers for their Long Beach plant. However, Lakewood only contained homes surrounding a shopping center; “other parts of urban life were absent: there were no cultural institutions and no cemetery, and churches and synagogues had to be improvised at first. There was a nowhereness, a randomness of place in the new suburbs.” Both Graham and Lockheed executive Ben Rich also explain that another environmental legacy is some of the worst industrial pollution in the country with three aerospace industry contaminated superfund sites.[81]

---

[80] Wade Graham, “Blueprinting the Regional City: The Urban and Environmental Legacies of the Air Industry in Southern California,” in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 140–41; Leslie, “Spaces for the Space Age: William Pereira’s Aerospace Modernism,” 135; Stuart W. Leslie and Robert H. Kargon, “Selling Silicon Valley: Frederick Terman’s Model for Regional Advantage,” *The Business History Review* 70, no. 4 (1996): 435–72; Christophe Lécuyer, *Making Silicon Valley: Innovation and the Growth of High Tech, 1930-1970* (Chemical Heritage Foundation, 2006).

[81] Graham, “Blueprinting the Regional City: The Urban and Environmental Legacies of the Air Industry in Southern California,” 257; D.J. Waldie, “Lost in Aerospace,” in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 37–44; Ben R. Rich and Leo Janos, *Skunk Works: A Personal Memoir of My Years at Lockheed*, 1st edition (Boston: Back Bay Books, 1996).

Another macro-scale topic on place and space was the design of research facilities. To give themselves another edge in hiring and retaining researchers, Leslie wrote that Southern California's aerospace industry had to project the right image. Unlike Du Pont's vision for a nuclear science center which looked like a massive military base, aerospace firms hired renowned modernist architects like William Pereira (Convair, General Atomic, Lockheed Rye Canyon, Autonetics), Harris Armstrong (McDonnell), and Walter Netsch (Air Force Academy). These designs were usually only facades and the scientific workspaces were relatively modest. Nevertheless, this space-age architecture characterized the industry in Southern California both in the minds of the scientists recruited to work there, and in the imagination of the public.[82]

The place of aerospace within the minds of scientists and of society also encouraged the diffusion of ideas. Westwick showed that technologies travelled from their origination in aerospace to cultural institutions as far afield as Hollywood films and surfing. Mihir Pandya also suggested that secrecy also diffused. It was such an inherent part of aerospace that it fractured and compartmentalized the traditional communications practices of both scientists and their communities. He suggested that in Southern California, stealth was not just a technology, but a way of life, and that "Secrecy fostered two cities laminated to each other, one seen and the other unseen."[83]

---

[82] Leslie, "Spaces for the Space Age: William Pereira's Aerospace Modernism"; Mihir Pandya, "The Vanishing Act: Stealth Airplanes and Cold War Southern California," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 105–26.

[83] Pandya, "The Vanishing Act: Stealth Airplanes and Cold War Southern California"; Peter J. Westwick, "Aerospace and Hollywood: How Computer Animationwent from Space to Cineplex," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 159–70; Peter J. Westwick, *The World in the Curl: An Unconventional History of Surfing*, First edition. (New York : Crown Publishers, 2013).

Missing Elements

A great deal of potentially valuable scholarship on aerospace is still to be completed. While M. G. Lord and Zuoyue Wang have made considerable effort to document the experience of women and Asian-Americans respectively, extensive efforts examining how minorities were involved or affected by aerospace do not yet exist. Additionally, there is a canon of popular histories which are useful to pinpoint dates and events, but not as useful for gaining insight into the field. They can be hagiographical in nature (Boyne) or carry a strong political bent (Hartung). There are also several which are aimed at military technology readers and trace the history of specific products (Suhler). The final missing piece, as identified by Pandaya, Hevly and Thomas Lassman is the narrative of the military labs, although Pandaya suggests this is a direct result of the culture of secrecy build around them.[84]

While the historical study of the management of science within the early aerospace industry does not yet have a rich literature, there is enough analysis surrounding the topic to aid in our understanding. Big Science became a norm in research universities after WWII and within its bounds the distinctions between applied and basic science lost meaning. The national scientific endeavor became institutionalized knowledge production. In practice this made industrial and academic science outcomes differed only in the location of the research; however, there was still a relatively clear line between academic science and industrial science in that academics fought for individual self-determination in

---

[84] M. G. Lord, "Cold Warrior's Daughter," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 45–53; Zuoyue Wang, "Engineering a New Space: Chinese American Scientists and Engineersf in Aerospace in Southern California," in *Blue Sky Metropolis*, ed. Peter J. Westwick (University of California Press, 2012), 197–214; Walter J. Boyne, *Beyond the Horizons: The Lockheed Story* (Thomas Dunne Books, 1998); William D. Hartung, *Prophets of War : Lockheed Martin and the Making of the Military-Industrial Complex*, 1 online resource (vii, 296 pages) vols. (New York : Nation Books, 2011); Hevly, "Reflections on Big Science and Big History"; Thomas C. Lassman, "Putting the Military Back into the History of the Military-Industrial Complex: The Management of Technological Innovation in the U.S. Army, 1945–1960," *Isis* 106, no. 1 (March 1, 2015): 94–120, https://doi.org/10.1086/681038; Pandya, "The Vanishing Act: Stealth Airplanes and Cold War Southern California."

research direction (although they did not always achieve it) and industry did not. Several authors identified the dearth of trained researchers after the war, and the human resources innovations developed by the aerospace industry to lure and compensate academic scientists speaks to the day-to-day conditions these scientists experienced. Similarly, the influence of the Cold War on science made it clear that many academic and industrial scientists understood the national security implications of their work. In this way, the Cold War had as large a role in directing "undirected" and "basic" science as did the government institutions controlling funding. Aerospace also embedded itself into the intellectual fabric of Southern California through its interaction with the environment. Corporate culture, aerospace technology, and suburbia all diffused into the social consciousness.

Having an understanding of the overall culture of science in the Post-War period does not, however give us insight into how knowledge was created, how innovation was encouraged, or how science-policy at the macro-level influenced scientific practice at the bench (the micro-level). There are several theoretical models which speak to these questions. The three most prominent are the Linear Model, the Mode 1/Mode 2 model, and the Triple-Helix model. There is a rich literature describing and evaluating these models.

Models of Science and Innovation: The Linear Model

A common and recurring theme in the literature of industrial science is the critique of the Linear Model as insufficient to describe knowledge development. Many of these critiques are founded on the perception that the model was not designed to model real innovation, but was simply a construct designed to ensure favorable policy decisions for academia. There is also a strong vein of commentary that dismisses the model on logical foundations, and even semantics. However, while there are flaws in the model, the concept of a linear progression of knowledge from innovation to technology had, and still has, utility in simplifying the process for non-scientists in positions of authority to make science-policy decisions.

The Linear Model is a collection of normative ideals within the scientific endeavor dating to the early 19th century. The model defines a differentiation between ‘pure science,’ ‘applied science,’ and finally technology. Pure science, which is also referred to as ‘fundamental science’ and ‘basic science’ in the literature with varied shades of meaning, is the undirected science conducted in an academic environment. These environments are theoretically free from the distracting and corrupting effects of a profit motive. The knowledge scientists produce in academic environments is explicitly without practical purpose and the scientists themselves show disinterest as a core normative characteristic. The knowledge created in universities and other pure research environments is then transferred to industry and applied science. [85]

According to the model, applied scientists use the fundamental laws of nature described by pure scientists to solve practical problems - usually motivated by profit, but sometimes for ideological or strategic purposes such as military advantage. Applied science is mostly conducted in an industrial laboratory and is concerned with solving the problems necessary to develop technology. While our general concept of technology usually revolves around physical artifacts, technology can also be intellectual in nature. The model essentially says that basic science is produced in the university; this knowledge is then applied in industry to create technology.

---

[85] Robert King Merton, *The Sociology of Science : Theoretical and Empirical Investigations* (Chicago : University of Chicago Press, 1973), 383, Merton’s influential work, published in 1947, was based on a study of enlightenment science starting in the 18th century. While the normative ideals which he identified were part of the enlightenment scientific endeavor, they did not match the modernist science which took hold during the interwar period. Even so, these ideals are still often assumed to be representative of science. Henry Etzkowitz et al., “The Future of the University and the University of the Future: Evolution of Ivory Tower to Entrepreneurial Paradigm,” *Research Policy* 29, no. 2 (February 2000): 313–30,

Initially this model began as the differentiation between applied and pure science. The term 'applied science' was coined by Samuel Taylor Coleridge in 1817 when translating the German term *angewandte Wissenschaft*. This differentiation is key to the Linear Model because it identifies applied science as 'other' and subordinates it to pure science. Historians Sabine Clark and Robert Bud showed that in Britain, this differentiation did not carry with it the same set of normative meanings that it did in the United States and the concept of a Linear Model did not describe the knowledge production system in place from the 19th into the mid-20th centuries. This was largely in part because Britain spent this period moving the bulk of its science to government laboratories, while the United States relied on newly established research universities to produce scientific knowledge. The university was not the dominant producer of knowledge in Britain, nor was industry the developer of application, so the Linear Model did not fit.[86]

The United States, however, had several developments in the 19th century which encouraged the differentiation between applied and pure science. In 1883, physicist Henry Rowland was concerned by inventor Thomas Edison's renown as the foremost "scientist" in the United States. In a speech to the American Association for the Advancement of Science, Rowland described and demarcated the differences between pure, and applied science. Rowland recognized that during this period, science and technology were perceived by society as identical, with technology having the greater prestige. His *A Plea for Pure Science* gave academic scientists a core set of values and reminded his listeners that there are no immediate profits in basic research, but it needs to be done nonetheless. Pure scientists were, according to Rowland, idealists committed to the greater good. "The curse of Adam," he said, "is upon us all, and we must earn our bread. But it is the mission of applied science to render this easier for the

---

[86] Robert Bud, "'Applied Science': A Phrase in Search of a Meaning," *Isis* 103, no. 3 (2012): 537–38; Sabine Clarke, "Pure Science with a Practical Aim: The Meanings of Fundamental Research in Britain, circa 1916–1950," *Isis* 101, no. 2 (June 1, 2010): 285–311, https://doi.org/10.1086/653094.

whole world." Applied science was therefore merely a money-making exercise, albeit a humanistic and important one.[87]

Historian Ann Johnson attributed this moment to the cementing of the Linear Model in the minds of academics. It gave clear missions to both applied and pure scientists, and more importantly it gave a clear path for policy makers. Pure science needed resources from government, while applied science could earn enough to sustain it. Johnson maintains that the Linear Model, while under almost constant attack by historians and sociologists of science, "remains a robust model for policymakers." It was, after all, Rowland's goal to protect the funding of university science in the face of a societal preference for application and scientific utility.[88]

The period in which *A Plea for Pure Science* was published also gave the argument strength. Paul Lucier contends that the Gilded Age, and its pessimism for a future corrupted by materialism, was the ideal environment to give pure science an incorruptible sheen. Cleverly, Rowland spun applied science to imply optimism and opportunity instead of corruption. Rowland was not offering an either/or scenario but a both-as-necessary vision of the scientific enterprise. ""Pure" and "applied" thus represented an essential tension in the relations between the search for knowledge and the pursuit of profit in a capitalist society."[89]

In 1945, the model was bolstered again. This time Vannevar Bush in his influential report for President Truman *Science, the Endless Frontier*. Made similar arguments to Rowland. One of the goals of his report was to ensure funding for university research in the Post-War environment. He tied

---

[87] Henry A. Rowland, "A Plea for Pure Science," *Science*, no. 2 (1883): 243; Paul Lucier, "The Origins of Pure and Applied Science in Gilded Age America," *Isis* 103, no. 3 (2012): 536, https://doi.org/10.1086/667976.
[88] Johnson, "What If We Wrote the History of Science from the Perspective of Applied Science?," 38.
[89] Lucier, "The Origins of Pure and Applied Science in Gilded Age America," 536.

everything from weapons research to healthcare to the importance of what he now termed "basic research." He asked "How will we find ways to make better products and lower costs? there must be stream of new scientific knowledge to turn the wheels of private and public enterprise…Further progress of industrial development would eventually stagnate if basic research were long neglected." If that were not clear enough, he went on to write "Basic research leads to new knowledge. It provides scientific capital. It creates the fund from which the practical applications of knowledge must be drawn." [90]

Historians David Hounshell, Margaret Pugh O'Mara and Glen Asner attested to the influence of Bush's report on post-war science. Asner demonstrated in his investigation of the development of the Internal Research and Development (IR&D) funding system, that it was constructed precisely along the lines of the Linear Model. In her broader study of Cold War science and the roots of the military-industrial complex, O'Mara identified both Bush's contentions that the post-war scientific endeavor should be managed by scientists not politicians, and that the process of innovation was delicate and needed a hands-off approach regarding directed research. Hounshell, O'Mara, and Daniel Kevles in his seminal work *The Physicists* all demonstrate that while science policy was established in the 1950's and 1960's guided by the Linear Model, it was a version Bush himself would not have liked. Bush wanted to protect the academy and to ensure a permanent revenue stream, but by 1963, the vast majority of scientists were employed in industry (68%) and 90% of R&D expenditures went to industrial science labs.[91]

---

[90] David E. Nye, "From Science to Industry? Flaws in the Linear Model," *Isis* 97, no. 3 (September 1, 2006): 543; Vannevar Bush, *Science, the Endless Frontier: A Report to the President on a Program for Postwar Scientific Research* (Office of Scientific Research and Development, 1945), 18.p. 19

[91] O'Mara, *Cities of Knowledge : Cold War Science and the Search for the next Silicon Valley*, 46; Daniel J. Kevles, "The National Science Foundation and the Debate over Post-war Research Policy, 1942-1945: A Political Interpretation of Science--The Endless Frontier," *Isis* 68, no. 1 (March 1, 1977): 5–26, doi:10.1086/351711; David A. Hounshell, "Industrial Research," in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 63.

There are several arguments which historians have used to critique the Linear Model. Fundamentally, the model does not actually model the process of science. Historian David Nye refers to the Linear Model as "a fable which "served the scientific community particularly well, justifying large grants for "blue sky" research and warning off politicians, defense contractors, voters, or other potential critics who wanted to tell scientists what to do." This same argument is given by Etzkowitz and Leydesdorff, authors of the Triple-Helix model, and Michael Gibbons, developer of the Mode 1/Mode 2 model of innovation.[92]

Some historians, such as David Edgerton and Thomas Misa, argued that the model is so far from representative of the real process of science, that we should dismiss it altogether. Misa has a moderated view that the Linear Model remains "an article of faith for many entrepreneurs, technologists, scientists, and journalists." However, for the historian, the model serves a reductive role which only has use in micro-histories. Misa argues that the model may be axiomatic within science, but we should not use it as an historiographical tool. [93]

Edgerton agreed with Misa and went further to suggest that the Linear Model, and all the Linear Models stemming from it, are false narratives. While the model was in use for more than 100 years, Edgerton has determined that the name "Linear Model" has been used only since the very late 1960's. Similarly, Graeme Gooday, Sabine Clark, and Robert Bud reject the Linear Model on the grounds that the essential differentiation between pure/basic/foundational science and applied science is unsupported in

---

[92] Nye, "From Science to Industry? Flaws in the Linear Model," 543; Henry Etzkowitz and Loet Leydesdorff, "The Dynamics of Innovation: From National Systems and 'Mode 2' to a Triple Helix of University–Industry–Government Relations," *Research Policy* 29, no. 2 (February 2000): 110; Michael Gibbons, Camille Limoges, and Helga Nowotny, *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies (1)* (London, GB: SAGE Publications Ltd, 1994).

[93] Thomas J. Misa, "Beyond Linear Models: Science, Technology and the Processes of Change," in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 257.

the historical record and therefore invalidates the model. Hounshell, who also sees the limitations of the model vehemently disagreed with these wholesale dismissals of it which leaves no room for debate. Hounshell wrote “The Linear Model was real, for it had real instrumentality; it compelled government and firms to invest in “basic” research.” He suggested that the easy way out for historians is to simply deny that the model ever existed.[94]

In summary, the traditional Linear Model states that there is a one-way pathway for knowledge flow which results in technological advancement: basic scientific research → industrial applied science → technological advancement. This model has been tradiationally been used by academia to argue that basic science should be financially supported as a public good because economic growth is correlated to scientific (and technological) advancement; and that academic science should remain undirected since innovation cannot be prescribed, only occurs in undirected science, and requires an undirected environment to maximize innovation potential. Since there is very little logical or historical support for any of these propositions, contemporary historians now see the model as a straw-man argument which reduces real scientific practice to a caricature. It is, and has always been, a hyperbolic device used to defend the resources of academic science.

Scientists and historians have rejected the Linear Model as a tool for describing the true relationship between academic and industrial science. On the other hand, the model is still used by university administrations, politicians, and public science agencies to justify funding for basic research. It

---

[94] Clarke, “Pure Science with a Practical Aim: The Meanings of Fundamental Research in Britain, circa 1916–1950”; Bud, “‘Applied Science’: A Phrase in Search of a Meaning”; Graeme Gooday, “‘Vague and Artificial’: The Historically Elusive Distinction between Pure and Applied Science,” *Isis* 103, no. 3 (September 1, 2012): 546–54, https://doi.org/10.1086/667978; Hounshell, “Industrial Research,” 63.

also persists as the mythology taught to young graduate students to promote the prestige of academic science over industrial applied science.

Mode 1/Mode 2

In 1994, Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott and Martin Trow published *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies.* This was an attempt to model modern knowledge production, and to explain its movement between stakeholders. While criticism of the book has been significant, the model has been persistent in the history of science since it describes a series of relatively recent changes in how knowledge is produced.

At a broad level Gibbons et. al. characterize the Mode 1/Mode 2 model as anti-differentiationist. The Linear Model requires a clear differentiation between basic and applied science (which are also represented by academic and industrial science). Indeed, this differentiation is inherent in Mode 1, or the traditional form of knowledge development. Mode 2, or modern knowledge development is characterized by a lack of barriers between fields and the promotion of transdisciplinarity.

In describing Mode 1, Gibbons et. al. write that "For many, Mode 1 is identical with what is meant by science. Its cognitive and social norms determine what shall count as significant problems, who shall be allowed to practice science and what constitutes good science." Mode 1 problems are described by specific and bounded communities, usually academic.[95]

---

[95] Gibbons, Limoges, and Nowotny, *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies (1)*, 2–3.

Mode 2 knowledge creation is usually carried out in a context of application and is engages stakeholders from many communities. There are four primary characteristics differentiating Mode 1 and Mode 2 knowledge development: Context of application, transdisciplinarity, organizational heterogeneity, social accountability and reflexivity, and quality control.

Mode 2 knowledge creation is performed with specific outcomes in mind. Unlike Mode 1 which tends to be undirected, Mode 2 knowledge is intended to be useful. This is not to say that the outcomes are product development, but are simply undertaken with a goal in mind. Gibbons et. al. offered as an example aeronautical engineering. This was a field which was established in universities to supply knowledge for an application (flying). Even so, much of the work was performed in a Mode 1 style. This is also a characteristic of this model that both Mode 1 and Mode 2 development can occur in interaction with each other.

Transdisciplinarity goes beyond collecting diverse specialists to contribute to knowledge creation. It is a description of the dynamism of the process of integrating both transdisciplinary skills, and the consensus of stakeholders in the level of success achieved in application development. It also describes the diverse ways in which the knowledge is “socially diffused” instead of being transmitted through institutional channels (as Mode 1 would be).

The organizational heterogeneity of Mode 2 is in contrast to Mode 1’s hierarchical processes. Since the individuals, groups, and networks working on a problem all represent innovation opportunities and these knowledge creation spaces are networked with other groups and projects, great flexibility in human resources skillsets can be utilized. Actors can rapidly move in and out of a project, and respond rapidly to new innovation. Additionally, Mode 2 is predominantly performed at the project, or meso scale, while Mode 1 can be performed at the micro, or meso scales.

Social accountability also takes a significant role in Mode 2. It is the acceptance of responsibility for a range of qualitative social problems which orbit the field under study. Gibbons et. al. wrote that traditionally "this has been the function of the humanities, but over the years the supply side - departments of philosophy, anthropology, history - of such reflexivity has become disconnected from the demand side - that is from businesspeople, engineers, doctors, regulatory agencies and the larger public who need practical or ethical guidance on a vast range of issues."[96]

Finally there are significant differences in how the "quality" of science is judged in Mode 1 and Mode 2. The quality of Mode 1 science is mostly determined by peer-review concensus which determines how well knowledge was developed. Mode 2 also includes peer-review, but also asks a broader audience to ask questions about the science. These questions can include social concerns, market concerns, or cost effectiveness. By widening the pool of reviewers beyond peers, Mode 2 makes it more difficult to produce accepted "good" science.

In addition to the construction of Mode 1 and Mode 2 models, Gibbons et. al. specifically addressed the relationship between science and industry. While in the model, there is no distinction between the science practiced in academia versus industry; the difference is only in location. In Mode 2, the university system relies on industry for funding the basic research unit - the research group. The benefit for industry is the freedom of technology transfer. They wrote "This transformation is one of the most far-reaching that we have described because it involves drawing the universities into the heart of the commercial process. The universities are no longer the remote source and wellspring of invention

[96] Gibbons, Limoges, and Nowotny, 7–8.

and creativity but, are part of the problem solving, problem identification and strategic brokering that characterize the knowledge industries."[97]

Technology transfer occurs in traditional institutional modes in Mode 1 science - journal articles, books, and talks. In Mode 2, many more casual communication methods developed in the early 1980s. These included immediate communication devices, but also legal instruments which bound labs and even colleges to research companies, and campus patent offices which helped spark universities to entrepreneurialism.

The Mode 1/Mode 2 model met with a great deal of discussion in the early and mid-1990's. It seemed to resonate more closely with the European scientific endeavor more than that of the United States. The arguments against this model generally fall into issues with function and modeling accuracy. Sociologist of science Terry Shinn was concerned that the book's lack of any empirical evidence made it read more as dogma than a useful model. He wrote

> Instead of theory or data, the *New Production of Knowledge* – both book and concept – seems tinged with political commitment. The authors appear to be true believers in a new cognitive and social order. They work actively in its favor and seek to persuade others to think likewise. One has to wonder if the perspective is not more a social platform than a serious, systematic framework for scholarly inquiry.[98]

This concern with a lack of empirical data and sweeping generalizations in Gribbon et al.'s *New Production of Knowledge* also prompted Shinn to doubt that the book has "methodological motor that is

---

[97] Gibbons, Limoges, and Nowotny, 86.
[98] Terry Shinn, "The Triple Helix and New Production of Knowledge: Prepackaged Thinking on Science and Technology," *Social Studies of Science* 32, no. 4 (2002): 604.

necessary to drive any research programme forward." He suggested that this is because the book is not grounded in generally accepted theory.[99]

Aant Elzinga agreed with Shinn that the Mode 2 model was bereft of empirical evidence. Aside from accusing the authors of rampant presentism, Elzinga says the major flaw with the work is that it fails to "analytically distinguish between functional differentiation on an institutional level, and the changes of organizational boundaries." Additionally, Mode 1 science has never truly existed in the terms the authors suggest and cannot be used as a premise to define Mode 2.[100]

Similarly, Henry Etzkowitz and Loet Leydensdorff complained that "Mode 1 is a construct, built in order to justify autonomy for science, especially in an earlier era when it was still a fragile institution and needed all the help it could get." in this same paper, however, Etzkowitz and Leydensdorff promote Mode 2 science as representing "the material base of science," and has been the real mode of science since the seventeenth century.[101]

Even one of the authors, Helga Nowotny, admitted that there were flaws in the original theory. The first is the idea that traditional Mode 1 communication systems were insufficient for Mode 2 communications. The issue is that the authors wrote this in an academic book. Nowotny said they had "fallen into their own post-modern trap." Additionally, Nowotny regretted that the book mimicked Thomas Kuhn's seminal *The Structure of Scientific Revolutions* with their descriptions of paradigms in

---

[99] Shinn, 604.
[100] Aant Elzinga, "The New Production of Reductionism in Models Relating to Research Policy," in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 291.
[101] Etzkowitz and Leydesdorff, "The Dynamics of Innovation: From National Systems and 'Mode 2' to a Triple Helix of University–Industry–Government Relations," 115; Etzkowitz, *The Triple Helix : University-Industry-Government Innovation in Action*, 142.

knowledge creation. In its defense, Nowotny also suggested that the work identified science's interactional with political epiphenomena and presented a new world of postmodern research.[102]

Nowotny also strongly denied that the lack of empiricism in the work lessened its value. However, since some of the criticism of the work had merit, Nowotny, Peter Scott and Michael Gibbon, in 2003, wrote *'Mode 2' Revisited: The New Production of Knowledge.* This was a simplified and more focused work which focused only on science. This final work, while expressing the same essentials as the first book, did cement a norm within modern science that Mode 2 does exist. Like the Linear Model, Mode 2 does not have a strong capacity to model the scientific endeavor. However, it does offer a name for the modern scientific endeavor when the location of knowledge development, be it academic or industrial, is irrelevant. This is not what the authors intended, but it is useful nonetheless because it relieves us of the differentiation between pure and applied science.

The Triple-Helix

The Triple-Helix model was developed in the mid-1990's by historians of science Henry Etzkowitz and Loet Leydesdorff. Unlike the largely discredited Linear Model, and the predominantly descriptive Mode 1/Mode 2 model, the Triple-Helix not only models knowledge creation, but is predictive in nature. It was originally designed to model the trajectories of scientific endeavors in a knowledge-based society, but morphed into modeling entrepreneurial universities, regional innovation, and the incubation of innovation.

The model is analogous to a three-dimensional triple helix. The call to the structure of DNA is intentional and implies an evolutionary process. The strands of the Triple-Helix are made from

---

[102] Helga Nowotny, Peter Scott, and Michael Gibbons, "Introduction: `Mode 2' Revisited: The New Production of Knowledge," *Minerva* 41, no. 3 (2003): 180.

academia, industry, and government. Each strand interacts reflexively as the helix twists and information is moved from one strand to another. These interactions can change the trajectory of the entire Triple-Helix, but tri-lateral interactions between groups tends to stabilize the entire endeavor. Each Triple-Helix is referred to as a *regime*.

These regimes interact internally and display four characteristics which differentiate traditional science from the modern version: a focus on human resources, innovation networks, output circulation (reciprocity), and non-linear innovation. There are also defined characteristics for government, industry, and academia.

Human Resources and the Circulation of Individuals

The circulation of individuals carries both knowledge and practical skill from one strand to another. This circulation can occur as permanent movement, dual-life in both spheres, or alternation where significant portions of time are spent in one area over another. The fear of losing bright young scientists to industry was part of Henry Rowland's motivation for *A Plea for Pure Science*. Vannevar Bush also made producing scientists a pillar of his vision in *Science, the Endless Frontier*. Etzkowitz and Leydensdorff understood how critical it was to have the right people in place for innovation to take place. But this model was designed to accommodate very recent entrepreneurial universities where the distraction of money is already integrated.

The authors contended that throughout the 19th century the industrial scientist was seen as subordinate to the academic scientist. A common trope in academic histories is to portray the industrial scientists as unhappy with his lot in life. Steven Shapin wrote that in this fable, "the industrial scientist was deeply disliked if he did not actively rebel against the violation of scientific values he found in industry: secrecy, regimentation, hierarchy, constraint, and short-termism. The money - for the money was on the whole good - never made up for it." If this was accurate, how, and why would academic scientists move into industry? Shapin went on to demonstrate in *Who is the Industrial Scientist?* that on the whole, industrial scientists in the middle of the 20th century had as much, if not more, job satisfaction as their academic counterparts. This made the movement of human capital within a regime far more fluid during the later 20th century than Etzkowitz and Leydesdorff give credit for.[103]

The Circulation of Information Within Communities

In contemporary science, the enormous power of information technology allows scientists to create networks of both support and communication. The Triple-Helix requires constant reflexive communication between strands and helices to encourage innovation. All we need to demonstrate to exhibit this characteristic is that a regime is using a robust and reflexive system of knowledge transfer. Reflexivity refers to the ability of a regime to send information back upstream if an interaction has somehow demonstrated more work needs to be done at a previous stop. It is important also to remember that unlike the Linear Model, the development of knowledge in a Triple-Helix requires bilateral systems of communication.

Reciprocity among actors is also a pillar of the model. The idea here is that each actor holds the others accountable for equitable contributions to the trajectory of a regime. Within this process you also see role reversion where industry, which is usually directed by government, might step into that role of policing another actor. In Mode 1 science, there is a clear hierarchy and roles are prescribed and never reversed.

The final characteristic differentiation the Triple-Helix from Linear Models is acknowledgment that innovation can occur anywhere within the trajectory of the endeavor, or within any of the actors at

[103] Shapin, "Who Is the Industrial Scientist," 337.

any time. This characteristic rejects the Linear Model's insistence that innovation occurs only in basic science. Etzkowitz and Leydesdorff insisted that innovation can occur at any time up until the enterprise has lock-in. Lock-in is a model-specific concept which describes the moment when the trajectory of a regime becomes fixed.

The Triple-Helix was proposed originally by Henry Etzkowitz in 1993, and enhanced considerably in partnership with scientometrician Loet Leydesdorff. Since it did not have a single definitive book defining it, Etzkowitz and Leydesdorff were able to polish and refine the model in response to criticism through several journal articles. By 2008, Etzkowitz published a definitive book on the topic after the concept had taken hold through several international conferences and many published case studies. In 2010 Etzkowitz founded the Triple Helix Research Group at Stanford, and in 2014, the journal *Triple Helix* was launched which was dedicated to discussion of the model. The most recent exposition of the Triple Helix model was written with Marina Ranga in 2013, and this version developed a framework for creating an innovation framework by incorporating community, geography, and society.

The success of the model for describing contemporary interactions between industry and science does not mean it is without critics. Aant Elzinga is critical of the work in that it artificially smooths over conflict and exclusion mechanisms which give the appearance of overly smooth and collaborative interactions between actors. He is also concerned that the model is designed more for policy development than analysis.[104]

Historian John Krige declared Etzkowitz's historical analysis of the university mission "deeply flawed." Etzkowitz reduces the historical missions of the university into teaching, teaching and research, and teaching, research, and economic growth. This last mission gave rise to the entrepreneurial

---

[104] Elzinga, "The New Production of Reductionism in Models Relating to Research Policy," 286–87.

university where knowledge is capitalized. Krige casts this part of the theory as clearly deterministic, writing “the historical process is necessarily contested, and any path that wins the day and that is eventually established is local, not general, particular, not universal, and due to the specific circumstances under which the battle was fought out in a certain time and place.” Jeff Hughes also voices concern that Etzkowitz and Leydensdorff’s use of “received” history of science assumes too many normative decisions “which would not sustain comparison with the results of recent historical research and critique.”[105]

Terry Shinn was generally supportive of the Triple-Helix in comparison to the Mode 1/Mode 2 model. It was empirical, theoretically grounded, useful across multiple fields; although perhaps too focused on policy development. Conversely Shinn agreed with Krige and Hughes that the work was historically lacking. He said the theory assumed that transversality (the crossing of cognitive, technical, economic, and societal boundaries) was a functional product of our time and culture. This assumption is nowhere historically supported. He also feared that the focus on the entrepreneurial university would “open the way to, or perhaps even legitimate, a neo-corporatist vision of the world.” He also felt that the writing style of the authors was impenetrable. This could lead readers to dismiss the co-evolutionary concept of the model since the theoretical foundations were poorly expressed.[106]

The Triple-Helix model, therefore, has several advantages over Mode 1/Mode 2. It is based in empirical evidence and does not define how knowledge creation should be conducted, but instead

---

[105] John Krige, “The Industrialization of Research: Commentary in Praise of Specificity,” in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 133; Jeff Hughs, “The History of Science, the Public, and the ‘Problem’ of Policy,” in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 369.
[106] Shinn, “The Triple Helix and New Production of Knowledge: Prepackaged Thinking on Science and Technology,” 606.

models how it is managed. Perhaps even more importantly, the Triple-Helix is infinitely scalable. Unlike Mode 1/Mode 2 which purports to model only individual and team or project-based science, the Triple-Helix can be used to model anything from individual-based science to giant multinational science projects. While the predictive nature of the Triple-Helix also differentiates it from Mode 1/Mode 2, this facet of the model is a less useful tool for historical analysis.

Conclusion

While there is limited literature specifically examining how science was managed at the meso and micro levels during the immediate post-war years, we have enough literature to give us a context for this topic. It is clear that at some point around WWII, the national scientific endeavor changed. Big Science became a national priority through a pre-war combination of Lawrence's cyclotron, Stanford's Linear Accelerator, and MIT's Radiation Lab, the wartime Manhattan Project, and the post-war pressures of the Cold War. Universities accepted a role in the military-industrial complex and the differentiation between basic and applied research blurred in the practice of science in both industry and academia. That is not to say that the mythology and prestige of basic, academic science as proffered in the Linear Model does not live on today. On the contrary, it is alive and well in academia where it continues to serve a role in protecting revenue streams for the universities. It does not, however, describe the knowledge-creation pathway of modern science.

Aerospace may have found a welcoming home in the blue skies, open land, and clear weather of Southern California, but the post-war aerospace industry could not rest easy. The Cold War motivated and drove aerospace research at every level. There was tremendous intrinsic and extrinsic pressure to accelerate industrial science. Unfortunately, what aerospace did not find in Southern California was adequate human resources. There was a national shortage of researchers and many firms left Southern California in the 1950's to partner with universities to share researchers. Similarly, the aerospace industry shared researchers with government labs like NACA and the Army Laboratories. Shared researchers meant shared data. When we look at the free exchange of human resources, the circulation of information between the academic, industrial, and governmental institutions within aerospace, and the focus on innovation at all research levels, we see that the Triple-Helix can also be used to accurately model knowledge creation in the early aerospace industry at the macro, meso, and micro scales. Moreover, it demonstrated that he early aerospace industry was practicing modern science. The next chapter examines the topic of human resources as the preeminent factor in modern science.

Chapter 3

Human Resources

This chapter examines why human resources were afforded such importance in aerospace, and the strategies employed by the Triple-Helix actors in recruiting, managing, and retaining employees. Particular attention is given to the system of loaning employees at the meso and macro levels. The chapter also includes a discussion of diversity (or lack thereof) in the early aerospace workforce.

While the Triple-Helix model is designed to describe, and subsequently forecast, innovation in science, it also describes several characteristics of modern science which differentiate it from system of scientific norms which preceded it.[107] The most pronounced of these characteristics is the primacy of human resources; in recruiting, in retention, and in management. At the macro level, the prestige of scientists was used for marketing. This focus on human resources might be expected to pit the stakeholders in the Triple-Helix against each other in competition for talent, but the movement of people back and forth between stakeholders created an equilibrium between industry, academia, and the government within the overall talent pool. At the macro scale scientists, engineers, and administrators moved among stakeholders on a full-time and part-time basis. At the meso scale, within a stakeholder, employees also circulated between departments, and divisions. At the micro scale, the focus on human resources operated predominantly on recruitment, retention, and collaboration between individuals. However, not all of the available human resources were efficiently used. Both women and people of color were almost entirely excluded from opportunities within the military-industrial-academic complex.

[107] For detailed discussions of both the changes in scientific norms, and the Triple-Helix model, please see chapters 1 and 2 respectively.

The growth of Big Science during and after WWII increased the demand for researchers without a corresponding increase in supply. In the late 1950's and early 1960's aerospace companies sought prestige to aid in recruiting personnel in competition with academia and also in the persistent competition for military IR&D funding.[108] The initial stages in the contracting process with the military were a balance between contractors supplying the military's needs and generating these same needs through research proposals and marketing. While it was common for the military to begin contracting by soliciting bids for militarily-identified weapon development, the military expected companies like Lockheed to develop new weapons and *then* convince the military that they were desirable.

Unfortunately, this did not always end well financially or in regards to prestige. The enthusiasms of aerospace engineers sometimes rubbed off on the military procurement officers and projects went a long way before they were ended as untenable. The CL-400 Suntan, for example, was developed in the late 1950's as a supersonic, liquid-hydrogen fueled reconnaissance aircraft. The advantage of liquid hydrogen was that it could provide greater thrust than contemporary jet engines and the plane was designed to fly at Mach 2.5. Unfortunately, liquid hydrogen is less dense than conventional jet fuel and the fuselage had to be enormous to give it a useful range. The project was finally ended when the Air Force realized that liquid hydrogen was immensely expensive to produce and required an established high-power electrical grid. This meant that the plane could not be refueled in the field and was therefore not viable. Unfortunately, the Lockheed-directed IR&D ran for several years before this was appreciated and the project was killed. This is not to imply that these projects were a total loss. Even if a technology program was abandoned, the research results were usually retained for other projects. The

[108] Depending on who is writing it, IR&D is either *Internal* or *Independent* Research and Development. The term refers to industrial science, which is ultimately funded by the government, but directed by industry. This was a source of control by the government over the other two stakeholders in any Triple-Helix regime.

data from Suntan, for instance, was transferred to Convair who used it in the development of the Centaur series of rocket boosters.[109]

Aborted projects like the CL-400 were forgiven by the military in light of more successful developments like the SR-71 six years later (which flew at Mach 3.3 and 90,000 feet). However, the cost of failed projects were not forgotten by the government, and it made the contracting systems progressively more complex to avoid these situations. As resources became harder to obtain, Lockheed enhanced their brand by emphasizing the prestige of individual scientists and the overall technological prowess of the company.

Prestige

Prestige was developed and used in multiple ways at the meso and macro levels in aerospace. The importance of recruiting top talent was necessary for companies to operate at the cutting edge of innovation, it was also recursive tool for further recruitment; the theory was that top talent best recruited other top talent. The importance of prestige can be seen in the eagerness of Lockheed to collect external perspectives on Lockheed's strengths. In 1965, William Rieke was loaned from Lockheed to NASA as Assistant Administrator of Industrial Affairs where he performed well enough to earn NASA's Medal for Exceptional Service. He was recalled to Lockheed in 1967 and stationed at Lockheed Missile and Space Company (LMSC) where he would ultimately become President. Daniel J. Haughton, president of Lockheed in 1967, requested from Rieke a complete analysis of NASA's impressions of Lockheed and the rest of the major players in aerospace. Rieke's report "ruffled a few feathers," since it portrayed Lockheed's reputation as one of excellent management but limited scientific innovation. Haughton was

[109] Ben R Rich, "Speech to Encino Men's Club," 1967, Box 4(2), Ben R. Rich Papers.; Ben R Rich, "Speech to Rensselaer Polytechnic Institute," April 5, 1983, Box 4(8), Ben R. Rich Papers.

concerned enough by this to request a response from LMSC's president Gene Root. Root requested that Willis Hawkins respond, and Hawkins in turn engaged Bill Wilson to perform a survey to gather more empirically reliable data. Wilson answered with a survey ranking Lockheed as having the best scientific reputation in aerospace. Hawkins and Root's response to Haughton was reassuring but suggested that Lockheed should continue to emphasize recruiting top talent.[110]

Unfortunately for Root and Hawkins, there was another published survey along the same lines early in 1968 which ranked Lockheed "fairly far down on the list of U.S. industrial companies insofar as scientific reputation was concerned." Haughton told Hawkins that this was "embarrassing" and must not happen again. Hawkins then wrote a scathing note to Wilson which was highly unusual for the usually polite and kind Hawkins. He requested that Wilson's consultants come up with ways for Lockheed to improve its prestige versus non-aerospace companies. Hawkins wrote to Winlson:

> We rank reasonably high with respect to the aerospace companies alone, even on this list, but somehow General Dynamics beat us out by a substantial margin. This, if I remember properly was absolutely backwards in your survey. In short…you have fallen flat on your face. What can I do to help you pick yourself back up?[111]

There is evidence, however, that the emphasis on prestige was not as important as Lockheed's operations leadership thought. In 1967, Willis Hawkins solicited comment on the capacity of Lockheed to market and develop a hypersonic[112] transport with a planned maximum velocity above Mach 5.

---

[110] William Rieke, "Impressions While At NASA," May 30, 1967, Box 46(6), Willis M. Hawkins Papers.; Gene Root, "Respone to Rieke Memorandum," August 2, 1967, Box 46(6), Willis M. Hawkins Papers.
[111] Willis Hawkins, "Memo to Bill Wilson," February 29, 1968, Box 46(6), Willis M. Hawkins Papers.; Willis Hawkins, "Diary Entry 4/12/1967," April 12, 1967, Box 2(1), Willis M. Hawkins Papers.
[112] Hypersonic is generally used to refer to speeds at Mach 5 or above.

Lockheed's chief scientist R.L. Thoren's answer could not have been less encouraging regarding Lockheed's potential for success. He wrote that:

> Although we have demonstrated a reasonable capability for vehicle design with our relatively small engineering team, we do not have the scientific capability to support it. CALAC[113] is engineering-oriented rather than scientifically oriented. The scientific approach is required in some areas, but more importantly, science is a way of life in the space business, and a company cannot really compete without a strong scientific image.[114]

Even the legendary aircraft designer Kelly Johnson recommended Lockheed stay away from Hypersonic projects. He felt that the contracts Lockheed were receiving were too limited in scale with "McDonnell and Martin being funded for hardware programs to an amount 20 to 25 times that given to Lockheed." Kelly urged caution in bidding because he also believed that the Air Force was only using Lockheed for ideas and passing them on competitors for further IR&D.[115]

Despite Kelly's concern, Lockheed's president, Carl Kotchian directed the ad-hoc hypersonic committee to maintain hypersonics as part of the Lockheed portfolio. Thoren was asked to volunteer to mange the R&D necessary. To Thoren's surprise, in May 1967, Lockheed received an IR&D contract for a reusable orbital launch vehicle which they planned to launch from a B-52. That internal and external concerns existed, and yet Lockheed still won the bid, is indicative that prestige was not a deal-breaker for the military.

---

[113] California Lockheed Aircraft Company.
[114] R. L. Thoren, "Internal Memo to C. S. Wagner," May 1978, Box 30(1), Willis M. Hawkins Papers.
[115] Kelly Johnson, "C. L. Johnson to A.C. Kotchian," 1967, Box 30(1), Willis M. Hawkins Papers.

Recruitment from Within the Triple-Helix

One of the obvious places to recruit employees was from the other stakeholders in the Triple-Helix. This movement of personnel went in other directions. Willis Hawkins, an outstanding recruiter for Lockheed, was himself recruited away from Lockheed in 1964 by the U.S. Army to work as Assistant Secretary. He was subsequently recruited *back* to Lockheed in 1966. Nonetheless, during the late 1950's and early 1960's, Hawkins' chief responsibility was to staff the new Lockheed Missile and Space Division (LMSD). His plan was to use compensation, self-direction, academic prestige, a path for advancement, job-security, and location as incentives for experienced and entry-level scientists alike.[116]

The LMSD (soon to become the Lockheed Missile and Space Company – LMSC) was founded by an ex-Air Force general name Elwood "Pete" Quesada. The LMSD was initially based in Burbank California and suffered the same HR pressures which were common the aerospace contractors at the time. In 1955, Los Angeles had only Caltech and UCLA producing aerospace engineers and the competition among aerospace companies in the region was strong. Quesada was able to recruit well initially, but in 1955 Lockheed bid for, and unexpectedly won, the contract for Polaris missiles. With a staff of only 80, Quesada was expected to ramp-up his operation by several thousand employees to meet a four-year delivery deadline. Without a ready supply of new talent, Quesada convinced Lockheed leadership that the entire company should move to where the talent was. Inexplicably, Quesada chose LMSD's new location as Asheville, North Carolina. The site gave no significant advantage to Lockheed, and Lockheed leadership chose an alternate site in Sunnyvale, California. The great advantages of Sunnyvale, at the Southern end of the San Francisco Bay, were that it was next door to Palo Alto, the home of Stanford University and Mountain View, the home of the NACA (predecessor to NASA) Ames

---

[116] Willis Hawkins, "W. M. Hawkins to L. H. Whittemore," May 4, 1956, Box 30(10), Willis M. Hawkins Papers.

Research Center and Moffett Field Naval Air Station. This struggle was a "hill Quesada was willing to die on," and in 1956 he left Lockheed with several high-level engineers and scientists to form his own company.[117]

The LMSD moved to Sunnyvale in 1956 and became the LMSC. LMSC's president was then L. Eugene "Gene" Root, but the growth of LMSC staff was in the hands of Willis Hawkins. Hawkins was an engineer and administrator for Lockheed for almost his entire career. After earning a BA in aerospace engineering from the University of Michigan in 1937, Hawkins immediately began work for Lockheed at their headquarters in Burbank, California as a draftsman. He was the project lead on the highly successful C-130 Hercules transport from 1951-1953 and contributed significantly on the F-104 Starfighter design.[118]

In 1953, he was assigned to the new Lockheed Missile and Space Company (LMSC). By 1961, his talent for administration was recognized with promotion to General Manager and Vice-President of LMSC's Space Systems Division. In 1962 he was promoted to Lockheed Aircraft Corporation (the governing entity for other Lockheed companies) Vice-President of Science and Engineering. He held this role until 1963 when he left Lockheed to serve as Assistant Secretary of the Army for Research and Development. In his time with the Army, he received the Distinguished Civilian Service Award in 1965 and 1966 for his work on the M-1 Abrams main battle tank. In July of 1966 he returned to his previous position at Lockheed and in 1969 was promoted again to Senior Vice President of Science and Engineering. In 1976, after a short retirement, he was asked to return to Lockheed as President of the Lockheed California Company. Hawkins was a talented administrator, engineer and networker. He was an officer in several industry associations, on the NASA advisory council, and the Chair of the National

[117] F. A. Cleveland, Interview with F. A. Cleveland, 1982, Box 75(18), Willis M. Hawkins Papers.
[118] In 2015, more than 2,500 C-130's had been built. They are still in production and service more than 65 years later.

Academy of Engineering Aeronautics and Space Engineering Board. He received honorary doctorate from the University of Michigan and from Illinois College.[119]

Hawkins, who in 1957 was the Assistant General Manager of LMSC, recognized that the terrific advantage of being so close to Stanford was reduced by Stanford's lack of an aerospace engineering program. Hawkins, whose HR strategy usually involved headhunting, identified Nicholas J. Hoff as a likely candidate for a high-level position with LMSC. Hoff was a Hungarian-born mechanical engineer who had graduated with his Ph.D. from Stanford in 1929. He had joined the Polytechnic Institute of Brooklyn in 1940 as an instructor, and by 1950 was the Chair of the Mechanical and Aerospace Engineering Department. It seemed to Hawkins that Hoff had reached a professional pinnacle in a regional, albeit well respected, college and might be looking for a professional challenge. Hawkins' strategy was to promote both the financial freedoms of industrial science, and the opportunity to participate in large-scale projects. Hoff was not swayed. He resisted any full-time appointment without the prestige and self-direction of academia.[120]

Hawkins was not dissuaded and turned to Frederick Terman, the provost of Stanford University. At this time, Terman had only been provost for a year, but had convinced the Board of Trustees to adopt his plan to create within Stanford intellectual "steeples of excellence." He had founded the highly respected microwave laboratory on the campus and created the Stanford Industrial Park which would house not only Lockheed, but Hewlett-Packard, Eastman-Kodak, Varian, and other technology firms.

---

[119] Brook Engebretson and Emily Wittenberg, "Biographical Note," in *Willis M. Hawkins Papers 1920-2009 Finding Aid* (The Huntington Library, San Marino, California, 2011), 3–4; Thoren, "Internal Memo to C. S. Wagner."

[120] Stephen B. Adams, "Follow the Money: Engineering at Stanford and UC Berkeley During the Rise of Silicon Valley," *Minerva* 47, no. 4 (December 1, 2009): 384; Willis Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957, Box 33(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to A. C. Kotchian," July 12, 1966, Box 33(3), Willis M. Hawkins Papers.

Terman's vision was one of creating a space where innovation between industry and academia would flourish.[121]

When Willis Hawkins contacted him in 1957 to discuss Nicholas Hoff, Terman was immediately interested. Terman recognized that the proximity of Ames and Lockheed, and Lockheed's willingness to supply resources, could rapidly make aeronautics a steeple of excellence. The proposal he and Hawkins developed was that Hoff would be the chair, and indeed the only member, of the Stanford Aeronautical Engineering Division. Lockheed would both fund the department and cover Hoff's salary. In return Hoff would split his time equally between academic duties at Stanford and consulting for Lockheed. This was a highly unusual arrangement for 1957, yet Terman received approval from the Board of Trustees in under three weeks. Hoff accepted and returned to California.[122]

Lockheed had funded individual scientists before with research of commercial interest, but Lockheed's interaction with Stanford brought entire organizations to Lockheed for funding requests. This unexpected result put Lockheed in a similar position to the government which influenced Triple-Helix regimes through funding. In Lockheed's case funding gave them a recruiting advantage. When Caltech approached Lockheed looking for $250,000 to endow a chair, Hawkins recommended to Lockheed President Carl Kotchian that Lockheed move ahead with it purely for the purposes of hiring talent. His reasoning to Kotchian was that the prestige of the chair was necessary for further recruiting. He wrote:

---

[121] Leslie, "Playing the Education Game to Win: The Military and Interdisciplinary Research at Stanford."
[122] Leslie, *The Cold War and American Science : The Military-Industrial-Academic Complex at MIT and Stanford*, 115–18; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957; Willis Hawkins, "W. M. Hawkins to H. J. Brown," 1956, Box 33(3), Willis M. Hawkins Papers.

> It puts you in good rapport with a member of the faculty, it permits you to expand the school's talent, and if you do indeed endow a chair, the Lockheed name is repeated constantly so that the students hear of the company in the best possible context.[123]

Hoff was the first of several joint appointments between LMSC and Stanford. This arrangement also helped Hawkins cope with a very significant issue affecting all aerospace companies – variable headcount needs. The military procurement system in the late 1950’s and early 1960’s was essentially to bid out large projects to several leading aerospace firms. Since producing bids was expensive, the government was willing to fund the initial research leading up to a bid. Once the bid was awarded, the unsuccessful bidders were forced to disband their preliminary research teams. The winner of the contract also had a challenging problem since they would have to increase resources to perform IR&D, but once manufacturing had begun, these initial research teams also had to be reduced. For example, in March 1957, the R&D behind the F-104 Starfighter and the C-130 Hercules had been completed. Manufacturing of the F-104 continued in California, but the C-130 was moved to the Georgia Lockheed Aircraft Corporation (GELAC) in Marietta, Georgia. The total number of employees at the California Lockheed Aircraft Company (CALAC) in 1957 was 32,026. By March of 1959, it had dropped by a third to 20,376 despite a 10% increase in overall sales between 1958 and 1959.[124]

These swings in personnel needs were an issue for all aerospace companies, and in the immediate postwar years scientists and engineers would simply move from one company to the next when projects ended or started. However, as the 1950’s wore on and human resources became scarce,

---

[123] Hawkins, “W. M. Hawkins to A. C. Kotchian,” July 12, 1966.
[124] Lockheed Corporation, “Lockheed Missile and Space Company Mission Statement,” 1956, Box 30(11), Willis M. Hawkins Papers.

it became an imperative to recruit and retain skilled personnel. This issue was constant source of anxiety for Hawkins because profitability for LMSC was initially forecast to be low. As such the LMSC Mission Statement had the requirement to keep personnel to a minimum to keep expense low. On the other hand, the people LMSC needed to retain were, according to Hawkins “very expensive personnel working in parallel to ensure state-of-the-art in all areas (no deliverables are obsolete). This gives speed of development, high level research, and flexibility in project acceptance.” Hawkins went on to emphasize the importance of retention and job security in recruiting, writing that “Stability must be visible in the organization to attract high-level personnel.” Hawkins strategy of splitting appointments with academia also allowed Lockheed to keep its most vital scientific core intact. This strategy worked for Hawkins and from 1956 through 1962, LMSC did not have a reduction in force and grew from 3,356 employees to 31,099 in that same span.[125]

The long term understanding behind the support of academia was not just as collaborators, but preparers of the next generation of scientists and engineers. The aerospace industry had a need for tens of thousands of highly trained employees, but the universities were not producing enough. Recruiting long-term academics like Nicholas Hoff might have solved short-term human resources shortages, but it posed a long-term threat to the overall employee development system. Even so, Lockheed persisted in recruiting from academia throughout the 1950’s. Hawkins had planned to raid the University of Michigan’s physics department in 1956, but by the 1960’s it was clear that recruiting from universities should be scaled back to a sustainable pace. [126]

---

[125] Hawkins, “W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.,” 1957; Lockheed Corporation, “Historical Earnings and Human Resources,” 1977, Box 75(8), Willis M. Hawkins Papers.; Lockheed Corporation, “Lockheed Missile and Space Company Mission Statement.”

[126] Oral history interview with Simon Ramo., interview by Peter J. Westwick, December 4, 2010, 42, Aerospace Oral History Project; Oral history interview with E. Milton Wilson, interview by Perter J. Westwick, April 4, 2010, 28–29, Aerospace Oral History Project.

F. A. "Al" Cleveland was a contemporary engineer and administrator with Hawkins. He called this kind of recruiting "eating your own seed corn," and argued against wholesale recruiting from academia saying:

> We, as a corporation, right now have shortages in certain skills, and those shortages come and go among the various companies. But the way in which the faculties of our major technically competent universities are being eroded by people like ourselves and our competitors -- who are robbing the faculties or their, … well, just plain robbing the faculties.[127]

To combat the threat of recruiting raids during the 1950's, universities emphasized and expanded the primary differentiations between themselves and industry – self-direction, job-security, and prestige. While industry could advertise specific examples of undirected R&D in Lockheed, RAND Corporation, and TRW, it could not compete with the cultural prestige of undirected science which remained a crucial attraction for academic science. [128]

The rush to recruit also caused divisions within the same company to trip over each other. In 1959, Harold Gumbel, an applied mathematician working for the Air Force, came to the attention of Lockheed. Gumbel's expertise in both computing and guidance systems made him a target on not one, but four separate divisions within Lockheed. All four failed to recruit Gumbel, and when Hawkins investigated, he found that the salary offers made were between $160 and $210 per week. This range

---

[127] F. A. Cleveland, Interview with F. A. Cleveland, 48.
[128] Willis Hawkins, "W. M. Hawkins to E. H. Krause," December 16, 1954, Box 30(8), Willis M. Hawkins Papers.

prompted Hawkins to write to J.B. Wassall in Lockheed's Human Resources Department to request standardized recruiting practices.[129]

Despite the setback with Gumbel, Hawkins recruited heavily from the military. Over the 1950's and 1960's several career military officers had been recruited by Lockheed as liaisons to their previous military services. The standard practice was to look for flag officers in technical roles. This was complicated by Lockheed's mandatory retirement age of 65 since many of the qualified officers were in their 50's. An example of Hawkins' commitment to excellence in hiring comes in the form of General Leighton I. Davis to head Lockheed's Corporate Safety Board as Executive Secretary. This was to be the highest-ranking liaison posting in Lockheed. In 1966, when Hawkins returned from his time with the Department of the Army, he was assigned to recruit for the role immediately since the previous Executive Secretary had already overstayed his mandatory retirement age. Hawkins was under significant pressure to fill the role and went through several highly qualified candidates until he settled on Davis in 1967. At the time, Davis was a Lt. General in charge of the National Range for the Air Force and the manned spaceflight for the Department of Defense. His immense network and experience in contracting made him a supremely qualified choice. Unfortunately, Davis resisted Hawkins's recruitment attempts. In 1968, Davis finally agreed to retire from the Air Force and join Lockheed. It had taken more than two years from starting to recruit to hiring.[130]

The Corporate Safety Board was designed as a conduit control to manage the flow of information (especially from safety related issues) to both Lockheed's corporate entity, and to the scientists in various divisions. Davis reported directly to Hawkins who at this time was Senior Vice

---

[129] Willis Hawkins, "W. M. Hawkins to J. B. Wassall," July 20, 1955, Box 30(9), Willis M. Hawkins Papers.; Mike Garrambone, Bob Sheldon, and Bill Dunn, "Military Operations Research Society Oral History Project Interview of Mr. Lewis A. Leake, FS," *Phalanx* 44, no. 2 (2011): 13–22.

[130] Willis Hawkins, "Subject File: Corporate Safety Board," 1969 1965, Box 75(1), Willis M. Hawkins Papers.

President for Science and Engineering. The Board significantly increased the reporting requirements from divisions which added a paperwork burden. Division presidents, especially at LMSC and Corporate (CORLAC) pushed back on Davis' requirements. Hawkins actively supported Davis and the division-heads ultimately fell in line. This episode had an unintended negative effect. While this program succeeded in increasing information flow to the corporate group, the policies for reporting and record-keeping which resulted slowed down the movement of developed knowledge back and forth through the system, thus suppressing innovation. [131]

Prospective employees did not associate the attractions of working in industry or academia with working for the government. Science and engineering positions in government agencies did not carry a great deal of prestige or financial reward (with the exception of excellent retirement benefits). However, in a period of heightened national security, working for the government offered a chance for scientists and engineers to serve their country. National pride was a strong motivating factor. Willis Hawkins, for example, had been invited to join the Department of the Army by the Kennedy administration in 1963 and gave up his vice-presidency at Lockheed to do so. His work for the Army burnished his reputation in the aerospace industry even though his time had mainly been spent on the R&D of the Abrams M1A1 main battle tank. On June 6th, 1966, Hawkins submitted his letter of resignation to President Johnson. The letter characterized his service as "the highest honor" of his life," and he was recognized among his peers for his service. When he returned to California to be the Vice-President of Science and Engineering for Lockheed, he received effusive letters of congratulations, not just for his promotion, but also for his service to the country.[132]

---

[131] Hawkins.
[132] Willis Hawkins, "W. M. Hawkins to Lyndon B. Johnson," June 6, 1966, Box 47(13), Willis M. Hawkins Papers.; Daniel J. Houghton, "D. J. Houghton to Lockheed Leadership," May 30, 1966, Box 47(12), Willis M. Hawkins Papers.; G. R. Makepeace, "G. R. Makepeace to Willis Hawkins," July 31, 1962, Box 47(12), Willis M. Hawkins Papers.

The assistance industry offered academia in recruiting was mirrored with government. Hawkins understood that the military did not have the same natural recruiting advantages among scientists as academia and industry, so he committed himself to facilitating movement of employees from industry into government. He maintained his strong personal network within the armed forces (which would later yield Leighton Davis for Lockheed), and actively recruited on behalf of the military from among industry personnel. This recruitment activity was with the approval of Lockheed's leadership and gave Lockheed yet another place to send valuable employees during periods of lean business.[133]

Lockheed and other aerospace companies also actively and successfully recruited from each other. Roy Anderson, who would later become the CEO and Chairman of the Board for Lockheed, was working for Ampex Corporation in 1960. He had been recruited away from Lockheed in 1959 but only a year and a half later, Ampex was failing. Willis Hawkins, who was building the LMSC at the time, heard about Ampex's issues and called Anderson. He said "Look, Roy, we keep reading about the problems Ampex is having, and I hear you're not too happy with your move over there, and we want you to come back." Anderson was flattered, but he had committed to a previous supervisor at Lockheed that if he ever came back, he would return to that division. Hawkins encouraged Anderson to contact this supervisor (Malcolm Williams) but Anderson ultimately ended up working at LMSC for Hawkins.[134]

International Recruiting

Lockheed did not invest significantly in international recruitment during the late 1950's and early 1960's because there were few experienced scientists to recruit. Most were already in the United

---

[133] Willis Hawkins, "W. M. Hawkins to R. Randall Irwin," July 18, 1966, Box 47(13), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to D. J. Houghton," February 1968, Box 33(3), Willis M. Hawkins Papers.

[134] W. D. Perreault, Interview with Roy Anderson, February 12, 1982, 6, Box 75(13), Willis M. Hawkins Papers.; Oral history interview with E. Milton Wilson, 30.

States. Immediately after WWII, the US government recruited German scientists and engineers to rapidly develop the technology necessary for both ballistic missiles and spaceflight. This was called Project Paperclip and it provided a core of researchers for aerospace, and skilled administrators for the developing U.S. military-industrial complex. The most famous of these was Wernher von Braun who, along with the team of German scientists had developed the V-2 rocket for Nazi Germany. Between 1945 and 1947 more than 1500 Germans joined the U.S. scientific community, predominantly in military and government roles.

One of these scientists, Willy Achim Fiedler, who had worked on both the V-1 manned rocket and the Bachem "Natter" rocket plane during the war, moved to California as part of Paperclip in 1948. Initially he began work at the Pt. Mugu Naval Airstation, but in 1956 joined Lockheed to work on the Polaris missile program. Fiedler, in June 1956, recommended to Hawkins two specific engineers still in Germany by the names of Jarosch and Maugsch. Hawkins, who at the time was building the LMSC and in urgent need of scientific talent, followed up immediately with letters of interest to both Germans. Unfortunately, since Project Paperclip and the Russian equivalent had denuded Germany of top-tier scientists, the U.S. State Department, on behalf of the West German government requested that Hawkins not move ahead with recruiting. In his final response to Fiedler in September 1956, Hawkins implies that national security concerns might also be in play and Hawkins proceeded to focus on domestic recruiting.[135]

---

[135] Willis Hawkins, "W. M. Hawkins to W. A. Fiedler," September 14, 1956, Box 30(10), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to E. Fife," May 22, 1956, Box 30(10), Willis M. Hawkins Papers.; Brian E. Crim, *Our Germans: Project Paperclip and the National Security State* (JHU Press, 2018), 4–10; Rit Staalman and Monica Wagner-Staalman, "Designer, Test Pilot, Aeronautical and Missile Engineer," n.d., 2, 41–44; Boyne, *Beyond the Horizons*.

Entry-Level Recruiting

For freshly minted scientists and engineers, the most obvious career path was to remain in academia. After all, the college environment had been all they had known through graduation and academia offered prestige, self-direction, tenure, and the opportunity to maintain the status quo in their lives. The aerospace industry in the early 1950's and 1960's needed to attract entry-level employees from academia in addition to headhunting experienced candidates. Aerospace did this by promoting lifestyle advantages in location, income, and social-life. Aerospace coped with the attractions of academia by offering a liberal approach to publishing and encouraging individual collaboration with local universities.

For entry-level positions in science and engineering, the opportunity to move to California was often a strong motivator and an important recruiting tool. The weather was fair and consistent, and the outdoors were beautiful, but not too distracting. Malcolm Currie, a scientist for Hughes, recalled that the view of Malibu from the research facility off the Pacific Coast Highway made him think he was "in paradise." Simon Ramo of both Hughes and TRW (the R in TRW), also recalled that it "California was a very important ingredient in bringing top talent to the Los Angeles area." The pull of the land was so great, that Ramo identified what he called "California-itis." This was the palpable discomfort that occurred in employees whenever they were required to spend long periods away from the state. But despite the beauty of the area, and the temptations of distraction, the scientists and engineers of aerospace generally stayed focused on their work. [136]

[136] Oral history interview with Simon Ramo., 2; Oral history interview with E. Milton Wilson, 31; Oral history interview with Malcolm R. Currie., interview by Peter J. Westwick, May 9, 2013, 24, Aerospace Oral History Project.

Management Culture as a Recruiting Tool

Another Lockheed technique for recruiting was to market their superior management culture. This was a three-pronged strategy which emphasized good leadership, professional development, and career advancement. By the mid-1960's Lockheed had grown into a major multinational military contractor recognized for its strong internal governance. Lockheed had been purchased in 1932 by an investment group led by brothers Robert and Courtland Gross. The Grosses were financiers, not engineers and operated Lockheed on sound financial and marketing principles, but they understood their limitations and looked for capable project managers from the engineering and scientific ranks. After they purchased the Lockheed assets from the bankrupt Detroit Aircraft Corporation, the Grosses bet the future on the Lockheed Model 10 Electra. The Electra design team was led by engineer Hall Hibbard. Unfortunately, the prototype was unstable at altitude. A young engineer named Clarence "Kelly" Johnson recommended a complete redesign of the tail to form a 'H' shape with vertical stabilizers at the tips of the tail of the tail's horizontal stabilizers instead of a single, centered vertical stabilizer. This insight was the first of many for Johnson who achieved renown for both his aircraft designs and his management style. Kelly led the development teams of many aircraft, the most famous of which were the P-38 Lightening, the F-80 Shooting Star, F-104 Starfighter, the U-2, and the SR-71 Blackbird.[137]

The successful development of the U-2 at Lockheed's Burbank headquarters resulted in Kelly (who was Lockheed's Chief Engineer) being appointed the Vice-President of the Advanced Development Projects Division, or as it was better known, the Skunk Works. The Skunk Works was a semi-autonomous, and top secret division of Lockheed based near the CALAC headquarters in Burbank,

---

[137] Rieke, "Impressions While At NASA"; Ben R Rich, "The Big Step," August 21, 1978, Box 4(7), Ben R. Rich Papers.

California. The management system Johnson applied freed his staff to cut through red tape and create an innovation community. Johnson had, over the years, developed a set of 14 principles by which he managed the Skunk Works to success.[138]

1. The Skunk Works manager must be delegated practically complete control of his program in all aspects. He should report to a division president or higher.
2. Strong but small project offices must be provided both by the military and industry.
3. The number of people having any connection with the project must be restricted in an almost vicious manner. Use a small number of good people (10% to 25% compared to the so-called normal systems).
4. A very simple drawing and drawing release system with great flexibility for making changes must be provided.
5. There must be a minimum number of reports required, but important work must be recorded thoroughly.
6. There must be a monthly cost review covering not only what has been spent and committed but also projected costs to the conclusion of the program. Don't have the books 90 days late, and don't surprise the customer with sudden overruns.
7. The contractor must be delegated and must assume more than normal responsibility to get good vendor bids for subcontract on the project. Commercial bid procedures are very often better than military ones.
8. The inspection system as currently used by the Skunk Works, which has been approved by both the Air Force and Navy, meets the intent of existing military requirements and

[138] For more on the Skunk Works, see Rich and Janos, *Skunk Works*.

should be used on new projects. Push more basic inspection responsibility back to subcontractors and vendors. Don't duplicate so much inspection.

9. The contractor must be delegated the authority to test his final product in flight. He can and must test it in the initial stages. If he doesn't, he rapidly loses his competency to design other vehicles.
10. The specifications applying to the hardware must be agreed to well in advance of contracting. The Skunk Works practice of having a specification section stating clearly which important military specification items will not knowingly be complied with and reasons therefore is highly recommended.
11. Funding a program must be timely so that the contractor doesn't have to keep running to the bank to support government projects.
12. There must be mutual trust between the military project organization and the contractor with very close cooperation and liaison on a day-to-day basis. This cuts down misunderstanding and correspondence to an absolute minimum.
13. Access by outsiders to the project and its personnel must be strictly controlled by appropriate security measures.
14. Because only a few people will be used in engineering and most other areas, ways must be provided to reward good performance by pay not based on the number of personnel supervised.[139]

---

[139] C. L. Johnson, "Basic Operating Rules of the Lockheed Skunk Works," n.d., Box 5(3), Ben R. Rich Papers.; Ben R Rich, "Skunk Works - 10 Multipoint Parameters for Success," n.d., Box 5(3), Ben R. Rich Papers.
There was also a 15th rule which Kelly added verbally depending on the audience. It was always a variation of "15. Don't do business with the Navy. They don't know what they want and will drive you up a wall before they break your heart." See Rich and Janos, *Skunk Works*.

Johnson's codified management strategy set the tone among divisions at Lockheed and is still in use. He set high standards for staff, demanded accountability, and engendered terrific loyalty through individual empowerment. Ben Rich said that "It was a privilege to work with [Johnson] because he made me see the importance of recognizing the Big Need, and/or the Big Opportunity, and to be willing to take the Big Step."[140]

Johnson, and then Lockheed in general, also recognized the value of professional development through higher education. Rich noted that "Almost every man in the Skunk Works technical areas (Aerodynamics, Thermodynamics, Propulsion, and Flutter) have a higher degree. Most obtained them through Lockheed. It is essential that our technical people keep updated. We have rewarded all young engineers for doing this."[141]

The combination of human resources scarcity and the promotion of higher degrees was so strong in 1967 that Willis Hawkins began pushing internally to develop some kind of training institute to alleviate the shortage of highly skilled employees which was forecast for the next several years. He suggested that one institute be in New Jersey and another in Dallas. The location in New Jersey was to supply Lockheed Electronics Company (located in North Plainfield, NJ) with personnel. Hawkins lobbied both Lockheed president Carl Kotchian, and Lockheed Electronics Company president Daniel J. Gribbon on the same day to support the idea. Hawkins even suggested that this might solve the regular loss of experienced personnel at the mandatory retirement age of 65 by converting them to faculty. Unfortunately, the idea was never realized, but it shows that while Lockheed Corporation was led by financial experts, the meso-level of management in both R&D and production was led by experienced

---

[140] Rich, "The Big Step."
[141] Ben R Rich, "Ben Rich to Karl R. Kunze - The Skunk Works Management Operation," December 18, 1972, 2, Box 5(3), Ben R. Rich Papers.

and talented experts who had been promoted into position from within. This gave confidence to incoming employees who saw stability in leadership, job security, and a career path.[142]

Lockheed was also dedicated to promoting from within. This was used as an example for recruiting but it also allowed Lockheed the opportunity to recoup training investment, retain the best employees, and maximize employee's skills prior to the mandatory retirement age of 65. Employee development and promotion was, however, generally as structured and labor-intensive as hiring at lower management levels. Division and company-based committees were established to determine if candidates for promotion had the talent, education, and experience necessary to succeed at a higher level of responsibility.

Along with Hawkins, Ben Rich was an example of home-grown leadership and promotion. Rich was an engineer and administrator who worked for Lockheed from 1950 until his retirement in 1990. Like many young engineers after the war, Rich joined the aerospace industry as a draftsman. In 1955, after working on the C-130 Hercules and F-104 Starfighter projects with Willis Hawkins, Rich was recruited by the legendary aircraft designer Kelly Johnson. Johnson needed someone to run a test project on a liquid hydrogen aircraft (the CL-400 Suntan) for the Skunk Works. Rich was a trusted assistant to Johnson and played key roles in the development of the U-2 spy plane, the SR-71 Blackbird, and the doomed CL-400.

While working on the SR-71 project, which took only four years from conception to completion, Rich designed the inlet of SR-71 engine nacelles to create both a travelling shockwave to increase thrust

[142] Willis Hawkins, "W. M. Hawkins to A. C. Kotchian," May 3, 1967, Box 33(2), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Dan Gribbon," May 3, 1967, Box 30(2), Willis M. Hawkins Papers.

at subsonic speed, and to convert the powerplant from a turbojet to a ramjet above Mach 1.5.[143] This engineering feat won several engineering society awards and lifted Rich's reputation into the highest echelons of aerospace engineers.

In the mid 1960's Rich managed several projects but was predominantly a liaison to the military. In 1975, Johnson retired and handpicked Rich to succeed him as director of the Skunk Works. Rich achieved even greater renown in the late 1970's by spearheading the development of the F-117A stealth fighter.

Rich was a naturally light-hearted man. He enjoyed laughing and was kind, thoughtful manager. He clung closely to Johnson's 14 management rules. He often referred to two of the principles as the core of good management: number three was "Only use a small number of very good people," and number 14 was that "performance should be rewarded, not the number of people supervised." Kelly and subsequently his protégé Rich both understood the importance of human resource management.[144]

At the macro levels of authority, promotion tended to be of employees who had already been identified by leadership and were the result of a mentorship system. Kelly Johnson, Willis Hawkins, and Ben Rich had all been selected and groomed by their mentors for leadership. Kelly Johnson and Willis Hawkins were mentored by Hall Hibbard, and Ben Rich by Johnson. Mentors within Lockheed took an active role in professional development. The mandatory age of retirement gave a defined end-point to a career and made developing a protégé a singular goal. Kelly Johnson recognized by the late 1960's that Ben Rich would be the best person to maintain Johnson's standards at the Skunk Works. Rich even had

---

[143] A turbojet uses fans to compress air in an ignition chamber and it is mixed with gaseous fuel and ignited. A ramjet has no fans. The speed of the aircraft is such that the pressure in the ignition chamber is high enough to simulate the use of fans. The SR-71 could convert its engines from turbojets to the much more efficient ramjets while in flight.
[144] Rich, "The Big Step."

the nerve once to tell Johnson that he “wanted Johnson’s chair.” Despite having the management skill and technical experience, Johnson knew that Rich did not have enough public recognition to assume the high-profile leadership role that the Skunk Works required. So in 1969 Kelly nominated Rich for the American Institute of Aeronautics and Astronautics (AIAA) Aircraft Design Award which he would win in 1972 for the jet intake nozzle on the SR-71. That completed Rich’s resume and when Johnson retired in 1975 (at 65) Lockheed President Carl Kotchian was satisfied that the selection of Rich to lead the Skunk Works would offer “no change to the unique policies and tightly knit methods of operation, established by Kelly Johnson, which have garnered international respect for the Skunk Works’ capabilities and accomplishments.”[145]

Not every hand-picked succession went as smoothly as Rich’s. L. Eugene (Gene) Root, was a well-known mechanical engineer and aircraft designer for Douglas Aircraft Company in the 1930’s and 1940’s. In 1951 he left Douglas to organize the Air Force’s Development Planning Office. After an extensive process, he was hired in 1955 as General Manager at what was then Lockheed’s Missile and Space Division. At the time, Willis Hawkins was the Assistant General Manager, a position he had earned while being promoted through the ranks of engineers. While new to Lockheed, Root carried a great deal of leadership experience and recognized Hawkins’ rare combination of administrative, managerial, and technical skill. Over the next several years, Root promoted Hawkins to General Manager, then in 1961 to Vice President of LMSC. Additionally, Root had nominated Willis Hawkins in 1957 to replace him as Lockheed’s representative at the Aerospace Industries Association (AIA) steering committee (this was the precursor organization to the AIAA). This was a stretch for the relatively young Hawkins and his nomination was declined at that time but would be accepted a few years later. But this support was

---

[145] Lockheed Corporation, “Kelly Johnson Retirement Press Release,” 1975, Box 1(3), Ben R. Rich Papers.; Herb Nystrom, “Herb Nystrom to Ben Rich Congratulating Rich on Winning Collier Trophy,” April 13, 1990, Box 1(2), Ben R. Rich Papers.; Sherm Mullen, “Retirement Speech for Ben Rich,” December 31, 1990, Box 1(1), Ben R. Rich Papers.

invisible to the rank and file engineers and scientists of LMSC. They were proud of one of their own achieving success and many felt greater loyalty to Hawkins than to (the then LMSC president) Root. Rumor circulated in 1961 that Root had been badmouthing Hawkins, and on June 19 Hawkins received an anonymous letter, written in a secondary hand to disguise the writing, informing Hawkins of Root's behavior and pledging allegiance to Hawkins. It was signed "the engineers of LMSC." What the authors could not have known was that Hawkins was already in process of another promotion at the behest of Root, and in August 1961, Hawkins returned to Lockheed Aircraft Company as VP of Engineering.[146]

The importance of these mentor relationships and succession plans were not lost on those affected. In 1993, Ben Rich wrote a birthday congratulation to Hall Hibbard who had been Kelly Johnson's mentor. Rich wrote:

> I personally never knew you very well. I got to know you through Kelly Johnson. You were to Kelly, what he was to me - his mentor. He often spoke of you as his stabilizing force. Your guidance focused his tremendous energy and enthusiasm - exactly what he did for me.[147]

Diversity in Hiring

Despite the focus of the early aerospace industry on human resources, this system of succession planning effectively eliminated women and minorities from macro and meso-level positions of authority. During WWII, Lockheed had hired thousands of women to compensate for the male labor pool lost to

[146] L. E. Root, "L. E. Root to Willis Hawkins," August 14, 1961, Box 47(11), Willis M. Hawkins Papers.; William Littlewood, "William Littlewood to L. E. Root," January 17, 1958, Box 47(8), Willis M. Hawkins Papers.; Root, "L. E. Root to Willis Hawkins," August 14, 1961; Anonymous, "The Engineers of LMSC to Willis Hawkins," June 19, 1962, Box 33(3), Willis M. Hawkins Papers.; Makepeace, "G. R. Makepeace to Willis Hawkins," July 31, 1962.
[147] Ben R Rich, "B. Rich to H. Hibbard Wishing Him a Happy 90th Birthday," July 25, 1993, Box 1(4), Ben R. Rich Papers.

military service. When the war ended, Lockheed dismissed the vast majority of these to cope with both a returning male workforce and reduced military orders. By 1951, the need for skilled technical workers had increased and the number of women working for Lockheed almost doubled, from 2700 to 5100 which represented more than 19% of the total Lockheed workforce. However, the women of Lockheed were both culturally and professionally objectified and repressed. In that same year, Lockheed decided to present a float with Miss Lockheed for the Rose Parade. Lockheed required all the women who applied to supply a photograph of themselves in a bathing suit. There were 141 applicants and five were chosen to ride on the float. Structural repression was also clear from the results of promotion. Women in 1951 occupied 19% of Lockheed's workforce, but by 1965 of the 171 members of management at LMSC, there were zero women, and only two people of color.[148]

While African Americans were represented in shop-floor positions for men, and administrative support positions for women, barriers to entry into the industry were high for people of color. Geographic location was a particular issue after WWII when many aerospace manufacturers abandoned their inner-city plants for the outer suburbs. For advancement, education requirements generally unavailable to African Americans in a segregated country. Even those who had graduated from segregated colleges found that "their background was insufficient for advancement."[149]

The reasons for this were cultural tropes in the white male hegemony of the 1950's and 1960's in America (see Figure 1). Hughes engineer Malcolm Currie remembered "a lot of brilliant women" at the Hughes research laboratories in Malibu and then estimated it at 10%. Chemist Milton Wilson remembered women engineers being closer to 20% at Aerojet and said that there were no

---

[148] Lockheed Corporation, "Lockheed Annual Report 1951," 1952, Box 4(6), Ben R. Rich Papers.; Lockheed Missile and Space Company, "LSMC Organizational Chart," 1965, Box 18(3), Willis M. Hawkins Papers.
[149] Herbert R Northrup, "In-Plant Movement of Negroes in the Aerospace Industry," *Monthly Labor Review* 91, no. 02 (February 1968): 22.

discrimination issues with hiring; in Wilson's view they hired the best people for the job, so if women or minorities could do the job, they would be hired. He said "I honestly don't know of any bigotry in the hiring process whatsoever. We had very few blacks because there were very few black applicants." Jesse Keville, a chemical engineer and manager at Aerojet remembered the number of women scientists and engineers as much lower. "I had two women working for me, but I had 50 other Ph.D.'s," Keville said, "so it wasn't too common." Additionally, Keville remembered that women "weren't as attentive" as male employees. Similarly, the responsibility for not hiring minorities was that "Mexicans and the blacks just hadn't woken up to the fact that if they would go to college and get higher degrees that they'd have better jobs." Engineer Frank Bullock at the Lockheed Skunk Works suggested in the decades he was with Lockheed, he only remembered working with a dozen women engineers and fewer African Americans.[150]

[150] Oral history interview with Jesse Keville, interview by Peter J. Westwick, April 29, 2010, 40–41, Aerospace Oral History Project; Oral history interview with Malcolm R. Currie., 20–21; Oral history interview with E. Milton Wilson, 33–34; Oral history interview with Frank Bullock, interview by Volker Janssen, August 21, 2009, 34–35, Aerospace Oral History Project.

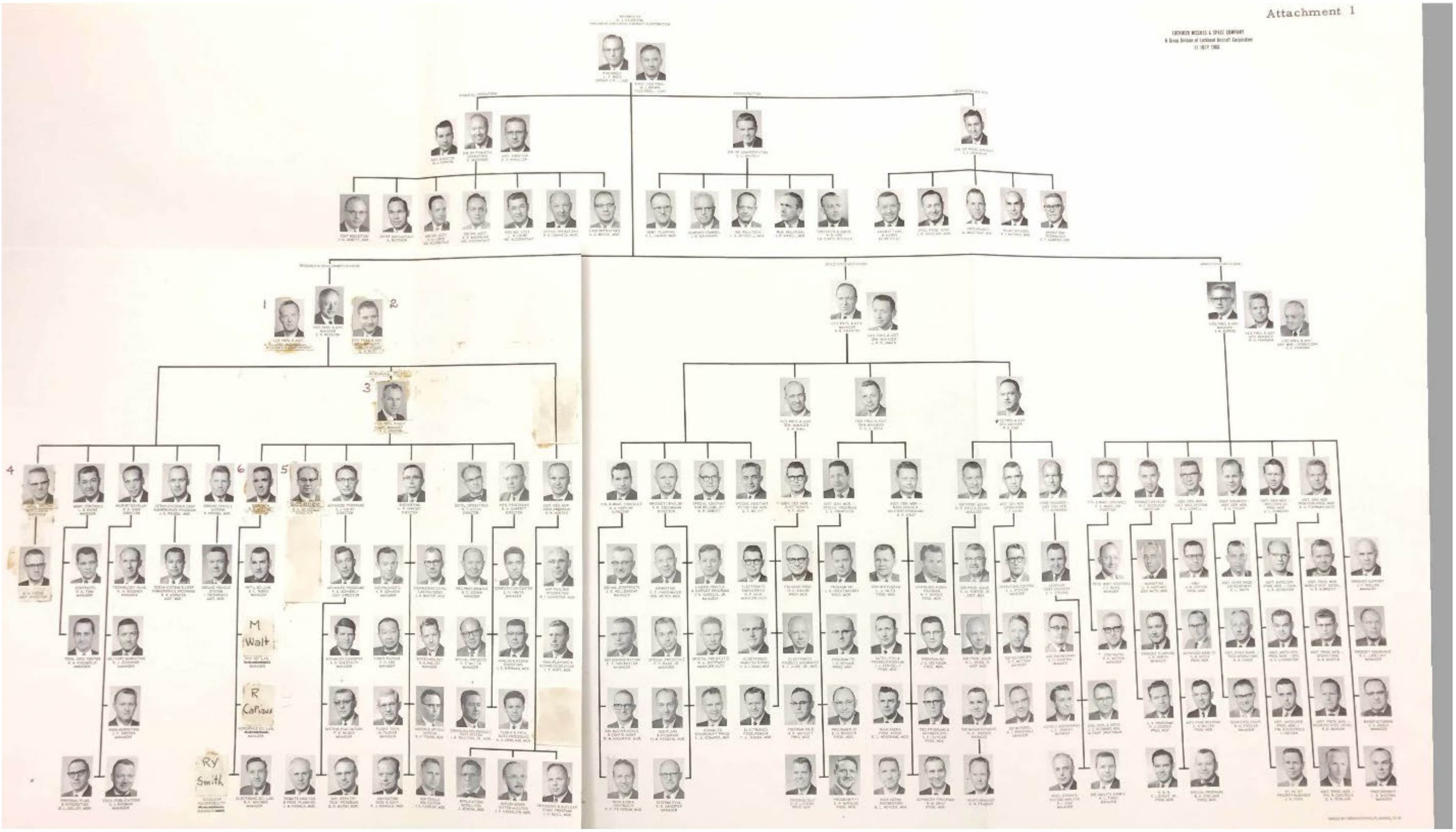


Figure 1 - LMSC Org Chart - 1967

Publication, Collaboration, and Education

At the micro scale of Lockheed's focus on human resources was the balance between the differing desires of individuals scientists and company leadership. The scarcity of available top-tier scientific talent in the late 1950's compelled Lockheed to compete with academia and the military for recruiting by emphasizing the point that Lockheed offered a very similar set of attractions to the individual scientist. One of the concerns held by these scientists in a move to industry was a perceived loss of self-direction in research. Having spent the last several years in academia, recent Ph.D. recipients were hesitant to relinquish their research freedom. This hesitancy also reflected the cultural prejudices within academic circles toward industrial science. Lockheed challenged these beliefs by encouraging publication of results by their scientific corps. This had the effect of diminishing academia's core argument against joining industry, but also burnished Lockheed's scientific reputation.

An example of this strategy was Lloyd Chase. Chase was a nuclear physicist hired out of Stanford in 1958. Chase had received both his B.S. and Ph.D. in physics from Stanford and had been recruited by Lockheed immediately upon receiving his doctorate in 1958. His path in Lockheed was one of rapid promotion and he moved from Associate Research Scientist, to Research Scientist, to Staff Scientist, to Senior Staff Scientist and Senior Member of the Research Laboratory; all in only six years. In 1963, he was able to spend time at Oxford University as a Visiting Senior Research Officer, and at Brookhaven National Laboratory as a Research Collaborator. From 1958-1965, Chase published 23 papers in the peer-reviewed journals *Physical Review*, the *Journal of Geophysical Research*, *Nuclear Physics*, and the *Bulletin of the American Physical Society*.[151]

[151] Lloyd F. Chase, "Resume of Lloyd F. Chase" (Lockheed Corporation, February 1967), Box 45(3), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Donald Horning," February 1967, Box 45(3), Willis M. Hawkins Papers.

Lockheed's encouragement of publishing also offered an attraction to scientists in other firms. Roland Meyerott was on an academic path after he received his Ph.D. in Physics from Yale in 1943. He remained at Yale until 1949 when he went to work for Edward Teller on development of the hydrogen bomb. In 1953, Meyerott was recruited by RAND corporation and moved to Los Angeles. In 1956, he moved to Palo Alto to head research and development for LMSC. Despite having no employment expectations to produce individual research, Meyerott still produced five peer-reviewed articles between 1956 and 1960 in *Physical Review, Astrophysics Journal*, *Physics Fluids Journal*, and the *Bulletin of the American Physical Society*.[152]

While Lockheed's encouragement to publish was attractive, for most scientists publications slowed in proportion to management responsibilities. R. Douglas Moffat was another nuclear physicist hired by LMSC in 1955. Moffat had, like Chase, received his entire post-secondary education from a single institution, Indiana University. Unlike Chase, however, he had been a staff member at Los Alamos Scientific Laboratory from 1950-1953 while he was completing his Ph.D. In 1953 he went to work at the University of Wisconsin performing charged-particle scattering experiments. In 1955, Moffat was recruited by LMSC to head their Experimental Nuclear Physics section. While Chase rose quickly through the scientific ranks of Lockheed, Moffat moved into a management pathway and by 1965 was the manager of the Aerospace Sciences Laboratory. Prior to working at Lockheed (from 1948-1954), Moffat had published eleven peer-reviewed journal articles in *Physical Review*. In the first three years at

[152] Ronald E. Meyerott, "Resume of Ronald E. Meyerott" (Lockheed Corporation, September 23, 1966), Box 46(6), Willis M. Hawkins Papers.; L. E. Root, "L. E. Root to D. Haughton and A. C. Kotchian," September 23, 1966, Box 46(9), Willis M. Hawkins Papers.

Lockheed he published three more articles in peer reviewed journals after that he moved out of a daily science obligation to management and his publications ceased.[153]

Between 1955 and 1965, *Physical Review* alone published 57 peer-reviewed journal articles authored by Lockheed scientists. The scientists were also encouraged to collaborate across domains with academic and government scientists. Chase published eight articles with Princeton physicist Ernest K. Warburton about nuclear structure. In 1961, Warburton went to work at Brookhaven National Laboratory so the collaboration moved from industry-academia to industry-government. Lockheed's first of these industry-academia collaborations was in 1954 between Lockheed's John L. Powell and the University of Wisconsin's Henry Barschall. Other collaborations were of every combination between Triple-Helix stakeholders and regularly involved more than a single representative from a stakeholder. Sometimes publications were supported by Lockheed but written by a single academic physicist. In 1961, for example, Jerome I. Kaplan of Brandeis spend a summer at LMSC and published an article on perturbation theory entirely funded by the LMSC budget. But collaborations between stakeholders were by far the most common project. Of the 57 articles, 9 were written by individuals and only 3 were collaborations between Lockheed scientists and other industrial scientists. In all three cases it was Bell Telephone Laboratories.[154]

---

[153] R. D. Moffat, "Resume of R. Douglas Moffat" (Lockheed Corporation, February 1967), Box 45(3), Willis M. Hawkins Papers.; Hawkins, "W. M. Hawkins to Donald Horning," February 1967.
[154] Chase, "Resume of Lloyd F. Chase"; Jerome I. Kaplan, "Perturbation Theory Using a Reduced Set of States," *Phys. Rev.* 129, no. 2 (January 1963): 644–646; H. H. Barschall and J. L. Powell, "Energy Distribution of Nuclear Reaction Products," *Phys. Rev.* 96, no. 3 (November 1954): 713–713.

Loaning of Personnel

Collaboration also occurred at different scales and was manifested at the meso-level in the *loaning* of employees between departments and, at the macro-level, loaning employees to other stakeholders in the Triple-Helix. Lockheed had several companies within the corporate structure which shared similar scientific needs. Shifting human resources gave Lockheed the capacity to internally transfer employees from expiring projects to viable projects and protect their overall workforce from business fluctuations.

An example of this lending was Al Cleveland. In 1955, Willis Hawkins was invited by Dan Haughton to go to the Georgia plant (GELAC) to oversee the creative processes there. This made sense to Haughton since GELAC was the primary plant building the Hercules C-130, which Hawkins had been instrumental in designing. Haughton desired GELAC to shift its corporate structure to be more scientifically inclined. But Haughton's desire to loan Hawkins to GELAC was still subject to Hawkins' willingness to move. Hawkins at this time had only been at LMSC for two years and was deeply engaged in the move to Sunnyvale. He sent a note to Haughton to decline the request to move. Instead, Hawkins suggested GELAC and CALAC (who had people available) exchange employees temporarily to bring GELAC up to standard. Hawkins recommended that Al Cleveland be returned (he was on loan to CALAC). Ultimately Cleveland returned to Georgia and prospered. Cleveland became Chief Engineer on Lockheed's winning bid for the C-141 Starlifter in 1960.[155]

Temporary movement between Triple-Helix stakeholders was relatively common and the practice had both recruiting and human resources management aspects. It allowed talent to be

---

[155] Willis Hawkins, "W. M. Hawkins to D. J Houghton," January 3, 1955, Box 30(8), Willis M. Hawkins Papers.

displayed on a more public stage and gave individuals the opportunity for advancement through changing responsibilities. We have already discussed William Rieke who Lockheed had loaned to NASA, but Willis Hawkins himself had been recruited by the Department of the Army from LMSC in 1963. This was an unexpected choice since Hawkins had been predominantly occupied with the Navy's Poseidon Missile and the Air Force's (and CIA's) Corona Satellite programs for the years immediately prior to joining the Army. Stranger still was the fact that his primary project as Assistant Secretary of the Army was the development of the not-very-aerospacey M1-A1 Abrams Main Battle Tank. However, the Army had not recruited him for his expertise in the field of aerospace, but his expertise in administering complex research and development projects.

The relocation to Washington D.C. was a difficult decision for Hawkins since he had a young family. While Hawkins was aspirational, the opportunity to serve his country in this move was the primary motivator in joining the Army. He said this explicitly in his resignation letter to President Johnson. Aerojet, General Electric, Raytheon, McDonnell, the Army, all sent effusive congratulations and thanks for his service with the Army when he left in 1966. In retrospect, the fact that he went immediately back to Burbank to work for Lockheed appears as though this was more of a loan to the Army than a permanent career move. However, the decision to move to a governmental post was not cynical or manipulative on Hawkins' part and it took some months of recruitment to convince him to return to Lockheed. Additionally, he continued to support his Army colleagues by actively recruiting for them both within and outside of Lockheed. He wrote some years after his resignation "I still have the Army's interests at heart, and always will." [156]

---

[156] Hawkins, "W. M. Hawkins to Lyndon B. Johnson," June 6, 1966; Houghton, "D. J. Houghton to Lockheed Leadership," May 30, 1966; Hawkins, "W. M. Hawkins to Donald Horning," February 1967; Willis Hawkins, "Subject

Universities also benefitted from a system of loaning people back and forth with the aerospace industry. Lockheed's agreement with Stanford and Nicholas Hoff was a permanent sharing agreement, but employees from Lockheed, Vidya, Hiller Helicopter, and United Research Corporation all lectured on the Stanford campus. Willis Hawkins taught a class at U.C. Berkeley on R&D administration. United Research's Howard Siefert even founded a rocket propulsion lab on the Stanford campus which was fully funded by the company. In 1955, UCLA requested a list of engineering division scientists who could teach. Hawkins responded that "As far as our organization is concerned, we are ready and willing to contribute any of these people on the basis of a half day or one day per week. Please let me know if we can become more specific — we are anxious to help."[157]

In Southern California Lockheed cultivated special relationships with Caltech and UCLA but would also take on loan faculty from other schools. Dr. M. J. Thompson of the University of Texas was one of several academic researchers who made a habit of spending the summers at Lockheed performing research. However, Lockheed also used academic scientists to teach in addition to performing research. GALCIT[158] physicist Frank J. Malina remembered that:

In 1943 or 1944, as I recall, Kelly or someone at Lockheed called Caltech to say, we think we may be encountering something that has to do with compressibility. Nobody over here knows anything about it. Could you send somebody over here to tell us about it? Hans and I found ourselves giving a series of lectures at Lockheed Aircraft to a class that included Kelly Johnson and a few great old names. We

---

File: Personal Correspondence," 1966, Box 47(13), Willis M. Hawkins Papers.; Hawkins, "W. M. Hawkins to R. Randall Irwin," July 18, 1966; Makepeace, "G. R. Makepeace to Willis Hawkins," July 31, 1962.

[157] Leslie, *The Cold War and American Science : The Military-Industrial-Academic Complex at MIT and Stanford*, 127; Willis Hawkins, "W. M. Hawkins to H. J. Brown," November 21, 1956, Box 33(3), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to D. O. Horning," May 3, 1956, Box 30(10), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to E. H. Krause," April 13, 1955, Box 30(8), Willis M. Hawkins Papers.

[158] Guggenheim Aeronautical Laboratory at the California Institute of Technology

presumed to tell them what happened to airplanes at high speeds without really having the faintest idea ourselves, but perhaps we at least shed a bit of light on the mystery.[159]

Other companies also had similar university relationships in Southern California. Malcolm Currie described the interaction between Hughes, UCLA, Caltech, and USC as bi-directional. Currie had taught classes on electromagnetism at UCLA and actively looked to hire his best students. Currie said "Caltech was the springboard for Hughes…A lot of the leadership, even in the research laboratories, came from Caltech, and some of them became professors there. They left the Hughes Research Laboratories and became professors." Additionally, professors from Caltech would come to work at Hughes. Hughes had a particularly close relationship with physicist Richard Feynman. Currie continued "When he got the Nobel Prize, we bought a red carpet and rolled it down and got a limo to drive him over from Caltech, and he came in and gave us an address. He was a real part of Hughes, and we encouraged that."[160]

Conclusion

Lockheed's focus on human resources through recruiting, management, and retention were indicative of a sea change in the American scientific endeavor. It was clear to Lockheed that recruiting top-quality talent was a tier-one priority. While universities and government moved much slower in accepting modern scientific realities, industry was more responsive and strategic to the shortage of scientists during the early Cold War. Lockheed's strategy, which was not unique to Lockheed, was to attract two streams of talent, first were established experts, and second were entry-level scientists. Similar approaches worked with both, but there were some fundamental differences.

---

[159] F. E. C. Culick, *Guggenheim Aeronautical Laboratory At The California Institute Of Technology: The First Fifty Years* (San Francisco Press, Inc., 1983); Willis Hawkins, "W. M. Hawkins to M. G. Kispert," March 15, 1955, Box 30(8), Willis M. Hawkins Papers. P.19

[160] Oral history interview with Malcolm R. Currie., 18, 21, 23.

The unique attractions to academic science were compensation, an extended career path, location (California), and active professional development. Lockheed also recognized that they also needed to compete directly with the attractions of academia and government. Government military postings offered job security and the opportunity to serve your country. Academia was characterized by self-direction, job security (through tenure), and prestige.

Job security was addressed by a system by which employees could be moved to projects within Lockheed, or loaned out to other stakeholders when reductions had to be made. It was also useful to point to internally developed talent in leadership such as Kelly Johnson, Willis Hawkins, Ben Rich, and Roy Anderson to show that Lockheed valued its employees.

Service to the country was inherent in military weapon construction which was the primary business of LMSC, the Skunk Works and GELAC. For those at CALAC or at non-military subsidiaries, the close relationship with the military groups was often enough, although even Willis Hawkins took time away from CALAC to serve the Army. This argument was especially powerful to prospective employees like Leighton Davis, Pete Quesada, Gene Root, and Willy Fiedler who had *already* served their country working for the military.

The attraction of a traditional college research environment was challenging to overcome. It required Lockheed to promote self-direction, which was often difficult in a hierarchical and corporate environment. Sometimes *limited* self-direction was all that was needed. Lloyd Chase, Douglas Moffat, and Nicholas Huff had all been recruited away from academia. In the cases of Chase and Moffat, Lockheed had encouraged publishing. In the other extreme, Nicholas Huff had been offered an entire department at Stanford and a dual posting at Lockheed with Lockheed picking up the tab. But this comparison was indicative of how the strategy focused on what entry-level vs. established scientists valued. Compensation and a career path only needed a sheen of academic respectability for entry-level recruits, but established scientists required true self-direction and the prestige of an academic role.

Lockheed had other strategies to compensate for the perceived loss of academic prestige. One was an encouragement to collaborate with academic scientists both at Lockheed, and by spending time on campus teaching. This had the dual advantage of allowing industrial scientists time in the college environment and getting the option of industrial work in front of students early in their education. Finally, Lockheed closely managed their scientific reputation in both academia and in industry so prospective employees felt like they had at least some form of scientific respectability. Once recruited and embedded, these scientists needed an environment that encouraged innovative thought. These environments are, in the terminology of the Triple-Helix, innovation networks, and that is the topic of the next chapter.

Chapter 4

The Development of an Innovation Networks

This chapter examines the requirements necessary for an environment where innovation is encouraged and not stifled. In the design and development of the Lockheed Missile and Space Company, Willis Hawkins understood that such an environment would require a strong internal and external communications system for output circulation, it would integrate undirected science into directed programs, and it would protect researchers from the drudgeries of resource management. The chapter also examines the government's strong push-back against the perceived cost of undirected science, and the governmental strategies to control cost through the lens of the C-5 Galaxy project.

At its core, an innovation network is an environment in which novel ideas are cultivated and encouraged. These networks formed in industry first because they were expensive and academia and government were more averse to financial risk. By the 1960's Lockheed was already known for internal innovation networks such as the Skunk Works, and for spinning off larger networks into new companies such as the LMSC. By the end of this decade, both Stanford and MIT lead the way among universities and had adopted the approach of industry and created a system which incubated both innovation and financial reward. This was the start of the entrepreneurial university, but they trailed industry's innovations in innovation. Lockheed, and specifically the LMSC, developed an innovation system in the 1950's primarily through the design and management of Willis Hawkins.

The need for innovation was inherent in Lockheed's mission to assist the defense department to maintain a technological advantage over U.S. adversaries. As a military contractor, Lockheed was expected to stay abreast of a concept called 'requirement'. This was the result of an analyses of enemy practice, and the options for an American military response or proactive operation. Unfortunately, these analyses were expensive and risky. They required extensive resources to create an environment where innovation was nurtured. Willis Hawkins was aware that the traditional model of science (the Linear Model) which was used by policymakers did not have a space for these innovation-positive environments, so he was politically careful to ensure that advocates of innovation networks were placed throughout the stakeholders of the Triple-Helix. After a Lockheed board of governors meeting in 1960, Willis Hawkins explained that:

> We are thin at the Requirements business as part of our responsibility to create good military systems. This responsibility not only required that we have these analytical methods and people, but also must participate at the highest levels in our government in order that we can see the beginnings of new requirements. We must be close to the people who have to fulfill the requirements, close to the Intelligence agencies, and close to all the agencies of the government which affect our future in military systems. Thus we are not only in the Operations Research business itself, we must be participants in national military activities. This is the reason you will find Gene Root, for instance, on the Scientific Advisory Board to the Air Force and you will find other members of the California, Georgia, and Missile and Space Divisions on other governmental committees.[161]

Even with political and corporate support, the expense of basic, undirected science necessitated quantitative outcomes to maintain government funding. The government used this funding control to influence and often suppress research freedom and direction in industry. Over the course of the 1950's and 1960's the relationship between government and industrial science deteriorated to a point where

---

[161] Willis Hawkins, "The Environment for Missile and Space Business."

both stakeholders were adversarial. This loss of trust resulted in the government implementing harsh procurement policies which directly and indirectly suppressed innovation at Lockheed.

Building an Innovation Space at Lockheed Missile and Space Company

The Triple-Helix model describes three components of a modern innovation network. These three categories, the innovation space, the knowledge space, and the consensus space, are complementary intellectual frameworks which encourage innovation. All three are scalable and offer physical and cultural manifestations. Innovation spaces are institutional concepts which transcend the ideal of knowledge development for technology generation, dispersion, and use. They promote entrepreneurial, social, cultural, and policy uses for science. Contemporary examples are science parks for university spin-off companies, and partnerships between municipalities and universities for economic renewal. The knowledge space is a networking system which connects scientists within and between communities to strengthen science resources, build the knowledge base, and prevent duplication of research. While these mechanisms can be far ranging, some present-day examples are journals, conferences, colloquia, grant proposals, arXiv.org, and even conference poster presentations. The final component of an innovation network is a consensus space. This a group of interdependent organizations and individuals who accept consensus in goals, rules of discourse, and access to resources to promote blue-sky thinking for creative solutions to problems. Examples are government technology boards, industry economic development boards, and industrial R&D labs.[162]

---

[162] Marina Ranga and Henry Etzkowitz, "Triple Helix Systems: An Analytical Framework for Innovation Policy and Practice in the Knowledge Society," *Industry and Higher Education* 27, no. 4 (August 1, 2013): 246–54, https://doi.org/10.5367/ihe.2013.0165.

Willis Hawkins was aware of the importance of developing an innovation network to ensure that LMSC remained at the cutting edge of aerospace science. Hawkins had been instrumental in designing the structure of the LMSC and he was explicit in his writings that it should be a space were innovation should be expected and encouraged.[163] He wrote in 1955 that

> There are many who say that you cannot schedule invention, but the industrial technical team in this country have been doing it with increasing success for years and I see no reason why, with the help of a strong research and scientific effort, we cannot continue in spite of the complexities of the newer concepts.[164]

Hawkins employed a logical series of steps when designing the cultural and managerial framework of the LMSC. Initially, he determined that undirected scientific research, which he referred to interchangeably as "pure" and "basic" research was of paramount importance to innovation. He wanted to establish low-resistance pathways for the flow of information within the LMSC, and for the flow of information to external stakeholders. Then he planned to implement a culture which respected and encouraged invention which was not necessarily tied to development of technology. This included a series of management and self-management guidelines which would ensure that LMSC's scientists were able to innovate freely. Finally, he wanted to ensure that the scientists at LMSC were given the resources to ensure they were supported in undirected science, and thus innovation.

This was initially challenging since the first director of the LMSC was not prepared to move beyond a traditional hierarchical structure. Pete Quesada was a difficult manager and created around

---

[163] The Lockheed Missile and Space Company (LMSC) was originally the Missile and Space Division (MSD) of Lockheed Aircraft Company (LAC). Since the LMSC was a name and corporate structural change only, I refer to the division an company both as LSMC.
[164] Willis Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 8.

him a negative work environment. He was rude to his people (and members of the public), and had a penchant for firing people as a demonstration of power. In 1954, Herschel Brown was the technical director for the nascent LMSD. Quesada fired him on several occasions. The first time, Brown took Quesada at face value and was packing up his office when Quesada's secretary arrived with a request from Quesada to hurry up on a report. Brown was stunned to learn that being fired was common and only Quesada's way of saying he didn't like what you had said. On several occasions throughout 1954 and 1955, Hawkins attempted to sway Quesada to design the LMSC as an innovation center, but was rebuffed. In January of 1955, Quesada made Hawkins describe the entire rigid structure that Quesada had designed to the Lockheed board of directors. Behind the scenes, however, Hawkins persisted in evangelizing his ideas among his peers. Eventually in 1956 Quesada resigned. Ostensibly this was because he committed to move LMSC to Ashville, North Carolina and the rest of the company committed to an innovation space in Sunnyvale, California, but it was also apparent that Lockheed leadership agreed with Hawkins that the LMSC should be an advanced, science focused, innovation network. Quesada was forced out using the move as a convenient reason but he had failed Lockheed on several levels. His departure allowed Hawkins to design the science-based company he had desired.[165]

One of the guiding principles of Hawkins' design for LMSC was the elevation of self-directed research[166] to prominence in an industrial setting. In the early 1950's the Linear Model was the prevailing theory for the creation of scientific knowledge and the subsequent development of it into products. Even so, Hawkins knew that real science constantly moving information back and between

---

[165] Hershel J. Brown, Interview with Hershel J. Brown, February 12, 1982, 16, Box 75(13), Willis M. Hawkins Papers.; Willis Hawkins, "Address to the Lockheed Board of Directors"; Willis Hawkins, "Remarks to the Executive Committee Regarding the Stanford Move," January 18, 1955, Box 57(7), Willis M. Hawkins Papers.
[166] By "self-directed" I mean science which is directed predominantly by the primary investigator, and not primarily at a meso or macro level.

research and development. Having a core of pure research in the company would contribute to innovation through three primary roles in industry. The first role of self-directed science was to search for new principles and new properties of matter as a basis for the development of tools. Aerospace design was coping with a myriad of scientific, mathematical, engineering, and practical issues when designing aircraft to fly at immense altitude and speed, so Hawkins said that any new principles in this field would "suggest new tools or new systems for the accomplishment of tasks not now deemed feasible." He was willing to invest in knowledge development without it being coupled to specific desired outcomes.[167]

The second role of self-directed science was "to indulge in creative conjecture for the purpose of suggesting new directions for scientific exploration." Hawkins saw this as a logical progression from the first role described above because the recognition of new principles required a safe space for creativity and blue-sky thinking.[168]

The third role of self-directed science was "to be available to advise development programs and to provide assistance by means of research endeavors" Additionally there was a role for self-directed researchers to determine "whether or not a new functional concept is feasible." This indicates that Hawkins understood that knowledge flow was a two-way, or reciprocal, process in science, and that communication was essential between stakeholders to ensure the viability of ideas.[169]

These three roles, developed by Hawkins in the early 1950's and implemented in the design of the LMSC, are homologous to the three factors characterizing the innovation space defined by the

---

[167] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 3–5.
[168] Hawkins, 3.
[169] Willis Hawkins, "W. M. Hawkins to E. R. Quesada - Comments on the Logical Division of Responsibilities in MSD Organization," May 13, 1954, Box 30(8), Willis M. Hawkins Papers.

Triple-Helix Model. His first role for self-directed science described an innovation space or the promotion of science without regard to development. His second role described a consensus space, or the access to resources to promoted imaginative thinking. Finally, this third role for self-directed research described a knowledge space which encourages and coordinates critical communication between scientists.[170]

The Innovation Space

In late 1954 Hawkins had a perspective altering meeting with B.W. Marsh of Lockheed's Theoretical Analysis Dept. And Dr. Theine of the executive Office of Scientific Research (OSR). The OSR wanted Lockheed to focus on undirected research without an interest in the usefulness of study data. Marsh and Theine included Hawkins so he could make suggestions on research topics, but the meeting led Hawkins to develop plans for meso-level management. In 1955, he suggested that "An increasing respect for the theoretical contributions must be developed," and "Industry and the Military must give it tools and support for true research into the properties of matter and permit it the time for creative conjecture."[171]

Hawkins' understood that designing a structure for LMSC required broad themes for use at the macro, meso, and micro levels. He achieved this by developing a series of maxims which maintained open communication between stakeholders within a scientific regime, or as Hawkins called it, a

---

[170] Ranga and Etzkowitz, "Triple Helix Systems," 252–54.
[171] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 21; Willis Hawkins, "Meeting Summary Between W. M. Hawkins, B. W. Marsh, and Dr. Theine.," September 29, 1954, Box 30(8), Willis M. Hawkins Papers.

"system." Above all, a system needed to be flexible enough to accept "scientific changes due to scientific discoveries or be responsible for creeping obsolescence."[172]

Hawkins management principles also addressed his concern about the habits of mind he saw in academia. For instance, he warned against "The Entrenched Idea," which he described as the habit of discarding new ideas which had either been developed elsewhere, or by younger scientists. He wrote that new ideas "must have their day in the sunlight and the best ideas must then be used." He was balanced in his views, however, and warned against blind acceptance of new research when the motivation behind it was wrong. He called this "The Scientific Monument," which was the habit of some scientists and managers to solve problems in novel ways in order to self-aggrandize; by leaping "into some new and greener pasture when a tried and true solution might have worked the first time."[173]

As a participant in corporate culture, Hawkins understood undirected science could not be completely unfettered. He limited his commitment to freedom in direction by requiring broad timeframes to be built around projects. He believed that inventiveness was defeated by both short *and* indeterminate timeframes, but adherence to a self-directed scientific core would save time in the long-run if managers defined broad tropical areas for study by individual scientists. He recognized that the the older scientific model which was focused on empirical testing (undirected deduction through observation) did not reflect modern scientific process which required an inductive and deductive process that began with a defined problem. He wrote that the "scientist must continually be asked to

[172] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 6.
[173] Hawkins, 11–12.

explore the theoretical world for ideas which will limit endless searching for fundamental knowledge through haphazard testing."[174]

Throughout the 1950's and 1960's, Hawkins did not change his commitment to ensuring that LMSC promoted an innovation space. In 1966 he wrote to R. S. Shairer and described the scope of science within Lockheed and again promoted basic science as a path to innovation. He wrote that:

> There are certain areas of growth in science and science application that may be of importance to Lockheed. Included are: Biology, space and marine life support, particle physics, solid state physics and quantum electrodynamics. Revolutions in communications, improved computer utilization, sensor technology, and new composite materials for lighter stronger structures are at hand. It is obvious that we must stay on top of these developments as well.[175]

Hawkins emphasis on developing LMSC into an innovation network brought respect and prestige to Lockheed. Bell Labs, also famed as an innovation leader, sent their VP of Research William O. Baker to LMSC to learn from Lockheed. He wrote to Hawkins, "We have had many management problems in operating our own research laboratories, and we had hoped that we could obtain a few solutions to these problems by our visit. We were not disappointed."[176]

The government also turned to Lockheed as a think tank on social issues. In 1967, Hubert Humphrey asked Lockheed Executive VP Carl Haddon to apply the research resources at Lockheed to the issues of rioting in the inner city. Mathematician H.A. Linstone was given the responsibility of a study

---

[174] Hawkins, 9.
[175] Willis Hawkins, "W. M. Hawkins to R. S. Schairer Scope of Science and Engineering Functional Planning Study in Support of LMP-67," November 21, 1966, Box 33(1), Willis M. Hawkins Papers.
[176] William O. Baker, "Thank You Note," September 29, 1967, Box 33(3), Willis M. Hawkins Papers.

and came up with 4 suggestions for Lockheed: 1. the adoption of high schools, 2. the training of teachers in significant skills, 3. Study of new approach to the design of teaching and training programs for poor learners, and 4. an industrial demonstration, learn-and-work facility. Hawkins liked the results enough to propose to Haddon "that Lockheed consider a separate division whose responsibilities encompass studies of this nature, including the provision of services to local communities as advisory bodies, if systems are actually instituted as a result of these studies."[177]

The application of Lockheed's resources to social change did not always go smoothly. The emphasis on blue-sky thinking led to several projects in Lockheed's nuclear research which did not match societal desire at the time. The first of these was work on a nuclear-powered airplane. Lockheed's willingness to focus on basic research led the company to purchase 10,000 acres of land in the Georgia mountains near Dawsonville, which became the Georgia nuclear facility (a subsidiary of GELAC). Several nuclear physicists were led by Dr. Jim Flack to gear up for the project by testing the effects of radiation on airplane materials. Lockheed purchased land, built labs, and installed a reactor. Ultimately the project did not get as far as prototyping, but when Al Cleveland was asked about whether this was because of the societal concern about flying a nuclear reactor around in an airplane, he said:

> I don't think it really ever became as significant as it should have been. We did a lot of work with regard to how we were going to handle the airplane, but by and large we ignored or discounted the fact that an incident or an accident, crash, could be as drastic as it obviously could have been. Much later, after 1965, the NASA people started doing some studies on how to design a reactor which could survive a crash, or at least a

[177] Lockheed Corporation, "Ad Hoc Industrial Planning Committee," 1967, Box 30(2), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to M. C. Haddon - Proposal for New Division," August 28, 1967, Box 33(3), Willis M. Hawkins Papers.

reasonably credible crash. But, again, that really didn't ever become terribly significant.[178]

The disconnect between research and social change was exemplified further when Lockheed had preliminary discussions with Maurice Timbs of the Australian Atomic Energy Commission who wanted to dig a harbor in Cape Keraudren, Australia, with nuclear bombs! This was cancelled as soon as the media got hold of it. However, the areas of study within LMSC and other groups show that Lockheed was investing more in knowledge development than physical products. By 1966 a huge number of LMSC's products were feasibility studies. Lockheed even considered purchasing a company which did nothing but labor planning for military and large corporate installations (e.g. how many crew you need on a new submarine, what their tasks and schedules should be etc.).[179]

Lockheed's development of innovation spaces also led to examples of entrepreneurialism among scientists. This was a relatively new development and manifested itself in several ways. From a financial perspective, Lockheed engineer Larry Edwards did a significant quantity of research in the 1960's on a vacuum train. This was an electric monorail which he proposed would run along a tube between Los Angeles and San Francisco (or other major cities). The vacuum would reduce the friction of air and allow much greater speeds. Edwards left Lockheed in 1967 and requested to purchase the rights to this research so he could found a company on the idea. Gene Root eventually sold the rights to Edwards for $10,000 and wished him well.[180]

---

[178] F. A. Cleveland, Interview with F. A. Cleveland, 18.

[179] F. A. Cleveland, 20; Root, "L. E. Root to D. Haughton and A. C. Kotchian," September 23, 1966.

[180] L. E. Root, "L. E. Root to L. K. Edwards," December 16, 1966, Box 33(1), Willis M. Hawkins Papers.; Root, "L. E. Root to D. Haughton and A. C. Kotchian," September 23, 1966; L. E. Root, "L. E. Root to D. J. Haughton," November 18, 1966, Box 33(1), Willis M. Hawkins Papers.

Another form of researcher-level entrepreneurialism resulted in financial support for a researcher's lab instead of the researcher themselves. For instance, chemist Y. W. Touloukian approached Lockheed with an idea for a thermophysical properties research program. The idea "met with approval throughout the organization," and Hawkins wrote to Kelly Johnson to suggest that the research would have such value to Lockheed, that they contribute $2500 to Touloukian's lab at Purdue.[181]

The Knowledge Space

Within an innovation network, the knowledge space represents a system which encourages communication of knowledge between stakeholders. In the Linear Model, this communication is uni-directional, but Hawkins understood that in practice new knowledge moved back-and-forth between stakeholders at all three scales, and between triple-helix actors until a consensus formed that the new knowledge was acceptable. This was especially important for Hawkins since he envisioned the three branches of the LMSC, Research, Engineering, and Program Development, acting in concert with each other and having multiple projects existing across all three branches at any given time. This would require each project to have a discrete, coherent, transparent, and accessible system of communicating knowledge and ideas across all three branches.

He said that in his concept, "no system can be designed or developed until basic research has been completed to such a state that the end result is predictable for every component of the system. By this concept, a total system cannot be proposed until research makes possible a comparable state of the art for all of the system components." By this he meant that basic research was necessary to set a

---

[181] Willis Hawkins, "W. M. Hawkins to Y. W. Touloukian," May 1, 1956, Box 30(10), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to C. Johnson," May 22, 1956, Box 30(10), Willis M. Hawkins Papers.

context and to define boundaries around any scientific regime. The Program Development Branch acted as both liaison to higher management, and managed information flow.[182]

Hawkins suggested that meso-level managers be dedicated to designing and managing the framework for information flow in systems. For this, he offered more management principles. The first was that the system designer/manager must always seek the counsel of the basic research teams in the solution of complex system problems. Since basic research set the boundaries of a system, the initial basic research team had unique insight into the system. This was also a way to avoid the situation he called "The Impossible Concept." The was the case where system designers tend to not recognize the limitations of a system. While this is entirely expected and appropriate in the pure-science realm, Hawkins' systems needed boundaries. Staying in regular conversation with stakeholders ensured that these boundaries remained consistent.[183]

Hawkins final goal was to ensure that technical information was successfully communicated to non-technical management. Since the opportunity to promote inventiveness through an innovation network was controlled by managers above operational levels of the corporate hierarchy, it was essential to ensure they had a role in the knowledge space. Unfortunately for Hawkins, Quesada did not agree with his plan. In 1954 Quesada determined that the groups under the umbrella of the LMSC would be organized in a traditional, engineering-oriented fashion. The Engineering Branch would be responsible for research and development in aerodynamics and thermodynamics, power plants, materials research and development, and airframe engineering. Intercontinental Ballistic Missiles would

[182] Hawkins, "W. M. Hawkins to E. R. Quesada - Comments on the Logical Division of Responsibilities in MSD Organization," May 13, 1954, 1; Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 7.
[183] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 11, 21.

be managed in their entirety by the Research Branch. Effectively, Quesada had created two branches with almost identical day-to-day responsibilities, albeit regarding different weapons systems. In early 1955, Quesada went beyond merely rejecting Hawkins' design plan and required Hawkins to outline the Quesada plan to the Board of Directors meeting. Hawkins did so, but later that year was invited back to the executive committee to comment on the move decision (Asheville or Sunnyvale) and the staffing issues of LMSD. Hawkins did not hesitate. He said that Pete Quesada had failed to staff LMSD appropriately and it became less of a research group than an airframe assembly group for missiles. Additionally, Hawkins said that Lockheed must speed up the move to Sunnyvale (which he tellingly called Stanford) so a space for the innovation and research needs of the aerospace arm of Lockheed could be built. [184]

Consensus Spaces

Consensus spaces are created at an organizational level to encourage science unfettered by the goal of practical application. Essentially it is an organizational investment in undirected research. Since the profit motive is a key factor in corporate decision-making, and payroll is routinely a key expense, Hawkins understood that creating a consensus space at LMSC would not be easy. He wrote that innovation "demands the detachment of purely scientific personnel from project-direction and it presupposes financial support will be coming from the Military Services…it must be accomplished before pure science can live and prosper in the industrial world."[185]

---

[184] Hawkins, "Address to the Lockheed Board of Directors," 16; Hawkins, "Remarks to the Executive Committee Regarding the Stanford Move."
[185] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 17–18.

He planned a four-pillared structure to protect his consensus space. He wrote that the "environment most conducive to scientific accomplishment and well-being. This environment, it seems to me, hinges about the word "freedom" –

1. Freedom from management problems
2. Freedom from time-scale restraints
3. Freedom from development tasks
4. Freedom to use all available tools in scientific exploration."[186]

Freedom from management problems required two approaches. The first was to convince Lockheed's leadership that this was an essential investment so researchers could be spared concern about resources. The second was to develop a series of management rules so that meso-level managers continued to encourage blue-sky thinking.

Initially, Hawkins sent a design of LMSC to Pete Quesada with a separate 'Program Development Branch' which he later referred to as the 'Design Branch'. It was responsible for the design, development, and administration of research systems. Part of the responsibility of this group was to manage the interaction between the research and development branches, but he also required them to "initiate and manage broad research programs to permit advances in the state of the art preparatory to implementing new system development programs." When Hawkins used the term "state of art," or "beyond state of art," he was referring to advanced research and innovation. By referring to innovation in commonly understood research terms, Hawkins attempted to make his organization design palatable to Quesada. Ultimately Lockheed leadership (without Quesada) acquiesced to Hawkins design because

[186] Hawkins, 17–18.

Hawkins argued that space programs were so filled with unknowns, a free research team was required to even define the boundaries of what *could* be known.[187]

In addition to acting as manager of information flow between the research branch, and the development, or engineering, branch, the design branch was a buffer between LMSC and other stakeholders. The military was to act directly with Program Development, and not with research for the development and fulfillment of requirements. Program Development was also responsible for suggesting requirement to the military after proposals had come up from research. Additionally, the design branch would act as a liaison with the Lockheed corporate entity. In essence, Hawkins had removed the administrative tasks from researchers so they could focus research.

Freeing scientists from time-scale restraints was hard because research time was billed to the military, and if they refused to pay, Lockheed would bear the costs of the overhead of pure research. Additionally, the tight timeframes around delivering on military requirements were traditionally inflexible. Hawkins understood these issues and laid out a series of balanced principles to prevent managers from distracting scientists. He urged managers to avoid tight deadlines on innovation or related job responsibilities. The tyranny of urgency, Hawkins said, would prevent researchers from being able to "indulge in long-range, creative conjecture – they will be too busy inventing to a rigid and impossible schedule."[188]

---

[187] Hawkins, "W. M. Hawkins to E. R. Quesada - Comments on the Logical Division of Responsibilities in MSD Organization," May 13, 1954, 4–5; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957.

[188] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 5; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957, 3.

Hawkins also saw most research scientists as mediocre administrators and developers. Administration was a tricky subject because scientists were proud of their intellects and suffered from "nobody can do it like I can syndrome." It pained Hawkins to think of the amount of time eminent scientists spent "laboriously pecking out technical copy with two fingers while stenographers smoke and project administrators wait for reports to be completed." In the same way, scientists who were excellent researchers but inexperienced engineers often struggled in developing research into products. It was much more efficient for Program Development to move that job over to the engineers at the Development Branch. This issue was then compounded by the addiction line-executives had with promotions. Even if managers had come from the technical or research ranks, they assumed that everyone else wanted promotion, so they would look to promote the best performers. It had been Hawkins experience that this usually pulled an outstanding researcher out of the lab to create a middling-quality manager. The only cure for this, according to Hawkins, was to pay scientists for success *without* adding leadership or administrative responsibility to their job description.[189]

Hawkins' goal to ensure the freedom to use all available tools in scientific exploration was a roundabout way of ensuring that the consensus space extended beyond the walls of Lockheed to the intellectual resources in the area. Hawkins had already made it clear to the Lockheed executive committee that he greatest advantages of moving to Sunnyvale were the proximities of Stanford University and NACA Ames Research Laboratory. Not only was the relationship between these institutes

[189] Hawkins, "New Responsibilities of Science and Engineering in Weapon System Development," 16, 19; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957.

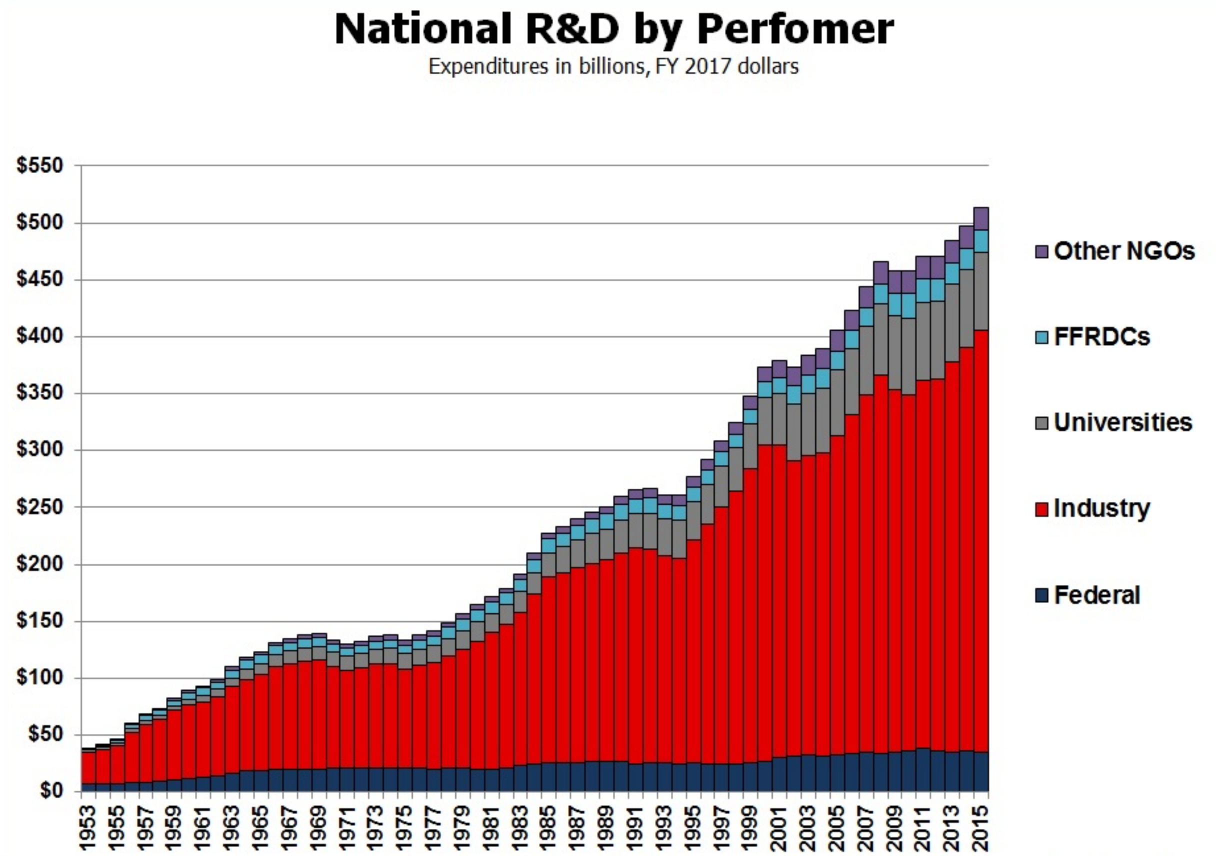
and Lockheed a boon for recruiting, but Lockheed's encouragement of their researchers to teach and work in an academic environment significantly increased the size of Lockheed's consensus space.[190]

Financing Innovation Networks

The large-scale science which emerged in the post-war period required large-scale budgets. The cost of producing the first four atomic weapons in the United States was approximately $40 billion (in 2018 dollars) and employed more than 130,000 people. Between 1940 and 1996, the US Government spent more than $5 trillion on the US nuclear weapons effort alone. Including nuclear work, the national investment in both basic and applied R&D between 1953 and 2015 was $14.942 trillion, with industry receiving more than $10.5 trillion of this total (see figure 2).[191]

Figure 2 - National R&D by Performer

[190] Hershel J. Brown, Interview with Hershel J. Brown, 10; Hawkins, "Remarks to the Executive Committee Regarding the Stanford Move."
[191] Stephen I. Schwartz, *Atomic Audit: The Costs and Consequences of U.S. Nuclear Weapons Since 1940* (Brookings Institution Press, 1998); American Association for the Advancement of Science, "Historical Trends in Federal R&D," AAAS - The World's Largest General Scientific Society, 2018.



Source: Source: National Science Foundation, *National Patterns of R&D Resources* series. Constant-dollar conversions based on GDP deflators from *Budget of the U.S. Government FY 2018* © 2017 AAAS

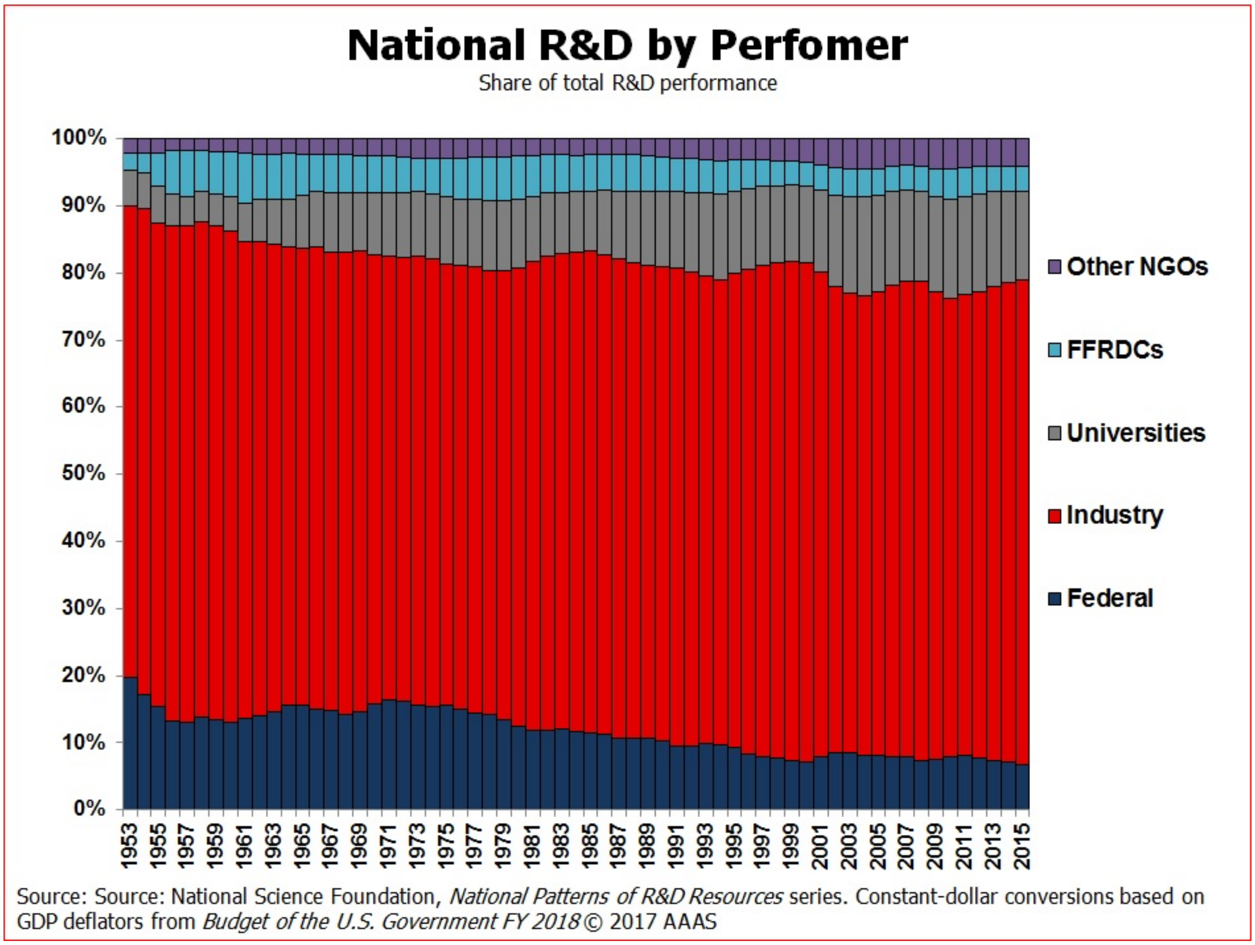


Figure 3 - National R&D by Performer

When analyzed as percent of total budgeted funds, industry has received just over 70% of all R&D finding since 1953 (see Figure 3).

As a percentage of the federal government's total budget, R&D peaked at almost 12% in 1966 but by 1975 had fallen to around 5% where it remained for the next forty years. Only in 2016 did the total drop to 3% with the bulk of the decrease absorbed by defense R&D. Non-defense R&D has been stable at around 2% since 1980.[192]

Even at the most macro of scales, these are large amounts of public money and the U.S. Government was obligated to ensure that it was spent carefully. Unfortunately, the post war period offered only an IR&D system which was built predominantly on trust and goodwill among triple-helix stakeholders. As the federal R&D budget grew explosively in the early 1950's, the Independent Research and Development (IR&D) system was born. Hawkins based his plans of creating an innovation network at LMSC on the IR&D funding system in place. When the government transformed the funding system into Total Package Procurement in the 1960's, funding for self-directed science became scarce and innovation withered.

IR&D

Independent Research and Development in very general terms was the part of a contractor's research and development program which was not under a direct contract or grant. It was self-directed and self-initiated to assist the company to complete otherwise contracted projects.

[192] United States Government, "Historical Tables, Budget of the United States Government, Fiscal Year 2018" (Office of Management and Budget, May 23, 2017), https://www.govinfo.gov/app/details/BUDGET-2018-TAB/summary. table 9.7, p. 190

IR&D involved a complex back-and-forth between the government and the contractor. Generally, these interactions occurred along a set structure. A research bid would be requested by a contractor to begin research into a specific area of national importance. In the early 1950's this was generally national defense. A modest grant would be made and contractor would begin researching "requirements". Cost overruns were so common that they were usually assumed and "overhead" was included as an expectation in the grant. Even so, the government required extensive documentation to issue overhead reimbursements. When a requirement was accepted by the government, a request for proposal (RFP) would be published and multiple contractors would be invited to bid. The cost for bidding on RFP's was (and continues to be) significant, so the government would award IR&D grants to all approved contractors under similar terms to the initial requirement research. When a bid was finally accepted, all previous research that was funded under IR&D was shared with the winning contractor and yet another contract was made to complete IR&D and begin production of a product if applicable. The cost of this system was very high, but aerospace contractors were statutorily restricted to a maximum of 15% profit. Until the late 1950's actual profit was capped at 7% by tradition.[193]

The inherent trust required by a system that allowed the contractor to bill for all hours work on a project made overruns a norm. This irritated the military, but the enormous power of funding made internal subsidizing a common tactic with aerospace companies. For example, the Air Force called for a $152,990 contract on arming and fuzing of nuclear warheads in 1955, but in 1957, changed the scope of the contract. The Air Force was unhappy with the delivered results from Lockheed and blamed Lockheed for using inferior personnel. They had hoped it would be worked on by high level technical engineers,

---

[193] Lassman, "Putting the Military Back into the History of the Military-Industrial Complex: The Management of Technological Innovation in the U.S. Army, 1945–1960"; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957.

but more than 60 people at Lockheed billed time to the contract. Ultimately the Air Force required revisions which would eliminate all profit which was only 7%. Lockheed's internal recommendation was to comply to ensure other contracts were not negatively affected. Both military relations department manager Warren D. Orr and Willis Hawkins recommended absorbing the loss. Orr was trying to maintain Lockheed's strong relationship with the Air Force. For Orr this was an absorption of roughly $8000 in overruns, but left Lockheed's remaining contracts, with profit expected to be over $14,000, intact and safe. Additionally, LMSC was in the running for another two contracts for up to $164,000 each. Hawkins agreed with Orr's financial math, but added that the Air Force might have been right and that Lockheed could have done better. He admitted that he had "reservations on our own performance."[194]

The 7% profit tradition changed with Lockheed because of the Polaris missile project. The Polaris submarine launched missile was a requirement contract won by Lockheed's new Missile and Space Company (LMSC) in 1955. Unlike other Lockheed ventures which were predominantly constructed by Lockheed companies, the LMSC subcontracted the majority of work to other aerospace specialists. For instance, the newly launched TRW corporation worked on guidance systems, Hughes on fire control, and Aerojet and Hercules collaborated on Poseidon's propulsion system. In fact, only 3% of the gross unfueled weight of a Polaris missile was created by Lockheed.[195]

The immense complexity of managing high-technology weapons systems required a new perspective on profit motive. For Poseidon, Lockheed charged the U.S. Navy 10-12% profit. There were several justifications for this presented by Willis Hawkins in 1957. He suggested that:

---

[194] Warren D. Orr, "W. D. Orr to W. M. Hawkins," July 2, 1957, Box 30(11), Willis M. Hawkins Papers 1920-2009.
[195] Orr; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957.

1. With so many subcontractors, Lockheed would not create its own tooling that could be amortized over years.
2. Missile productions runs were short lived compared to aircraft so profit cannot be counted on in the long-term.
3. Most of the test equipment created is specific for these limited production runs and cannot be reused for other projects.
4. Many highly trained and expensive researchers are used in parallel on these high-technology projects so IR&D can deliver a state-of-the-art product with great speed.
5. Stability in the company must be externally visible to attract highly skilled researchers.

The Navy supervised this complex project through the newly created Special Projects Office (SPO) operated by Rear Admiral William F. Raborn. Raborn and Captain Levering Smith the head of SPO's Technical Division, publicly agreed with Hawkins, and took the position that the technical challenges faced in the project justified contracts based on level of effort rather than completion of contracted goals. Behind closed doors, however, Raborn was frustrated that production systems tended to be idiosyncratic and production could not be moved from one contractor to another without reliability suffering. To promote consistency and urgency in project work, the SPO in conjuction with Lockheed, and the consulting firm Booz, Allen, and Hamilton developed an innovation management system called the Program Evaluation Review Technique or PERT. PERT mapped out the required R&D timelines for each contract family within a weapons system, then required scientists and engineers to forecast best, expected, and worst-case timeframes for completion. The SPO developed an algorithm to combine these timeframes and predict both and most-likely case, and how that case would be affected by increased man-hours.[196]

In practice, however, PERT required systems designers to forecast innovation and flawless time management among scientists and engineers. It was used more as a screen to divert oversight of the SPO by presenting SPO projects as running on perfectly mapped timelines. These requirements were antithetical to Hawkins' requirement to free scientists from the tyranny of time-scale restraints. How then was Lockheed and the SPO able to produce Poseidon missiles on time and under budget? This was predominantly due to three factors. The first was outstanding technical knowledge among leadership. Levering Smith ran the missile programs for the Navy from 1957 until 1977, and Willis Hawkins was an aeronautical engineer before moving to management. Additionally, Poseidon was a high-priority program for the Navy so Raborn was able to source talent from other projects. Hawkins too was a great proponent of hiring the best talent. The second influence on innovation was decentralized and competitive bidding among subcontractors. Hughes, as an example, had outbid the giant General Electric to produce the fire control systems. These were produced in half the expected time at a significant savings by minimizing red tape and administrative overhead. Finally, the sense of community fostered by Hawkins and Raborn ensured excellent communication within the system. Both men

---

[196] Willis Hawkins, "Proposed Structure for Polaris Submarine Special Task Group," 1957, Box 75(8), Willis M. Hawkins Papers.; Elliott Vanveltner Converse, Walter S. Poole, and United States, eds., *History of Acquisition in the Department of Defense* (Washington, D.C: Historical Office, Office of the Secretary of Defense, 2012); Shannon A. Brown, *Providing the Means of War: Historical Perspectives on Defense Acquisition, 1945-2000* (Government Printing Office, 2005).

cultivated friendships among all levels of the Triple-Helix and acted as conduits of information and connectors of resources.[197]

Despite PERT impairing the consensus space in the Poseidon project with rigid time constraints, the knowledge space and the innovation space expanded and strengthened to compensate. Raborn and Smith were both engineers and understood the complexity and difficulty of mandating innovation, so they looked to create innovation networks within the project environment of SPO contracts. There was also a consensus at the time that PERT was designed more to "flim-flam" policymakers and allow Raborn to protect the IR&D system and get on with the project with limited oversight. Unfortunately, not all the branches of the military were as understanding to the nuances of IR&D-based funding.[198]

Military and Government Resistance to Innovation Networks

Unfortunately, the IR&D system faced structural hurdles for funding since the military procurement policy was built on three foundational tenets which worked against innovation communities: these were the Linear Model, the assumption of profitable products, and the budget year. The Armed Services Procurement Regulation 15-205.35 explicitly defined the allowability of compensation for IR&D based on the Linear Model that pure research becomes developed products. Under 15-205.35, all of independent research was allowed under an IR&D contract was allowed, but only a portion of the development work. This system was in place to prevent companies from double

---

[197] Schwartz, *Atomic Audit*; Willis Hawkins, "W. M. Hawkins to L. E. Root - Discussion with Admiral Pearson RE: Use of Second Stage Polaris as an Air-to-Surface Missile," May 31, 1957, Box 33(11), Willis M. Hawkins Papers.; Hershel J. Brown, Interview with Hershel J. Brown, 15.

[198] Walter S. Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, History of Acquisition in the Department of Defense; Volume II (Washington, D.C.: Historical Office, Office of the Secretary of Defense, 2013), 251–52, http://catdir.loc.gov/catdir/toc/fy15pdf02/2013017876.html; For a detailed analysis of the Poseidon project, see Harvey M. Sapolsky, *The Polaris System Development; Bureaucratic and Programmatic Success in Government* (Cambridge, Mass.: Harvard University Press, 1972). This book is still relevant today since it details an early achievement in systems design and implementation.

dipping and profiting off the government for both development and the product which was sold to the government after development ceased and production began. Unfortunately, the Linear Model did not represent the process of scientific knowledge development in IR&D, and both industry and the military were left disgruntled. The military felt that industry was running up expenses, and industry was regularly paying for overruns themselves. Limiting resources undermined opportunities for consensus spaces, and the Linear Model restricted the dynamic communications necessary in a knowledge space. The Linear Model also presupposed practical development as an outcome for research and therefore weakened innovation spaces.[199]

The annual fiscal budgets exacerbated the issue, since both contracts and budgets for the military were annually renewed at the legislative level. The issue with the system was that the military would agree to a budgeted IR&D expense during a given year, but there was no way to corral the projects into calendar-year block. So, when time ran over, Lockheed included that in the next year's budget, but the military is annually capricious so if it was not billed in 1965 on a 1965 budget, the research must continue in 1966 (and perhaps 1967), at Lockheed's expense. This caused Lockheed to implement strict time controls and limited the opportunity for consensus spaces.

By the mid-1960's the incompatibility between how the military expected IR&D to work, and how industry actually ran it caused enough friction that Willis Hawkins wrote to lament the end of IR&D as early as 1966. He began suggesting to other Lockheed leaders that they should only bid for contracts with inherent IR&D approval clauses. The military, on the other hand, developed an approach which clearly established the contractor and military as adversaries: Total Package Procurement (TPP).

---

[199] United States. Deparment of Defense, *Armed Services Procurement Regulation Manual for Contract Pricing*, 1 v. in various pagings (Washington, D.C. : Dept. of Defense : For sale by the Supt. of Docs., U.S. G.P.O., 1969), 1519.

Total Package Procurement

Total Package Procurement started in 1964 as a reaction to the immense costs of IR&D. The idea was presented to Secretary of Defense Robert McNamara by Assistant Secretary of the Air Force, Robert Charles. Charles had been an executive vice president of McDonnell Aircraft and had been frustrated by the habit of other aerospace companies bidding low and then finding profit in IR&D overhead billing. His idea was simple, make the contractors bid for design, development, production, and support at one single price. Any overruns were the responsibility of the contractor. McNamara, supported the concept enthusiastically. He had been frustrated by the tolerance in the system for “iceberg procurement” where companies would only bid on the costs of the visible contract when they knew full well it would cost far more. TPP would have six core components:

1. Providing firmer 5-year force structure program package planning information concerning performance cost and schedules;
2. Discouraging contractors from "buying in" on the design and development effort with the intention of recovering on the subsequent production program;
3. Permitting program decision and source selection based on binding performance, price and schedule commitments by contractors for the total program or major part of it;
4. Providing a firmer basis for projecting total acquisition and operational costs for use in source selection, and in the determination of appropriate contractual incentives;
5. Motivating contractors to design initially for economical production and support of operational hardware which may not receive sufficient emphasis in the absence of productions commitments;

6. Requiring contractors to assume more responsibility for program success, thereby permitting the Government to monitor programs more in terms of surveillance and less in terms of detailed management.[200]

TPP was only applied to a few large-scale weapons systems: the C-5A Galaxy Cargo Plane, the LHA amphibious assault ship, the AGM-69 Short Range Attack Missile (SRAM), the Maverick missile, the AH-56 Cheyenne attack helicopter, and the Spruance class destroyers. The result was that it stifled innovations and creative technology. Like PERT, the concept was reliant on the premise that the military and the contractor could accurately forecast what technologies would be needed, how long they would take to develop, how much work was involved in developing these technologies, and if the Linear Model would move research to development to production in a timely manner. None of these things happened and the contracts awarded under TPP were even more costly than before. An example of how TPP stifled innovation is the Lockheed C-5A Galaxy.[201]

The C-5 Galaxy

When Robert Charles obtained approval to move forward with TPP, he convinced the Air Force that the upcoming bidding process on the C-5A would be a strong test case. The C-5A was to be a turbofan powered transport capable of enormous payloads and range. It was ideal for TPP, Charles argued, because it was based on proven technology, and had performance requirements which were both reasonable and attainable. In 1964 these requirements resulted in a Specific Operational

---

[200] *The Code of Federal Regulations of the United States of America* (U.S. Government Printing Office, 1969), 63; Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 50–77.
[201] Brown, *Providing the Means of War*, 82–83; Jack V. Michaels and William P. Wood, *Design to Cost* (John Wiley & Sons, 1989), 238.

Requirement (now called an RFP – Request for Proposal) which was more than 1500 pages long. Lockheed, Boeing, and Douglas were invited to bid, and between the three produced bid packages of more than 240,000 pages.

Lockheed's proposal was strongly encouraged by Lockheed Aircraft Company (LAC) president Daniel Haughton. Haughton had been recruited to Lockheed by the Gross brothers in 1936 as a systems engineer and by 1951 he was managing the Georgia Lockheed Aircraft Company (GELAC) which had just put a bid in to build a turboprop driven transport called the C-130 Hercules. Ultimately Lockheed would produce more than 2500 of these aircraft. In 1960, still with GELAC, Haughton bid on the C-141 Starlifter and again won the contract. The C-141 was a turbofan[202] powered aircraft and was, in the eyes of both Charles and Haughton, an obvious forerunner to the C-5.

Unlike Charles, Haughton had been successful in finding profit in the IR&D system. Not all employees at Lockheed were comfortable with the ethics of this approach and Herschel Brown remembered clashing with Haughton on several occasions. He said:

> "He and I had some tough talks about bidding. I was a great one for going gung-ho for the full cost and getting our emoluments though performance. He had, through his airplane days, relied on the fact that he would rather bid low and get it back in change orders… Dan and I had a time or two…"[203]

---

[202] Turbofans are part of the jet engine family. Turboprops which have a propeller (and which powered the C-130) and turbofans (which powered the C-141, C-5, and most commercial jetliners) are much more fuel efficient that turbojets which power fighter jets, but operate on the same fundamental principles of a jet engine.
[203] Hershel J. Brown, Interview with Hershel J. Brown, 16.

Haughton was generally able to overcome the concerns of others by using personal warmth and community building. In the same discussion about disagreeing with Haughton over bidding, Brown remembered that:

> I was always amazed that, as we walked around the plant, old timers from LAS or California would say "Hi Dan," and Dan would say "Oh John, how's your wife Mary?" This staggered me, how many people that he knew so intimately and so well and so highly regarded. It was a real inspiration to walk with Dan.[204]

In addition to strong leadership with robust internal communication, the GELAC team had strengthened since Willis Hawkins had suggested that Al Cleveland should move to Georgia. Much of it was based on strong communication and teamwork. Cleveland remembered that the production of the C-141 was successful more from teamwork than any other single factor. He said "we knew all the technical things that one ought to do; we just didn't have the experience of having done them. And… that's a very serious deficiency, but the Georgia teamwork was one of our great attributes." This teamwork was based on good people at every level. Chief Engineer Art Flock for instance was "a very fine, competent engineer, with a lot of feeling for the human interests of his people, and I thought he was one of our top engineering leaders."[205]

With a confidence founded on a strong team, experience, expertise, and ample resources, Haughton felt that he was in a position to enter into the first TPP contract as prepared as he could be. The Air Force agreed and in October of 1965, Lockheed was granted the contract for 115 aircraft. The price target was $1.945 billion with an absolute ceiling of 130% of the price target. However, there were

---

[204] Hershel J. Brown, 16.
[205] F. A. Cleveland, Interview with F. A. Cleveland, 28–30.

differences between this agreement and previous contracts. The first was that any and all faulty performance, even on behalf of the government, was to be borne by Lockheed alone. Charles saw this as a booster of innovation in both economy and knowledge development. Others were not as sure and through 1967 three separate studies were performed on the effect of TPP on innovation. Dr. Finn Larson, deputy director of defense research and engineering, and the Logistics Management Institute determined in 1967, that there were not innovation losses within the scope of the project, but innovations outside the scope were clearly limited. This was not an issue for Charles. He explicitly wanted an end to the generation of private profitable knowledge on the public's dime. However, both studies faulted the extended timeframes of the project. The contract was for seven years, instead of the normal two, and since the terms were inflexible, it did not account for changing needs of the military. Moreover, if changes were made it would be on the contractor to pay for them and that would jeopardize the outcomes of the entire project.[206]

From the perspective of Lockheed, the rigidity of the contract stifled innovation through time pressure, and scarce resources. It also created an adversarial relationship with the Air Force, and even internally within GELAC. An example was the power to weight ratio. The C-5 was to have four times greater payload than the C-141. Early in 1966, it was clear that the prototype would be 12,000 pounds overweight. The simplest solution was to increase the thrust of the engines by 10% and ask General Electric to design and build a stronger powerplant. However, Charles argued strongly against this saying that the reeked of "iceberg" science and insisted Lockheed maintain the specifications they originally agreed to. A cure notice was sent from the Air Force to enforce the decision. Lockheed was forced to accept the entire cost of lightening the aircraft. They did thus through chemical milling of the structure

---

[206] Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 74–76.

of the wings. Unfortunately, this solution caused the wings to crack in prototyping, the Air Force added another layer of bureaucracy to the project by appointing a Defense Acquisition Guidebook (DAG) Modification Committee which was ostensibly there to aid Lockheed in solving the wing issue. [207]

The DAG Committee was comprised of members from the government, academia, and industry (including direct competitors). It delayed decisions and aggravated the already tense relationship between Lockheed and the Air Force. In February 1966, Willis Hawkins was sent to GELAC to see if he could help. He arrived for a terrible meeting between GELAC leadership and the Air Force which meant that Hawkins had to take over direct interactions with both the Air Force and the DAG Committee. He wrote in his diary "AF meeting TERRIBLE. I am a task force to help GELAC talk to AF."

In March 1966, Hawkins told the C-5 System Program Office (SPO) and the DAG committee together that TPP was "inhibiting invention." The SPO was very concerned about this since Charles and the Department of Defense had been promoting TPP as encouraging innovation. The DAG Committee's response was to request of Lockheed a verification from Lockheed that they had enough profit margin left to fix the wings.[208]

In June of 1968, Charles told congress that the project was going to exceed its contracted ceiling. By September of 1968, the SPO was forecasting a total project cost of $2.436 billion. Even so, 57 more aircraft were added to the order and the project ended up more than $2 billion over its initial price. For a company whose gross revenue was about $2 billion, this was a devastating development and the total

---

[207] Perreault, Interview with Roy Anderson, 10–11; F. A. Cleveland, Interview with F. A. Cleveland, 32, 38–39; Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 76.
[208] Willis Hawkins, "Diary Entry 2/17/1967," February 17, 1967, Box 2(1), Willis M. Hawkins Papers.; Willis Hawkins, "Diary Entry 3/8/1967," March 8, 1967, Box 2(1), Willis M. Hawkins Papers.; Willis Hawkins, "Diary Entry 3/29/1967," March 29, 1967, Box 2(1), Willis M. Hawkins Papers.; Willis Hawkins, "DAG C-5 Modification Committee Charter," 1967, Box 67(8), Willis M. Hawkins Papers.

order was reduced from 172 to 81. The news for Lockheed was even worse because it had entered into three other TPP contracts which were costing the company hundreds of millions more. In 1971, Lockheed agree to take a charge for $484 million and to perform free maintenance for years in order to settle with the government and stay out of bankruptcy.[209]

TPP was the result of the progressively more adversarial relationship between industry and government, but both Roy Anderson and Al Cleveland both remembered TPP being the root cause of animosity. "You can just lay it all at this idea of Total Package Procurement," said Anderson, "because that kind of contract just causes adversarial positions to be taken and it's the worst kind of contract you can have on a new airplane design." Ostensibly, TPP failed primarily because it required contractors to accurately forecast both research outcomes, and unforeseen problem, which by definition are unforeseeable. In terms of the Triple-Helix, TPP suppressed innovation by building a knowledge management structure that concreted a one-way path from knowledge development to production. The financial pressures applied to industry required a complete stop to all extraneous research and sharing of potentially valuable data with other stakeholders. Perhaps most detrimental to a modern research effort, it placed Triple-Helix stakeholder in position of adversaries instead of advocates, or at worst, competitors. Unlike PERT which allowed knowledge spaces and innovation spaces to strengthen to compensate for strict time pressures impacting consensus spaces, TPP suppressed all three pillars of innovation networks to the point where innovation suffered at GELAC, and, with the notable exception of the Skunk Works, at Lockheed globally.[210]

---

209 Lockheed Corporation, "Historical Earnings and Human Resources," 3; Roy A. Anderson and Newcomen Society in North America., *A Look at Lockheed*, Newcomen Publication ; No. 1186 (New York: Newcomen Society in North America, 1983), 44–45.
210 Anderson and Newcomen Society in North America., *A Look at Lockheed*, 44; F. A. Cleveland, Interview with F. A. Cleveland, 32; Perreault, Interview with Roy Anderson, 11–12.

Conclusion

The tension between funding source and scientific researcher has been with us since the 19th century with the advent of the Linear Model. Initially the tension was simply that investors in research expect to monetize, or benefit somehow, from the results. While academia had traditionally claimed the realm of pure science, the advent of huge scientific endeavors after WWII made the stereotypical model of a solo academic researcher on minimal funding seeking out the secrets of nature untenable in the world of nuclear weapons, jet aircraft, and spaceflight. By the 1950's the U.S. government poured money into industrial scientific research at the state-of-the-art level as a matter of national security but without strong fiscal controls.

Early aerospace companies like Lockheed recognized the core need for innovation both for continued funding, and to contribute to the nation's defense. With the insight of gifted leaders like Willis Hawkins, Gene Root, and Kelly Johnson, Lockheed recognized that innovation could not be mandated at the individual level. Instead, environments needed to be created where innovation was regular, normal, and encouraged. These environments needed to establish robust communication systems between stakeholders, promote research without regard to development, and ensure resources to allow scientists freedom of research choices. While there was not general nomenclature for these needs at that time, the Triple-Helix Model of modern science calls these characteristics the knowledge space, the innovation space, and the consensus space respectively.

Unfortunately, policymakers and the system of military procurement in the 1950's and 1960's was based on the Linear Model and the government expected basic research to be accomplished mostly by academia, timely innovation in the development phase in industry, and product outcomes for all the research it funded. Science simply did not work this way and it the character of the tension changed between funding source (the government), and researchers (industry). Industry insisted that internal research and development was essential for innovation, but over the years, the government saw this as an excuse to exploit the system for higher corporate profit. An additional issue was that these issues were occurring at the height of the cold war, and government funding of private research smacked of socialism. As these tension broke down trust, both industry and government were positioned as adversaries in a battle for resources, instead of advocates for innovation and science.

Ultimately the advent of Total Package Procurement applied strictly scheduled innovation, limited financial resources, strangled communication, and excessive bureaucracy to the scientific endeavor and severely damaged the innovation networks that Lockheed had developed. By tracking the pathway of Willis Hawkins' design of the LMSC under a relatively generous IR&D model to the slow disintegration of innovation we can see that from the perspective of an innovation network, LMSC was conducting recognizably modern science in the mid-1950's.

The government also tried to tighten control over communication of scientific outcomes in the name of national security. To be fair, secrecy became a norm across all Triple-Helix stakeholders in the 1950's and 1960's. How then, if communication systems (knowledge spaces) were essential in innovation networks, did scientists circulate their output? This is the question addressed in the next chapter.

Chapter 5.

Output Circulation

This chapter examines output circulation and reciprocity in the early aerospace industry in the 1950's and 1960's. It shows several different forms of non-linear output circulation at varying scales and among the actors in the Triple-Helix. The chapter also differentiates this characteristic from the Mertonian norm of communism in that output circulation persists in spite of a culture of secrecy within modern science.

The term 'output' refers here to newly developed scientific knowledge and the term 'circulation' refers to the reciprocal transfer of that knowledge between actors. Communication methods in modern science are direct correspondence between individuals, journal articles, monographs, and colloquia. These systems offer a framework for knowledge spaces and an innovation networks.[211] These traditional conduits of scientific knowledge are still dominant for output circulation because a vestigial enlightenment-based cultural norm promoting freedom of scientific information still exists in academia. This is also described by Robert K. Merton as 'communism,' which he described as the idea that scientists believe that they should have access to, and ownership of, all scientific knowledge. However, this cultural expectation of freely sharing scientific information between stakeholders, or 'reciprocity,' causes dissonance within contemporary science because it is in direct conflict with the relatively modern norm of secrecy.[212]

Patents, non-disclosure agreements, espionage, McCarthyism, paranoia, and the publish-or-perish culture all acted as obstacles to the traditional norm of free circulation of scientific knowledge. So

---

[211] See Chapter 4.
[212] Merton, "Science and Technology in a Democratic Order."

too did the national economic system of capitalism which expressly encouraged both the private ownership of assets, and the exploitation of those assets for profit. In the world of modern science, knowledge carried immediate financial value only if it remained secret. In the case of the early aerospace industry, the close relationship between aerospace and the national security apparatus made secrecy an obligation even after the research was complete, so covert conduits of information developed.[213]

These new systems of information circulation were generally interpersonal and often stretched the boundaries of discretion. They occurred company to company, through the contract bidding process, via visiting academic scientists, through military projects officers, through formal project reporting channels on an intra-company basis, and particularly through industry associations and advisory boards.

Secrecy as a Barrier to Circulation

The development of secrecy as a norm in modern science stems from the development of large-scale science during and immediately after WWII. Vannevar Bush's Office of Scientific Research and Development (OSRD) had been the first effective government coordinating agency for scientific research during the war. In 1945's *Science – The Endless Frontier,* Bush laid out a blueprint for a peacetime national system of scientific endeavor involving academia, government, and industry. His chief opponent was New Deal Senator Harley Kilgore, who preferred a politically managed system instead of Bush's proposed technocracy. The negotiations lasted from 1945-1950. The National Science Foundation (NSF) was then established almost exactly as originally proposed by Bush, offering only oblique governmental

---

[213] Etzkowitz, *The Triple Helix : University-Industry-Government Innovation in Action*, 23; Henry Etzkowitz and Chunyan Zhou, "The Entrepreneurial University in Various Triple Helix Models" (Triple Helix Conference, National University of Singapore: The Triple Helix Association, 2007), 26.

participation, administration by scientists, and funding control for a national system of basic research to the exclusion of social sciences and applied science. As an incentive to ensure the cooperation of industry, which had been vital to Bush's wartime work with the OSRD, Bush insisted that companies working on federally-funded projects could keep the rights to patents established during research and development. In a field like early aerospace where profit was capped at 7%, the opportunity to develop patentable technology was a key incentive to the creation of the Triple-Helix.[214]

While Bush had been occupied with the development of the NSF, an equally important movement had been taking place within the military. General Leslie Groves, who Bush had appointed to manage the Manhattan Project in 1942, did not want to give up military control of either nuclear technology or nuclear weapons. In 1946 Harry S. Truman established the Atomic Energy Commission (AEC) to give civilian oversight to all atomic-related weaponry and research. Groves fought this decision with all the influence he could muster. By January 1947, the transfer had taken place as Truman had intended but at the start February, Groves had the control of the nuclear stockpile moved again to the newly created Armed Forces Special Weapons Project, which was, not coicindentally, headed by Groves. The control of nuclear research remained with the AEC, but in the two years since the end of the war, Groves had directed all branches of the military to invest in their own R&D systems. Groves was obsessed with security and kept the extent to which the military had established its own science programs quiet. By 1948, the military accounted for 62% of all research and development expenditures,

---

[214] Bush, *Science, the Endless Frontier: A Report to the President on a Program for Postwar Scientific Research*; Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945: A Political Interpretation of Science--The Endless Frontier"; Hawkins, "W. M. Hawkins to L. E. Root - Technical Aspects of Missile System Management Leading to Low Potential Profit.," 1957.

and controlled 60% of all grants to research universities. In 1950, the Navy alone had funded research in over 200 universities with a budget in excess of two million dollars.[215]

Groves' dedication to secrecy manifested in the industrial workplace through systems of security clearances. It was enforced through oversight, both formal and informal, by the military's procurement staff. Civilian clearances were not usually all encompassing, but were for specific topics. The military subjected themselves to a similar set of rules, but were not consistent in the application of these rules. Ben Rich felt much of this stemmed from competition between the services. He remembered an experience with Air Force Major General Robert Bond who was visiting the Skunk Works. He was suspicious that the Skunk Works was prioritizing a Navy project over an Air Force project (which it was). Rich informed the General that he had no need to know about other top-secret projects and if there was further discussion, both of them could go to jail. Even so, Bond saw a secure door and banged on it until it opened to reveal several "startled" Navy officers. Realizing he had erred, both he and Rich had to fill out inadvertent disclosure paperwork and formally apologize to the Navy who Rich remembered "were outraged…An Air Force general seeing their secret project was as bad as giving a blueprint to the Russians."[216]

This secrecy was a problem for complex projects when components were being developed from many sources but the purpose of those components could not be shared. Even so, the military gave conflicting messages about the importance of secrecy. It was far from a binary system where divulging

[215] Bruce L. R. Smith, *American Science Policy since World War II*, First Edition edition (Washington, DC: Brookings Institution Press, 1990), 52–58; Michael J. Hogan, *A Cross of Iron: Harry S. Truman and the Origins of the National Security State, 1945–1954* (Cambridge: Cambridge University Press, 1998), 234–64.
[216] Rich and Janos, *Skunk Works*, 37.

secrets was treated as a criminal act. Milton Wilson, a chemist at Aerojet, remembered an episode where he was asked to informally brief a group of officers, and one stood up and said:

> "You know, you are giving classified information out. How do you know I'm a real colonel? How do you know I have any security clearance whatsoever? You are in real trouble." And then they smiled about it and said, "Well, this is a test case. But from now on, please be careful. I could have walked in from the street and been anybody, a Russian spy." So the next time I got called in, Bill Zisch[217] introduced me to these guys, and I said, "I'm sorry, gentlemen, but as you know, there was an embarrassment where someone didn't check to find out who you guys truly are and whether you have any clearance." And so they laugh and laugh. Bill says, "I can guarantee, I will vouch for every one of these guys. Go ahead and talk." But that's what security was all about. You wanted to be darn sure you followed the rules of the game.[218]

The complexity of bureaucracy-based security was frustrating to industry for reasons beyond the inconsistency of application. The first was that it was immensely expensive to operate internally under the military's imposed systems. While obtaining security clearances for individuals for specific projects was straightforward, it was time consuming, and under the procurement systems the military used, there was little time to spare. Achieving blanket top-secret clearance was far harder and led to maddening situations. When building the SR-71, the entire project was top secret, but only five members of the Skunk Works had clearance enough to access entire project. The cockpit was marked "Top Secret" and when test flights occurred, one of these executives had to sign out the entire plane

---

[217] CEO of Aerojet from 1961-1966
[218] Oral history interview with Malcolm R. Currie., 47; Oral history interview with E. Milton Wilson, 28.

and would have been held personally responsible if it did not return with all its secrets intact. Another example is given by Milton Wilson who recalled that the extent of security sometime ran to the ridiculous. He recalled that once at Aerojet there was immense security around Secret Fuel Number 1 (SF-1). Wilson had worked on formulations of kerosene for rocket fuel but all of these had been only "confidential."[219] For SF-1, Wilson had to "have a special safe for my one and only secret fuel." The fuel, it turned out, was hydrogen, the most common element in the universe. [220]

"Technology Transfer"

In a capitalist economic system, assets have owners, and the government claimed ownership of the research they funded. Sometimes this placed the military in an adversarial position to the government's civilian leadership. Despite the military's insistence on secrecy, the civilian government expected the aerospace industry to share technology and scientific knowledge[221]. In 1958, the National Aeronautics and Space Act required NASA to provide "the widest practicable and appropriate dissemination of information concerning its activities and the results thereof." Additionally, NASA was tasked with shrinking the time gap between government funded research, and the development of publicly accessible technology. By 1965, under the pressures of secrecy and industrial competition, the timeframes between governmentally funded science, and public dissemination had lengthened so NASA commissioned the Denver Research Institute at the University of Denver to study the mechanisms of

---

[219] The progression of clearance levels was restricted, confidential, secret, top secret, then specialty clearances above that.

[220] Oral history interview with E. Milton Wilson, 26–27; Rich and Janos, *Skunk Works*, 32; Oral history interview with Malcolm R. Currie, 47.

[221] These are different. Why technology is a part of knowledge transfer, it is a technical term referring to completed stages of development. That being said, technology was not necessarily a completed physical product. It could be a physical, or intellectual tool.

dissemination. The report defined the term "technology transfer" to mean the effective communication of "technical information, including scientific knowledge" for use in application.[222]

The report was based on a series of surveys sent to professionals in defense manufacturing industries. John S. Gilmore, senior research economist at the Denver Research Institute headed the group. He chose to focus on 62 firms in four industries – electric batteries, printing and reproduction, industrial controls, and medical electronics. The surveys were designed to assess which official and unofficial channels of dissemination were used at the individual and departmental levels. The most common technology transfer channels that Gilmore identified were split into two categories: intra-organizational transfer, and inter-organizational transfer. For external channels, professional and trade journals were the most relied-upon channel of information. Interpersonal relationships followed closely behind, and these included interactions with vendor personnel, customer personnel, university consultants, and conferences and symposia. For internal communication within an organization, by far the most important channel Gilmore found was verbal communication with other employees, vendor personnel, customer personnel, and peers in other companies.[223]

The results were disheartening to NASA. Despite budgeting almost $5 million per year to a formal dissemination program, official government funded channels of communication were the least-relied upon. The study concluded that "most individuals felt it too difficult to retrieve relevant material

---

[222] John S. Gilmore, "The Channels of Technology Acquisition in Commercial Firms and the NASA Program" (National Aeronautics and Space Administration, 1967), 7.
Pub. L. 85-568, July 29, 1958, 72 Stat. 426
[223] Gilmore, 22–50; Daniel S. Greenberg, "Civilian Technology: NASA Study Finds Little 'Spin-Off,'" *Science* 157, no. 3792 (1967): 1016–18.

from the mass of government publications and indicated that they expected to learn of important government-developed technologies through trade and professional channels."[224]

Senator Jennings Randolph, who was the chairman of the Senate Subcommittee on Science and Technology, saw flaws in the simplistic transfer and use model Gilmore had employed, but still agreed with the net outcome. He wrote that "public funds support two-thirds of all scientific and engineering effort in the United States, and in the past decade the federal investment has totaled $100 billion. The government has a responsibility to get full benefit from the resulting technology." Randolph arranged hearings on the pace of technology transfer and invited Lockheed board chairman Courtlandt Gross to testify. Gross in turn asked Willis Hawkins to weigh in on Randolph's concerns and help prepare Gross' testimony.[225]

Instead of answering each question specifically, Hawkins wrote to Lockheed General Counsel Rodgers Donaldson suggesting several overarching themes that Gross should emphasize. Hawkins' tone was defensive. This was not a surprise since this moment was also at the peak of Lockheed's tension with Total Package Procurement and the adversarial nature of military contracting in the mid-1960's.[226]

Initially, Hawkins wanted it to be clear that the contracting process was object-oriented and directed. This required a larger investment than undirected research and often Lockheed would invest its own money into project-based IR&D. Nevertheless, all the research from Lockheed was then submitted to the contracting agency for dissemination, so Lockheed had no role in the technology transfer process. This was an important point to Hawkins because he wanted to avoid any implied

---

[224] Gilmore, "The Channels of Technology Acquisition in Commercial Firms and the NASA Program," 26.
[225] Jennings Randolph, "'Spin-Offs' of Federal Research," *Science* 158, no. 3800 (October 27, 1967): 438; Willis Hawkins, "W. M. Hawkins to Rogers Donaldson," December 13, 1966, Box 33(1), Willis M. Hawkins Papers.
[226] See Chapter 4.

criticism of Lockheed for sitting on technology when, in fact, most of the science they developed was circulated to the military immediately.

Additionally, Hawkins wanted Gross to be clear that with a massive conglomerate like Lockheed, different divisions were not given advantage over competitors. Hawkins was again concerned that the questioning would imply that Lockheed would share information internally before releasing it to the contracting agency. Hawkins emphasized the contrary, the "various division have to go through normal channels of transfer to obtain he technology resident in another division."

On the topic of patented and proprietary knowledge, Hawkins was conflicted. He did not have a good argument for why government funding should result in a patent for Lockheed's benefit. Instead he suggested that Gross focus on the cost savings and speed advantage available to the patenting organization.

Finally, Hawkins let his grievances show about both government dissemination, and about security procedures in general. Hawkins complained that the large amount of new science combined with the vast bureaucracy of the military made the "quick collation and distribution of the information very nearly impossible." Similarly, his suggestion for the best single improvement the government and industry could make together was to create specialized clearances that allowed researchers access to information from a specific field of study, instead of a specific project. This would allow researchers across industry to work on problems and communicate openly across unnecessary security barriers.[227]

Hawkins' argument was that government policy was to blame for slow dissemination, not industry. Industry (or at least Lockheed) was doing what they could, but the breakdown in the

[227] Hawkins, "W. M. Hawkins to Rogers Donaldson," December 13, 1966.

dissemination of science was all at the doorstep of the military. In practice, this was not quite the case. Lockheed's output circulation, even within the company, was often stunted, incomplete, and obscure. The experience of the Georgia Lockheed Aircraft Company (GELAC) receiving a cure notice for the C-5A weight issues was a case in point. The Design Advisory Group (DAG) committee, which had been formed by the Air Force to help Lockheed, made several suggestions for weight reduction which the California Lockheed Aircraft Company (CALAC) had already been systems testing for another project. Unfortunately, this information was unknown to GELAC and they spent several months replicating CALAC's work. Hawkins proposed a solution in the form of a "linker-type organization," or an intermediary which could legally circulate outcomes. This would track IR&D throughout the company and, within the rules of secrecy, proactively inform divisions of new knowledge developed in other divisions. His plan was to lower the barriers to output circulation by pushing the secrecy rules to their limits.[228]

Industry Associations and Advisory Groups

Associations and advisory boards are a normal part of the contemporary academic and industrial scientific establishment. They offer an organizational overlay to traditional conduits of scientific communication such as journals, conferences, and colloquia. They assume authority by the assent of their memberships, and they also manage standards of both operation and of peer review, thus acting in a governance role. Finally, these associations create spaces and opportunities for members to communicate the results of research and to create feedback loops with peers. These interpersonal opportunities are a core characteristic in the modern circulation of scientific outcomes.

---

[228] F. A. Cleveland, Interview with F. A. Cleveland, 32; Willis Hawkins, "W. M. Hawkins to R. Thoren & L. Frisbee," September 18, 1967, Box 33(3), Willis M. Hawkins Papers.

The associations had the same functions as they do in contemporary science. Associations offered four roles which promoted output circulation: by encouraging personal relationships and communications between members, by representing an industry sector to the government, by acting in a governance and oversight role, and by offering an official setting for the careful sharing of information.

The importance of associations was well understood within the early aerospace industry. In 1957, Gene Root who was then the General Manager of the Lockheed Missile and Space Company (LMSC) resigned from the APA due to other duties. Instead of leaving the position open, he proposed that his post on the steering committee be assumed by Willis Hawkins (at the time LMSC's Assistant General Manager). William Littlewood, a longtime engineer for Douglas and the APA president at the time, denied the request since he was looking for someone with more experience. Littlewood was looking for membership of a higher position to ensure high-quality communication between members of similar rank. [229]

Even though interpersonal communication was a strong impulse, association members still understood that there were limits to how free their communication could be. Milton Wilson remembered that many of his contemporaries belonged to the American Chemical Society and the Society of Mechanical Engineering during the growth of Aerospace in Southern California. The members would "not violate security, but ended up exchanging ideas and feeding off each other. Once aerospace got started here, it was perpetuated due to the fact that we all knew each other and could easily call up a rival company and say, "Gee, we're about to work on this. Do you know anything about it?"" Al Cleveland, who ultimately ascended to the presidency of the American Institute of Aeronautics and Astronautics (AIAA) agreed with Wilson. He said that active membership gave "access and knowledge of

[229] L. E. Root, "L. E. Root to William Littlewood," December 24, 1957, Box 47(8), Willis M. Hawkins Papers.

people in other companies in similar kinds of work…It's just a very healthy transfer mechanism." He continued to say that the memories of taking on leadership in the AIAA "was a great experience. I got to know an awful lot of people, who it turns out to be were very useful to know, in the sense of having people which you can ask questions of, that you can talk to about all kinds of aspects of engineering."[230]

Groups like the AIAA also offered another platform for the purposes of communication: the convention. While conventions encouraged personal communication at the micro-scale, they also offered opportunities for speakers to communicate obliquely about otherwise sensitive issues. In 1957, when the LMSC opened in Sunnyvale, more than 200 members of the Institute of Aeronautical Science, made up of members from government, industry, and academia, travelled to Sunnyvale to tour the new R&D facility. While there, Ronald Smelt gave a lecture on Lockheed's work with hypersonic missiles, and Willis Hawkins detailed the X-7 and X-17 missile programs (with a color film of rocket firings!). When Hawkins spoke at the American Rocket Society Forum in 1958, and then the Aerospace Club of Washington a decade later, he was inundated with personal requests for copies of his speeches, and for further details. Some of these he answered with elaboration, but for others he chose to be more discreet and supplied only the speech. Similar large-scale visits occurred to Hughes Aircraft in Tucson, the GE Microwave Lab in Palo Alto, and Boeing Headquarters in Long Beach.[231]

Industry associations also acted as liaisons between industry sectors and the government. They represented industry in policy development, and represented the government in oversight of industry

---

[230] Oral history interview with E. Milton Wilson, 29; F. A. Cleveland, Interview with F. A. Cleveland, 46.
[231] Willis R. Hawkins, "Impact of Space Flight on Industry" (Speech, American Rocket Society Forum, Fall 1958); Willis R. Hawkins, "New Convenience and Comfort from the Subsonic Giants" (Speech, Aerospace Club of Washington, July 30, 1968); Robert M. Crane, "IAS Sections - San Francisco," *Aeronautical Engineering Review*, Institute of the Aeronautical Sciences, 16, no. 12 (December 28, 1957): 76; Willis Hawkins, "W. M. Hawkins to M. Worst, Visit to Hughs - Tucson," May 17, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to G. L. Haller, Manager, Laboratories Dept., General Electric.," May 10, 1955, Box 30(9), Willis M. Hawkins Papers.

standards and norms. The Aircraft Industries Association (AIA) represented all the major aerospace companies and were able to link key members in issues such as the overcrowding of missile test facilities in Southern California. The AIA had formed a Guided Missile Committee as a space for structured interactions, and Willis Hawkins was able to present the issue directly to John Crowley, the Director of the Office of Guided Missiles for the OASD. The probability that a testing backlog would lead to a significant delay of Polaris led to the establishment of the Santa Cruz System Test Facility.[232]

Hawkins, who was Assistant Secretary of the Army in the early 1960's was comfortable both recommending policy to industry associations, and directly to the government. In 1958 Hawkins wrote to Guyford Stever who was President of Carnegie-Mellon University and Chair of the Aeronautics and Space Engineering Board. This board was part of the National Academy of Sciences, and while not technically a government agency, was an influential public policy advisor. Hawkins suggested that the board support the creation of a Civil Aviation Agency within the federal government. Hawkins also supported converting NACA to NASA[233] and to give NASA direct procurement authority to "develop and maintain the peaceful exploration of space." In that same letter, Hawkins exemplified the ambivalence of industry toward secrecy by promoting circulation of basic research and lamenting how hard it was to protect Lockheed's unfinished work from prying eyes. He wrote that there was no mechanism yet developed "whereby industry utilizes research facilities or services without so disclosing their concepts and ideas that they become public property before the concept has been fully developed." In 1967 the government and industry were grappling with changes to procurement policy. In order to represent the interests of both Lockheed and the greater aerospace industry, Hawkins wrote to industry associations

---

[232] Willis Hawkins, "W. M. Hawkins to S. W. Burress, Discussion with John Crowley," May 7, 1957, Box 30(11), Willis M. Hawkins Papers.

[233] National Advisory Committee for Aeronautics (NACA) became the National Aeronautics and Space Administration (NASA) in 1958 with very significantly expanded authority.

with suggestions on what policy positions the associations should take. He wrote to Lloyd Kuhn, president of the AIAA, that the AIAA should recommend to the U.S. Senate that it fund basic research into aerophysics, weather, and material sciences. Hawkins also suggested that Kuhn remind the Senate that member companies were not charities and needed a fair profit to remain in the business. [234]

Industry associations also took a role in measuring and managing the industry response to common public and government concerns. For example, the noise created by early subsonic jet engines used in commercial aviation was becoming progressively louder. Willis Hawkins told the California Chamber of Commerce as early as 1952 that noise pollution of jets was the second most pressing issue with commercial aviation (behind air-traffic control). Jet powered aircraft produced roughly twice the perceived noise of a prop-driven airplane. In 1963, M. J. Lighthill presented the Wright Brother Lecture to the Institute of Aerospace Sciences on jet noise. He was extremely critical of the slow approach taken by industry to solving this problem. His perspective was adopted by the AIAA which, in conjunction with NACA, developed an advisory board and consistently pressured industry to lower volumes and ensure that noise pollution was a prioritized design consideration. [235]

Another example of an industry association working on behalf of the public was in 1973 when the International Air Transport Association expressed a formal concern directly to Lockheed that the glide-slope antenna on the L1011 passenger jet had an inappropriate geometry in relation to the landing gear. Willis Hawkins researched this matter extensively and wrote a very respectful letter back to the

---

[234] Willis Hawkins, “W. M. Hawkins to Lloyd Kuhn,” January 16, 1967, Box 33(2), Willis M. Hawkins Papers.; Willis Hawkins, “W. M. Hawkins to Guyford Stever,” July 26, 1967, Box 33(2), Willis M. Hawkins Papers.

[235] Willis R. Hawkins, “New Promise in Air Transport and New Problems” (Speech, California Chamber of Commerce, January 5, 1952); M. J. Lighthill, “Jet Noise,” *AIAA Journal* 1, no. 7 (July 1, 1963): 1507–17; Beranek Bolt and Newman, inc. and Port of New York Authority., *Studies of Noise Characteristics of the Boeing 707-120 Jet Airliner and of Large Conventional Propeller-Driven Airliners, Prepared for the Port of New York Authority.*, 1 v. (various pagings) (Cambridge, 1958), 3-9-4–4.

IATA assuring them that all was well. A month later the IATA sent another letter expressing concern about the L1011 cockpit smoke removal system. This was in response to a crash in Orly, France. R.R. Shaw, the assistant director general of the IATA was asked by the IATA board to write to all manufacturers urging them to reevaluate their current procedures. In this case Hawkins sent a complex and extensive description of the entire testing process.[236]

Another role held by the industry associations was to maintain cultural norms. One of these norms was to protect members from poaching from each other. In 1957, Hawkins was feverishly recruiting to staff the LMSC. LMSC had three huge projects (Polaris, Agena, and the X-17 supersonic drone), and was significantly understaffed. Between the end of 1956 and the beginning of 1958 LMSC went from 4798 employees to 8707 – a jump of 81%. Hawkins was under tremendous pressure to recruit, but understood that there were rules for this. In June of 1957, he began recruiting Boeing employee Benedict Cohn, but warned "If you do decide to continue with our discussions, we would appreciate your releasing us to talk with Boeing so that we don't get in trouble with the AIA."[237]

Associations used awards as behavioral rewards to define individual excellence within industry. These carried prestige for not only individuals, but companies. Lockheed encouraged nominations from within. Hawkins himself won many awards, medals, trophies, and honorary degrees throughout his career. Appointments to leadership and steering committees within associations were both rewards and ways to mix people from many stakeholders in the aerospace industry. As an example, the roster of the Defense Science Board held 28 total participants, with eleven from academia (including the National Academy of Science, Harvard, MIT, Cornell, Lehigh, the University of Illinois, UCLA, and Los Alamos), nine

[236] Willis Hawkins, "W. M. Hawkins to R. R. Shaw," January 16, 1973, Box 72(4), Willis M. Hawkins Papers.
[237] Willis Hawkins, "W. M. Hawkins to Benedict Cohn, Boeing," June 21, 1957, Box 30(9), Willis M. Hawkins Papers.

from industry and consulting firms (including Hercules Aircraft, IBM, General Motors, Texas Instruments, RAND Corp., and Hughes Aircraft), and the remainder from government agencies (including NASA, the Air Force, the Department of Defense, the National Bureau of Standards, and the National Academy of Sciences). With respected, influential, and highly regarded leaders in aerospace meeting regularly, the norms and values which would otherwise evolve independently, were combined, developed, and adopted.[238]

Despite the value these organizations offered to the aerospace industry, they were still funded by membership and when the associations lost sight of their mission, they were quickly corrected. In 1956, the AIA sent a letter to the Army criticizing its slow approach to technological development. Gene Root, the V.P. of LMSD, wrote to the AIA defending the Army who were in the midst of a move to modernize. He told the AIA that the Army was already sensitive and criticism would hurt the industry so they should keep a lower profile in the coming months. Hawkins too had no problem reminding the AIA when they stepped out of line. In 1965, he returned from his service with the Army and informed the AIA that he would be taking over on the Aerospace Advisory Council for Carl Haddon who had been promoted to group VP. Normally this appointment would be by invitation, but Hawkins made it clear that the AIA had no choice in the matter since he was now VP of Science and Engineering for their largest member. He was placed on the council immediately.[239]

The Role of Academia in Managed Output Circulation

---

[238] Willis Hawkins, "Memo to Chief Engineers," November 8, 1968, Box 33(3), Willis M. Hawkins Papers.; Engebretson and Wittenberg, "Biographical Note"; "Roster of the Defense Science Board" (Office of the Director of Defense Research and Engineering, March 1, 1968), Willis M. Hawkins Papers.

[239] L. E. Root, "L. E. Root to D. J. Haughton," January 2, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to C. R. Lowery, Aerospace Technical Council, AIA," August 5, 1966, Box 33(1), Willis M. Hawkins Papers.

The interaction between industry and academia was close in the early days of aerospace. Most of the scientists in industry had received an education in traditional universities, and this trend became more prevalent during the 1950's and 1960's. This meant that personal, micro scale networks between industry scientists and academic scientists were often well established prior to an individual scientist's recruitment to industry. These historical, personal networks allowed a great deal of knowledge diffusion, but there were other paths which also promoted output circulation including exchange of personnel where industry employees taught at local universities, and academic scientists would often consult in industry.

There were communication advantages to industry professionals teaching and working in academia as time allowed. When Willis Hawkins requested to endow a chair at Caltech in 1966 for $250,000, he reminded Executive VP Carl Kochian of Lockheed's partnership with Stanford in 1957, and the benefits of a close association. Hawkins wrote

> "Although we did not actually endow a chair at Stanford, we, in effect, did the same thing when LMSC helped Stanford bring Dr. Nick Hoff into the school. This turned out to be an extremely good procedure since it puts you in good rapport with a member of the faculty, it permits you to expand the school's talent, and if you do indeed endow a chair, the Lockheed name is repeated constantly so that the students hear of the company in the best possible context." [240]

[240] Hawkins, "W. M. Hawkins to A. C. Kotchian," July 12, 1966.

Additionally, the suggestion was for a chair in sound physics to combat the problem of aircraft sound which was getting much more serious in this time. The expectation was that Caltech would provide research data in return for the endowment.

Teaching on campus was also a way for individuals to strengthen, or reestablish their academic networks. Hawkins taught at U.C. Berkeley, and Hughes CEO Malcolm Currie taught electromagnetics at UCLA. This conduct was highly endorsed at Lockheed, and in 1955 Ernie Krause requested and received a list of hundreds of engineers and scientists who would be eligible to teach at UCLA and Caltech. It was also beneficial for universities to recruit leadership service from industry, and in 1966, Hawkins joined the Advisory Council for the Stanford School of Engineering. Certain academic luminaries seemed to work without a care for security; and got away with it. It was not uncommon for Caltech's Clark Millikan to invite industry people in to unabashedly discuss sensitive industry research. These invitations were usually honored. Lockheed's relationship with Millikan dated back to the early 1957 when Hawkins had personally invited Millikan to tour the Sunnyvale facility. Hawkins sweetened the invitation with a private flight from Burbank. By the end of the year, Millikan was an active consultant on Polaris and the fuel utilization on payload for the New Horizon Project (the Agena satellite).[241]

Academic scientists also took opportunities to work in industry. Early on, joint appointments were common. In 1950, Austrian astrophysicist Fritz Zwicky joined Aerojet to direct research while he was still a full professor at Caltech. Nicholas Hoff was given a joint appointment to Lockheed and

[241] Hawkins, "W. M. Hawkins to D. O. Horning," May 3, 1956; Oral history interview with E. Milton Wilson, 19; Hawkins, "W. M. Hawkins to E. H. Krause," April 13, 1955; Willis Hawkins, "W. M. Hawkins to J.E. Wallace Sterling," February 1967, Box 45(3), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. Edwards," April 25, 1956, Box 30(10), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Clark Millikan," July 22, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to H. J. Brown, Engineering Branch Weekly Report," January 24, 1957, Box 30(11), Willis M. Hawkins Papers.

Stanford in 1957. Sometimes it was regularized consultancies. Caltech's Richard Feynman gave monthly lectures at Hughes, and Bruce Sage, chair of the Chemistry Dept. at Caltech, would consult monthly at Aerojet. By 1955, the practice of hiring academics for summer consultancies was so common that standardized hiring forms had been developed at Lockheed for that singular purpose. [242]

Since the period of availability for academic consultants was usually short, these appointments were often synthetic in nature and resulted in reports of describing the current state of research. For instance, Dr. M. J. Thompson of the University of Texas was invited to spend the summer of 1955 determining

> …from existing theoretical, research and experimental programs the nature of information available in the field of hypersonic flow... This effort will result in a report containing a summary of the most usable theories and methods of calculation with accompanying references to assist practicing engineers in the solution of hypersonic problems. Also included will be a summary of significant gaps in the state of knowledge which require further theoretical and experimental programs.[243]

These networks gave ample opportunity for unofficial output circulation back and forth between Triple-Helix actors. We can see how this information flowed in Hawkins' description of the LaCrosse Missile program, and the breadth of its collaborations. He said "Another program for which we have high hopes and which is still under consideration is the proposal which we made jointly with the Federal

---

[242] Oral history interview with E. Milton Wilson, 2–3, 28; Oral history interview with Malcolm R. Currie., 21–22, 28; Willis Hawkins, "W. M. Hawkins to M. J. Thompson," February 24, 1955, Box 30(8), Willis M. Hawkins Papers.; Hawkins, "W. M. Hawkins to M. G. Kispert," March 15, 1955; Willis Hawkins, "W. M. Hawkins to H. J. Brown, Proposed Consulting Services of Prof. Paul E. Holden," June 25, 1957, Box 30(11), Willis M. Hawkins Papers.
[243] Willis Hawkins, "W. M. Hawkins to M. J. Thompson, Summer Consultant Task Statement," February 4, 1955, Box 30(9), Willis M. Hawkins Papers.

Telecommunications Lab through Army Ordnance for the LaCrosse Missile. This particular missile is a developed missile. It was designed and developed by Cornell Aeronautical Laboratories for Army Ordnance." In one description, Hawkins describes the intertwined collaboration of all three actors in the Triple-Helix. Similarly, the guidance systems for the Polaris missile were a collaboration between General Electric, and MIT.[244]

Intercompany Circulation

While individual researchers found opportunities to socialize and share research notes in the university and industry association settings, more formal interactions between aerospace companies took place where knowledge was circulated. These took the form of sanctioned interactions (which sometimes became indiscreet), and semi-sanctioned interactions where high-level executives took it upon themselves to share information. These semi-sanctioned interactions occupied a gray area where the sharing of information was possibly inappropriate for reasons of either competition or national security.

Despite frequent collaborations, the competitive current between companies was always just below the surface. When executives and scientists from competitors did have discussions, there was a threshold beyond which sharing was expressly avoided. However, this threshold was nebulous and shifting. For example, in 1967, Lockheed employee John H. Sides[245] met with Charles Perrine, Executive VP of General Dynamics in Pomona. They were old friends. When their conversation turned to

[244] Hawkins, "Address to the Lockheed Board of Directors"; Willis Hawkins, "Address to the Lockheed Board of Directors, LMSD Overview."

[245] John H. Sides had retired from the U.S. Navy in 1963 as a Rear Admiral. He had been immediately hired by Lockheed as their primary liaison to the Navy. This was a common approach for Lockheed, and in 1967, they had 3 Lieutenant Generals and Rear Admiral Sides on the payroll.

shipbuilding technologies, Sides cut off the discussion before it reached an improper level "inasmuch as we would probably be competitors in the program." However, Perrine reassured Sides that this was not the case and gave several reasons why General Dynamics would not compete in the building of the ship. Much of this information was very sensitive including budgets, directions, and Perrine promoted a partnership in the project between General Dynamics, Lockheed, and Raytheon (who were already partnered with Lockheed). In this particular case the threshold Sides was careful not to cross was based in programmatic competition (ship building) not overall company competition. Sides immediately reported the information which indicates he was concerned about the appropriateness of it.[246]

Another example of modest pushback on sensitive information came in the same year when Dr. Eugene Fubini, Group Vice-President of Research for IBM, requested of Hawkins detailed information on folding stowed rotor aircraft projects. Hawkins had worked with Fubini while both were assistant secretaries of defense a few years before. Hawkins response was extensive and filled with detailed and sensitive information, including a description of Hawkins efforts to avoid a conflict of interest between his duty to Lockheed and his duty to the Army. Hawkins reminded Fubini of this in a postscript, writing "We consider the attached information on the CRA and CARA Proprietary and ask you to hold it this way. Obviously, my editorial comments on the Army's progress are "top sacred." Ever since Lockheed entered the AAFSS competition I have had to stay out of the fray."[247]

A better-defined conduit for knowledge circulation was sharing data from defunct or dying programs. This was a normal process and was often to the advantage of Lockheed. In 1954, Kelly Johnson at Lockheed's Skunk Works had been in contact with Aerojet and discovered that they had

---

[246] Hawkins, "Memo to Bill Wilson"; J. H. Sides, "J.H. Sides to W.M. Hawkins," June 29, 1967, Box 47(2), Willis M. Hawkins Papers.
[247] Willis Hawkins, "W. M. Hawkins to E. G. Fubini," July 12, 1967, Box 33(3), Willis M. Hawkins Papers.

ceased development on the Aerowolf air to air missile. The Navy had moved to Raytheon's AIM-9 Sidewinders. Since the Sidewinder was too large to fit to the Lockheed F-104 fighter, but the Aerowolf did fit, Johnson suggested that Lockheed Missile and Space Company take over the research. This was enthusiastically greeted at Aerojet and the entire project was transferred to Lockheed and they ultimately produced the missile.[248]

Lockheed also circulated its own research externally. In 1958, Lockheed cancelled the CL-400 Suntan project[249]. This was a liquid hydrogen powered reconnaissance aircraft capable Mach 2.5 and had been in development at the Skunk Works for four years. Ultimately liquid hydrogen proved impractical for reconnaissance planes, but was ideal for rocket boosters, so Kelly Johnson sent years of research over to Convair who used it in developing the Centaur rocket boosters. [250]

Ultimately those boosters went into competition with Martin's Titan rockets for the Air Force's satellite launch system contract. Lockheed had developed its own system for launching the 117L Agena Satellite Reconnaissance System but it was not a finalist for this contract. Even so, it was obvious to Hawkins that the data Lockheed had developed on the 117L should be shared. He wrote:

> It seems obvious to me that the Air Force will have to decide between the Atlas and Titan in the not-too-distant future and that the temptation will exist to give the 118L, or at least a major responsibility in it, to the losing team. It therefore seems

---

[248] Kelly Johnson, "C. L. Johnson to H. Hibbard," April 26, 1954, Box 30(8), Willis M. Hawkins Papers.
[249] See Chapter 6.
[250] Rich, "Speech to Rensselaer Polytechnic Institute"; Rich, "Speech to Encino Men's Club"; Johnson, "C. L. Johnson to H. Hibbard," April 26, 1954.

appropriate that we should make what studies we have, with respect to the 117L on the Titan, available to the Air Force.[251]

Downturns in contract opportunities also allowed a freer circulation of data. When LMSD moved to Sunnyvale, it left the Van Nuys plant largely empty. Through the AIA Guided Missile committee, Hawkins knew that Boeing's Bomarc missile project was so large that it would need a second manufacturing site. He asked W. D. Orr and Clare Harris to approach Boeing and sell them on the opportunity to share data and co-produce the Bomarc. In a similar way, Fairchild Aircraft approached Lockheed in 1957 with an "opportunity." Initially Hawkins assumed that it was to assist in working on the Goose air-launched decoy, but when Fairchild's Director of Engineering, Grayson Merrill, arrived he refused to discuss Goose. Instead he had developed a detailed plan in which Fairchild could become a subcontractor to Lockheed in the Polaris project. Merrill was on the Navy Research Advisory Board with Hawkins and entirely privy to the details of the Polaris project. His presentation was based around this knowledge.

Fairchild was protective of their Goose project in this interaction because they feared Lockheed would move into competition with them. Lockheed seemed to have the resources to compete in any field, however, Fairchild need not have worried. Specialization had protected their Goose. The decoy had a plastic airframe. This was a specialty of Fairchild and Lockheed did not have the technology, facility, or personnel to compete. Specialization was often a protection from both espionage and competition. E. Milton Wilson of Aerojet remember a moment when Aerojet was

[251] Willis Hawkins, "W. M. Hawkins to J. H. Carter," August 19, 1957, Box 30(11), Willis M. Hawkins Papers.

"about to handle very high concentrations of hydrogen peroxide, and I knew Rocketdyne was great at it. So they said, not only is it good, come on over here and spend the day with us and we'll teach you how to handle it. We'll give you some samples you can take home with you. And you're right, we are the experts at it, and we don't mind if other people know it." So we scratched each other's back all the time this way.[252]

These ostensibly pleasant collaborations could descend rapidly into acrimony. Aerojet was a specialist in propellants and Lockheed was an overall project manager. They were comfortable working together because they were generally protected from each other's competition by structural measures – specialization for Aerojet, financial resources for Lockheed. By the middle of 1957, the relationship between Lockheed and Aerojet was so close that when the Air Force cancelled the Auxiliary Power Unit contract from Aerojet, Aerojet offered to send the entire corps of researchers to Lockheed Missile and Space Co. to assist with Hawkins' human resources needs. However, later in 1957 Aerojet was alerted by a subcontractor that a technical drawing he had seen at the Aerojet headquarters had shown up at LMSC with an LMSC stamp on it. Aerojet was livid and accused Lockheed of theft. Ultimately the situation was defused by the direct intervention of Rear Admiral William Raborn who was in charge of the Polaris project for the Navy.[253]

---

[252] Oral history interview with E. Milton Wilson, 29; Willis Hawkins, "W. M. Hawkins to W. D. Orr, Sales Prospects for Van Nuys," January 29, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Discussion of Fairchild Programs with Ed Speakman," August 27, 1957, Box 30(11), Willis M. Hawkins Papers.; L. E. Root, "L. E. Root to D. J. Haugton, Fairchild Aircraft Corporation.," August 26, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Call to Grayson Merrill Regarding Meetings in Washington, January 7-9, 1957," December 31, 1956, Box 30(10), Willis M. Hawkins Papers.

[253] Willis Hawkins, "W. M. Hawkins to S. W. Burress, Relations with Aerojet," November 26, 1957, Box 31(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to S. W. Burress, More of APU Competition," May 21, 1957, Box 30(11), Willis M. Hawkins Papers.

As industry associations grew in number, so too did fora for output circulation. In 1967, Hawkins invited various industry executives to join him in the newly formed National Academy of Engineering Aeronautics and Space Engineering Board whose first point of responsibility (as agreed to by NASA) was to promote R&D on civil aeronautics. One of those who joined was Grant Hendrick, a vice president for Grumman Aircraft Engineering Corp. In 1968 Hendrick sent an internal Grumman report to Hawkins called "Effective Use of R&D Expenditures for Aircraft Structures Research." Grumman had determined that aircraft structural research had the greatest opportunity for IR&D approval at that time, but only about 3% of the 100 million dollars assigned to IR&D for NASA was being used for this area. Unfortunately, a report suggesting the government fund more R&D in the aerospace industry looked biased coming only from Grumman, so Hendrick was looking to gain support, and data, from other industry players. Hawkins was interested in helping and assigned Morris Steinberg, Lockheed's Director of Technology Applications, to determine if Grumman's outcomes matched Lockheed's. Hawkins wrote back an extensive letter detailing Lockheed's proprietary company information and suggested that as part of IR&D, companies may assign their scientists to NASA or other governmental research agencies, and then bill it to the government. He also suggested that he would support the expansion of structural research within NASA and the report should be sent to NASA's Technical Advisory Committee on Aircraft Structures for an in-depth review.[254]

The suggestion of sending industry scientists to work in government agencies was an output circulation strategy that Hawkins had outlined in 1958 in a speech to the American Rocket Society Forum. The speech, "The Impact of Space Flight on Industry," illuminated the enormous challenges of large-scale scientific endeavor, and focused on personnel and communication. As a solution to most

[254] Willis Hawkins, "W. M. Hawkins to Var. Industry Executives," November 6, 1967, Box 33(3), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to G. Hendrick," October 1968, Box 18(3), Willis M. Hawkins Papers.

challenges, Hawkins promoted communication through industry-association partnerships as a way of sharing pools of specialists for a single vision. These associations were, in Hawkins' view, the "linking organizations" he had envisioned when he was designing the LMSC.[255]

Output Circulation within Lockheed

By the end of 1968, Lockheed had more than 92,000 employees spread over 11 divisions. The government saw this as a weakness in national security. If intra-company data flow was unrestricted, it was not secure. The military's system of mitigating this risk was a stringent system of security clearances overseen by a project officer. However, the impulse to circulate outcomes was strong. Despite the barriers to communication, Lockheed used two methods to circulate scientific output – personnel transfer and project committees.

The transfer of personnel between research and engineering within LMSC was relatively easy since Willis Hawkins had been both a believer in similarity of roles in research and engineering, and an active facilitator in the movement of human resources. When designing the structure of the LMSC in 1954, Hawkins had suggested directly to Pete Quesada that scientists and engineers were interchangeable. He expounded upon this directly with Lockheed's board the next year saying:

> I think that this is an arrangement that works out very well for the Missile Systems Division, particularly in the Lockheed family as a whole, because by interchange of personnel in this fashion not only do the Research folks benefit by the more or less practical slant and the effects of the Engineering response to the problems, but also the

[255] Hawkins, "Impact of Space Flight on Industry."

> Engineering personnel are also affected by the longer-range thinking that takes place in the Research Branch.[256]

When Ernie Krause, an early manager in the LMSC's research division attempted to poach some of Hawkins' engineering team, Hawkins reluctantly acceded. Of one of the poached men, N. K. Marshall, Hawkins wrote "I certainly hope, however, that the salary level for this type of man is compatible with the salary levels in Engineering, since he will obviously be doing the same type of work."[257]

Hawkins responded the loss of Marshall with an no-holds-barred poaching campaign from other divisions within Lockheed. Hawkins was relentless. As soon as he heard that Carl Haddon at CALAC had lost the Connie Rotodome project[258] he called and asked for all the project engineers. Haddon declined and said that they might be available in six months. Hawkins then memorialized the conversation with a note to Haddon's boss. He then got Clare Harris to loan him five "A" engineers, which like everyone else, Harris was reluctant to do. Hawkins convinced him otherwise only through dogged persistence.[259]

When engineer Stan Burress was promoted to Weapons Systems Manager on the Polaris project, Hawkins was indefatigable in finding a replacement for him. He wanted Dick Pulver from GELAC and came up with the idea of just "borrowing" him for 30 days. Hawkins' summary of the grueling process to Gene Root showed his motivations were overt. He wrote:

---

[256] Hawkins, "W. M. Hawkins to E. R. Quesada - Comments on the Logical Division of Responsibilities in MSD Organization," May 13, 1954; Hawkins, "Address to the Lockheed Board of Directors," 17.
[257] Willis Hawkins, "W. M. Hawkins to E. H. Krause," March 23, 1955, Box 30(8), Willis M. Hawkins Papers.
[258] This was a C-121 Constellation aircraft with a large dorsal dome at the back. The dome contained a rotating radar system.
[259] Hawkins, "W. M. Hawkins to H. J. Brown, Engineering Branch Weekly Report," January 24, 1957; Willis Hawkins, "W. M. Hawkins to L. E. Root, Loop Closed - Project Meeting," April 2, 1957, Box 30(11), Willis M. Hawkins Papers.

I have been discouraged by Dan[260] regarding the prospects of getting Dick Pulver for the project which I mentioned in my last report, namely analysis of our experimental procedures. Haughton's suggestion was a combination of Dick Boehme from Kelly Johnson's Special Projects organization, along with two boys from Georgia - Louis Bauer, and Jack Lewis. Hall Hibbard[261] discouraged me a little on Boehme and suggested I talk to Jack Wassell, which I did, requesting Art Flock. This also fell on deaf ears. I now have another idea which I would like to pursue, namely, to get Dan Gribbon up here for the same purpose. I just suggested this to Haughton and did not get knocked down. With your permission I would like to pursue this further. He might even be better than Pulver.[262]

Hawkins got his man in Gribbon, and immediately tried to poach Tom Higgins of the CALAC Engineering Branch. This was instantly squashed by Hall Hibbard who by this time had caught on to Hawkins project of staffing LMSC by gutting other divisions. Hawkins suggested Root send a letter to Dan Haughton suggesting LMSC's urgent need. He wrote "Carter is preparing a letter for your signature to Haughton and Monesmith[263] requesting submittal of additional names. Harnois should be our next target."[264]

---

[260] Group VP Dan Haughton – Gene Root's supervisor.
[261] Corporate VP – Dan Haughton's supervisor.
[262] Willis Hawkins, "W. M. Hawkins to L. E. Root, Assignment of Dick Pulver," September 3, 1957, Box 31(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Reorganization of Engineering Test Responsibilities," January 18, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Operations Report, Sept. 5," September 5, 1957, Box 31(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Operations Weekly Summary Report - Sept. 12," September 5, 1957, Box 31(1), Willis M. Hawkins Papers.
[263] VP of Personnel
[264] L. E. Root, "L. E. Root to D. J. Haughton, Tom Higgins," September 13, 1957, Box 31(1), Willis M. Hawkins Papers.; Hawkins, "W. M. Hawkins to L. E. Root, Operations Weekly Summary Report - Sept. 12," September 5, 1957.

Hawkins poached without hesitation, but was reticent about sharing. Even so, he was politically savvy enough to recognize when he had no choice. When the Pied Piper project poached design engineer Bob Salter, Hawkins reluctantly agreed but wrote to Ernie Krause "Hawkins wants him back when the project is complete!" Hawkins, however, did not return "borrowed" employees in a particularly timely manner. Al Cleveland, then an engineer for GELAC came to Burbank to report on the 125A weapons system. Hawkins was very taken with him, and told the LMSC Director of Research Louis Ridenour to "find a place for him." He came to work for Hawkins on loan in 1957 but was not transferred back to GELAC until 1960 went the C-141 Starlifter project needed a Chief Engineer.[265]

Hawkins also was regularly on the lookout to facilitate the movement of personnel outside of Lockheed. In February of 1967, Donald Horning of the executive Office of Science and Technology (OST) requested a list of young professionals to assist as consultants with the mission of the OST. Hawkins beat the bushes and compiled a list of several candidates from different Lockheed divisions and forwarded their resumes to the OST.[266]

Project committees at Lockheed performed a similar function to external advisory boards. They were a structured opportunity to circulate information under the cloak of security. The X-7 Ramjet Re-entry Test Vehicle Project Committee was formed from several departments and division in 1955 to offer overall project oversight. A decade later, the practice was still in effect. In 1967, Lockheed Executive Vice President Carl Kotchian formed the Ad-hoc Hypersonics Committee to "to create a program "which would achieve and maintain hypersonic capability within Lockheed sufficient to remain

[265] Willis Hawkins, "W. M. Hawkins to E. H. Krause, Availability of Bob Salter for Pied Piper Program," March 24, 1955, Box 30(9), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Visit of Al Cleveland," May 1, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. N. Ridenour, Background on Al Cleveland," October 7, 1957, Box 30(11), Willis M. Hawkins Papers.
[266] Hawkins, "W. M. Hawkins to Donald Horning," February 1967.

competitive and win significant programs. Considerable discussion, much of it highly classified, indicated a road map leading to significant programs for Lockheed."[267]

Lockheed used two other structures for communication. The first of these was the semi-formal Management Club which split into Northern and Southern California Branches when LMSD moved to Sunnyvale. This was a social club-based forum used to discuss high-level project issues across departments, but without the security structure of a project committee. Similarly, the annual Chief Engineers Meeting was held at Rye Canyon Research Center and was comprised of chief engineers and scientists from all Lockheed divisions and projects. This was also a secure opportunity to share classified information and to gain feedback and perspectives.[268]

The exception to these output circulation mechanisms was provided by Kelly Johnson who had his own semi-autonomous division – the Skunk Works. He was innately secretive and operated with communication only to the highest levels of Lockheed leadership. Not only did Johnson refuse to share engineers with LMSC, he flatly refused to share data with anyone but LMSC Director of Research Louis Ridenour who possessed an impeccable reputation for discretion and scientific excellence. When Hawkins was with the LMSC and needed something from Johnson, he had to write to Ridenour to ask him to ask for it. This was not a personal dislike of Hawkins, but Johnson's highly developed sense of security closely managing output circulation.[269]

---

[267] Willis Hawkins, "W. M. Hawkins to H. W. Kohl, et. al., Combined X-7 Project Committees," June 21, 1957, Box 30(9), Willis M. Hawkins Papers.; M. C. Haddon, "M. C. Haddon to A. C. Kotchian, Hypersonics," June 16, 1967, Box 30(1), Willis M. Hawkins Papers 1920-2009.
[268] Willis Hawkins, "W. M. Hawkins to Lockheed Chief Engineers," November 1, 1966, Box 33(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Les Andrews, Reorganization of the Management Club," March 7, 1957, Box 30(11), Willis M. Hawkins Papers.
[269] Willis Hawkins, "W. M. Hawkins to L. N. Ridenour, Contacts with Kelly Johnson RE His Special Projects," April 4, 1957, Box 30(11), Willis M. Hawkins Papers.

Suppression of Output Circulation

The military was aware of tension between national security and the impulse of modern science to communicate new knowledge. In the early 1950's the system of clearances and secrecy were mostly left to industry to self-enforce. This changed over time and by the mid-1960's, the military had added some structural requirements to contracting which had the effect of suppressing communication. Two common systems were by obscuring important data in massive amounts of paperwork, and by simple destruction.

Kelly Johnson felt that his security protocols were already superior to military efforts and declined to suffer military programs. His Skunk Works held a special place in aerospace design. Founded in 1943 as Lockheed's Advanced Development Projects, Johnson developed a reputation rapid development, high quality results, and above all, secrecy. The unit produced P-38, the F-104, the SR-71, and the F-117 among many other aircraft. This gave Johnson leverage in how he managed secrecy behind the walls of the Skunk Works. In fact, the immense external security he established was a perimeter around the Skunk Works as a unit, but within the unit, information circulated much more freely. Where he saw military procedures working without impact, he tolerated them, but when they slowed down research, he cut corners or unilaterally modified protocols. In 1960, the Skunk Works began Project Oxcart which ultimately produced the SR-71 Blackbird. When discussing administrative expectations, Kelly wrote:

> A minimum number of reports were required and important work must be recorded thoroughly. I do not like the situation that existed in at the start of the Blackbird program, where the flight-control system reporting, for instance, on the Apollo, was 20,000 8 1/2x11 pages a month. On the Dinosaur, it was something like 6000 pages a month, and the same company was doing the same work and they started

to send me progress reports. The first one had 330 pages and in the middle of it I ran across Bernoulli's theorem, which I used to teach. Whereupon the monthly progress report was kept to a maximum of 30 pages over seven years. If you cannot explain what you have done in a month in 30 pages, you don't know how to write.[270]

The frustration with paperwork was also evident with Kelly's successor, Ben Rich who complained that "McDonnel-Douglas is forced to store 92,000 boxes of data for their F-16 fighter program alone. They pay rent on a 50,000 square foot warehouse, pay the salaries of employees to maintain, guard, and store these unread and useless boxes, and send the bill to the air force." The dilution of key information was not good for science and communication, but was very good for security.[271]

Perhaps the more direct approach was the military's efforts to destroy records of secret research, and the tooling necessary to build aircraft. In 1967, secretary of defense had ordered Lockheed to destroy the tooling for the SR-71 even though it was still on active duty. Later, Ben Rich discovered that the Air Force had "ordered all Lockheed's and Air Force data and records, classified and unclassified on the SR-71, be destroyed. The Air Force wanted to be sure we couldn't resurrect the program." This meant that no further spare parts would be available for the aircraft very early in its life-cycle. This order startled the Skunk Works leadership and Ben Rich remembered that they moved to protect the U-2 from the same fate writing, "we hid the 25,000 tools in five different places so that some idiot wouldn't order them destroyed." Despite this effort to destroy the SR-71 research, in 1975 the CIA sent Ben Rich the radar cross-section lab results and stealth data from the SR-71 to forward on

[270] Culick, *Guggenheim Aeronautical Laboratory At The California Institute Of Technology*, 60.
[271] Culick, 60; Rich and Janos, *Skunk Works*, 328.

to Dr. George Heilmeier the Director of the Defense Advanced Research Projects Agency (DARPA). This was a shock to Rich who had assumed the CIA would deny the SR-71 even existed. Moreover, it was even more shocking that the CIA had maintained all the data and research developed for the SR-71 without even the Skunk Works knowing about it. The data had only been destroyed outside the government's ownership control.[272]

Output Circulation and a Conflicted Military

The government's stringent requirements for secrecy were a frustration for members of the military as well. In order for innovation to occur, the military understood that systems of communication needed to exist in which stakeholders could share information. Too much freedom, and indiscretion could occur, but too much structure would stunt circulation. But this tension caused significant inconsistencies in the military's messaging about secure circulation. To bend and even circumvent its own rules, the military used three of the platforms already in use by industry and academia. These were scientific advisory committees, project DAG committees[273], and directed industry partnerships through project officers.

Scientific advisory panels were established in each military branch in the early 1950's and were ostensibly an opportunity to collect expert analyses on topics which each branch of the military found important. The rosters of these advisory committees were generally composed of representatives from all branches of the military, industry, and academia. The 1957 Naval Research Advisory Committee for

---

[272] Rich, "Speech to Rensselaer Polytechnic Institute," 3–4; Ben R Rich, "Lockheed Interdepartmental Memo," March 10, 1993, Box 1(4), Ben R. Rich Papers.; Rich and Janos, *Skunk Works*, 24.

[273] Initially this was a Design Advisory Committee but with the advent of TPP pricing, the acronym was changed to stand for Defense Acquisition Guidebook modification committee. Effectively these were the same thing: a non-negotiable group of aerospace experts gathered by the military to advise a manufacturer on a project.

instance, had 45 members including Willis Hawkins, Frederick Terman the Provost of Stanford, Captain Levering Smith the Polaris project officer, and Rear Admiral William Raborn the Naval Director of Special Projects. The content of discussion at these committees was top secret and required both a valid top-secret clearance and an affidavit attesting to loyalty, secrecy, and openness while in session. It allowed key triple-helix stakeholders clear insight into each other's sensitive research. Lockheed, for instance, had their LMSC Director of Research Louis Ridenour give a long and detailed lecture to the committee on the challenges of the Polaris missile atmospheric reentry.[274]

Membership to these committees was by invitation only and generally viewed as valuable. Membership also crossed over between military-branch committees. Willis Hawkins joined the Navy, Army, and NACA scientific advisory committees in 1957 alone. His acceptance letter to the Army indicated that Lockheed saw great value in this opportunity to network. He even wrote to decline compensation for his involvement, "It is my feeling that participation in these activities is of sufficient mutual value that I would prefer to donate my services. This has been discussed with the other members of Lockheed management and they agree."[275]

DAG committees were smaller and unlike the military-branch scientific advisory committees, they were focused on specific projects or systems. Like all advisory committees, membership often

---

[274] Willis Hawkins, "Naval Research Advisory Committee Roster," February 8, 1957, Box 30(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root & L. N. Ridenour, Presentation to Scientific Advisory Committee," March 18, 1957, Box 33(11), Willis M. Hawkins Papers.

[275] Willis Hawkins, "W. M. Hawkins to W. M. Bruckner, Acceptance of Appointment to Army Scientific Advisory Panel," May 6, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Maj. J. D. Neumann," May 6, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "Appointment Affidavit for National Advisry Committee for Aeronautics," December 30, 1957, Box 31(1), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to Gen. W. L. Weible," May 2, 1957, Box 30(11), Willis M. Hawkins Papers.; J. F. Victory, "J. F. Victory to W. M. Hawkins," December 17, 1957, Box 31(1), Willis M. Hawkins Papers.

crossed over within projects. In the case of Polaris, Lockheed and Aerojet had membership on no less than 17 of the total 22 advisory committees.[276]

The DAG committee convened to address Lockheed's C-5 weight and subsequent wing issues, shared highly sensitive data between contractors, the military, and academia. The C-5 DAG modification committee had members from MIT, General Dynamics, NASA, and LTV.[277] From a security perspective, the scope of the committee purview was unlimited so all the members were privy to the most Lockheed's most sensitive C-5 data. Far from being defensive, Hawkins welcomed the broad perspective the committee brought. He also saw their feedback as a welcome collaboration compared to the adversarial interactions Total Package Pricing had generated between Lockheed and the Air Force. He later wrote to Robert Loewy, a committee member from the University of Rochester, that "your handling of the Air Force DAG Group for the C-5 was a magnificent example of how groups of scientists should be used. All the folks at Georgia commend your objective attitude and the remarkable amount of information you handled in such a short time."[278]

A third military-based output circulation conduit was through project officers. These officers were often stationed at large contractors to represent the military's interests and to offer project oversight. The Air Force developed an extensive set of project offices in the mid-1950's followed by the Navy. In 1961, Robert McNamara directed that all branches of the military were to employ project offices for all major weapons systems. Generally, there were two for each contractor; one was assigned to manage a system, and the other had a general responsibility for oversight of the entire facility. These

---

[276] Hawkins, "W. M. Hawkins to S. W. Burress, Relations with Aerojet," November 26, 1957.
[277] Ling-Temco-Vought was a major aerospace conglomerate from 1961 to 2000.
[278] Hawkins, "DAG C-5 Modification Committee Charter"; Willis Hawkins, "W. M. Hawkins to R. G. Loewy," July 20, 1967, Box 33(3), Willis M. Hawkins Papers.; Hawkins, "Diary Entry 3/29/1967."

officers were usually among the very few with clearances for all phases of a given project. Often, they would have information from other project officers, or from other research sites. Milton Wilson remembered a time when a project officer acted as a go-between to link industry with military researchers. He said:

> When the Titan missile came along, which was ours, it started doing what we called chugging: the engine would go shoom, shoom, shoom, shoom, shoom and finally blow up. The Titan was an overfunded project, so they asked us whether we could get inside the engine while it was running and grab samples, to find out whether there's some distribution problem from the liquid injector of the oxidizer and fuel being used. It turned out we could probe it very nicely. We were using what was called a mass spectrometer to do the analysis, which is very slow and inefficient. We had heard through the Naval project officer of a new science called gas chromatography at the Naval Ordnance Test Station out at Inyokern. We went out there, and they had just bumped into some English work on gas chromatography. We became the experts in this field, a very simple technique. I published like five of the first seven U.S. papers on the subject of gas chromatography. We used it then to investigate what was going on inside the Titan engine, to find out if there was bad mixing or bad combustion going on in the various areas, and we then had the mechanical engineers redesign the injector.[279]

Project officers also managed the interface between industry partners. Sometimes this was necessary since competition and profit-motive acted to reduce output circulation. Levering Smith and

---

[279] Converse, Poole, and United States, *History of Acquisition in the Department of Defense*, 78; Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 78; Oral history interview with E. Milton Wilson, 8.

William Raborn, who were overseeing the Polaris project for the Navy, actively managed the relationships between actors within the Polaris project. The tensions that developed between Aerojet and Lockheed were so serious that Raborn hired a management consultant, Jack Dunlapp, to ostensibly "evaluate GE, and Aerojet's performance in the project." Raborn "suggested" that Lockheed use Dunlapp for their own performance evaluation, which of course they did. Effectively this added an experienced consultant to manage the interactions between all three companies, de-escalate tensions, and report back to Raborn.[280]

Raborn and Smith were also happy to direct Lockheed to look for external assistance when they felt issues were not being addressed. Late in 1957, the Polaris guidance systems were not developing fast enough, so they directed Lockheed to hire a Mr. Roth, an expert in the field. This was extremely awkward as Roth was already employed by Solar Aircraft Company, a Lockheed competitor. It was made even more awkward since the AIA frowned upon employee-poaching. Nevertheless, Hawkins wrote to Solar Vice-President of Operations W. G. Dollmeyer and requested Roth on a consulting basis. Within a month Roth was working full-time for Hawkins in Sunnyvale.[281]

Lockheed saw the close interaction with project officers, and even their intrusion into project management as an overall benefit. A benefit in which Lockheed was willing to invest time and. In 1955, the California military test-firing ranges were so overcrowded that it was having a material effect on completion deadlines. Lockheed proposed to build a new range – the Santa Cruz System Test Facility.

---

[280] Willis Hawkins, "W. M. Hawkins to S. W. Burress, Dunlapp and Assoc.," March 29, 1957, Box 30(11), Willis M. Hawkins Papers.

[281] Willis Hawkins, "W. M. Hawkins to W. G. Dollmeyer, VP Operations, Solar Aircraft Co.," August 13, 1957, Box 30(11), Willis M. Hawkins Papers.; Willis Hawkins, "W. M. Hawkins to L. E. Root, Operations Weekly Report," September 3, 1957, Box 31(1), Willis M. Hawkins Papers.; Johnson, "Basic Operating Rules of the Lockheed Skunk Works."

Not only did Lockheed propose to allow other companies to use the range, they planned to spend more than two million dollars on an office building at the facility specifically for Navy project offices and officers.[282]

Not everyone at Lockheed saw value in the military's duality in promoting output circulation while they also promoted strict security. Skunk Works founder Kelly Johnson was regularly frustrated by the military, and especially the Navy. In the early 1950's he had developed his own internal systems of security. Often these played on military his knowledge of military norms. For example, he directed that no documents in the Skunk Works should be stamped with their security classifications. The idea was that this would suppress curiosity and prevent prying eyes from differentiating between secret and non-secret work. At the macro-scale, Johnson saw that the military clearly shared Lockheed's secrets through the bidding process, DAG committees, and project officers. This leaky system frustrated Kelly so greatly that in 1967 he suggested to Lockheed President Carl Kotchian that he pull Lockheed out of military research entirely. He wrote that while Lockheed was being fed small contracts from the Air Force, "McDonnell and Martin are being funded for hardware programs to an amount 20 to 25 times that given to Lockheed. It appears that the Air Force merely uses us to develop ideas and analytical results which can be readily passed on to our competitors."[283]

Conclusion

In contemporary science, there remain significant hurdles to sharing knowledge between the government, industry, and academia. Academia has traditionally been the freest with the circulation of

---

[282] Willis Hawkins, "W. M. Hawkins to L. E. Root, Copy of Rough Budget," March 12, 1957, Box 30(11), Willis M. Hawkins Papers.

[283] Rich and Janos, *Skunk Works*, 32; Johnson, "C. L. Johnson to A.C. Kotchian," 1967.

output, but is naturally stifled by the need to trade secrecy for financial support. An information-based economy has made secrecy and security paramount concerns. In the years following the second world war, these values permeated governmental science policy and significantly reduced the flow of science between actors within the Triple-Helix. There were still opportunities to publish, but reciprocity relied on the spoken word, and the quiet action.

The military controlled large scale security processes, but were unable to control what individual scientists spoke to each other about. The aerospace industry exploited this fundamental security issue by developing systems which promoted personal interaction. The most common of these were platforms, or spaces in which scientists were relatively safe to talk. Usually these were temporary, sponsored, and structured events. The length of these interaction could be extended through recruitment into longer career paths.

Knowing this, the military actively suppressed output circulation from industry through secrecy culture, clearances, obfuscation, and thoughtful destruction. However, these strategies were most useful in controlling the written, not the spoken word. An added level of complexity to maintaining security were the mixed messages sent by the military. Scientific military staff were frustrated with the suppression of innovation that occurred concurrently with secrecy. They too employed formal committees, conventions, and human resource movement, but they also developed the use of project officers to become facilitators, either transferring the information themselves or introducing people and resources which encouraged output circulation.

With this information, and compared to contemporary science, we see that the marker for modern science is not simply output circulation, it is the persistence of output circulation in the face of a culture promoting secrecy and security. This culture, and the tools used to circumvent it, were the same in the early aerospace industry as they are today.

A final point is that the structure of innovation networks relied on recursive, reflexive, and complex patterns of communication in knowledge spaces. This modern system created a three-dimensional spiderweb through which knowledge passed as it developed. This was a polar opposite of the smooth flow of science under the Linear Model. So why then was the Linear model still in active use at the policy-setting levels of government throughout the 1950's and 1960's? Why is it still in use at those same levels today? The next chapter examines these questions.

Chapter 6.

Non-Linear Innovation

This chapter approaches non-linear innovation in the aerospace industry by examining the Triple-Helix stakeholder's responses to Project Hindsight which was an effort by policymakers to quantify, codify, and value innovation through the lens of the Linear Model. The chapter also includes examples of non-linear innovation which demonstrates the entire descriptive structure defining basic and applied science had ceased to have practical meaning in the 1960's.

The fourth differentiating characteristic of modern science management according to the Triple-Helix Model is non-linear innovation. In Henry Etzkowitz's conception of the Triple-Helix, non-linear innovation describes a complex network of communications between actors which motivates and influences knowledge development. The Linear Model, which had been reinforced to policy-makers by Vannevar Bush in *Science, The New Frontier*, was insufficient to describe how modern science was performed. Nevertheless, it was used as a framework for funding and organizing the American scientific enterprise. This chapter is not to argue against the validity of the Linear Model, for as Paul Forman wrote in 2007, "To campaign today against the Linear Model is to throw oneself against a door that has been wide open for two decades." Rather, it is to show that the model was promoted at the national policy level where its purpose shifted from explaining the process of innovation to justifying the funding primacy of applied research over undirected research. When the model was used for policy, its obvious inability to describe modern, non-linear innovation was recognized and opposed by contemporary researchers. These researchers recognized that a clean pathway where knowledge and innovation were passed through discreet phases did not exist, and diverse forms of collaboration between Triple-Helix stakeholders were essential for every innovation. [284]

Forman, in his *The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology (2007)* argued that directed, or applied, science gained primacy in the public consciousness in the early 1980's. However, from a policy perspective, the shift occurred over a period of several years in the 1960's and was exemplified by Project Hindsight (Hindsight). This was a large-scale study based on the Linear Model of how, and where, innovation occurred in the scientific establishment funded by the U.S. government after the Second World War. The initial results were published in 1966, and the final report came out in 1968. The results were a bombshell in the research community. Through both faint praise and direct attack, the report threatened to destroy the financial underpinnings of the independent research and development (IR&D) system in military contracting. It also explicitly undermined the cost-value equation of academic research. At the height of the cold war, the prospect of defunding undirected research pleased no one but budget-cutters. A close examination of how the results of Hindsight were reached, the response by Triple-Helix stakeholders, and the counter-reaction of policy-makers created a crisis point for post-war research. It marked the moment when directed industrial research gained political primacy over undirected academic science. [285]

Hindsight's authors aimed to, and failed to, quantify the respective values of research, development, and technology. The foundations of this project's research strategy were based on the

---

[284] Paul Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," *History and Technology* 23, no. 1–2 (March 1, 2007): 2; Etzkowitz and Zhou, "The Entrepreneurial University in Various Triple Helix Models."

[285] Henry Etzkowitz, "The Triple Helix and the Rise of the Entrepreneurial University," in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 50–53; Etzkowitz and Zhou, "The Entrepreneurial University in Various Triple Helix Models."

Linear Model and were thus foiled by the complexities of innovation in a triple-helix based scientific environment. Nevertheless, the project was an unqualified political success for shifting the focus of government research to practical outcomes.

The context of Hindsight can be traced back to the scientific establishment planned by Vannevar Bush in the years immediately after WWII. Bush chose to promote the Linear Model to simplify the mission of the National Science Foundation for policymakers in Washington. The model was elegant and gave clear roles for academia (basic research), industry (applied research, technological development, and then production), and the government (funding and policy control). Bush described this model as funnel shaped, where basic research was necessarily inefficient because it was impossible to predict practical outcomes. He wrote that:

> Many of the most important discoveries have come as a result of experiments undertaken with very different purposes in mind. Statistically it is certain that important and highly useful discoveries will result from some fraction of the undertakings in basic science; but the results of any one particular investigation cannot be predicted with accuracy.[286]

He went on to clarify this further by writing that "the simplest and most effective way in which the Government can strengthen industrial research is to support basic research and develop scientific talent." [287]

---

[286] Bush, *Science, the Endless Frontier: A Report to the President on a Program for Postwar Scientific Research*, 19.
[287] Bush, 21.

Independent Research and Development

The government expected early aerospace companies to develop knowledge at a pace at least commensurate with the U.S.S.R, and preferably much faster. Unfortunately, undirected, academic research is by definition slow to solve immediate practical problems. Even though Bush was a strong proponent of the Linear Model, it became clear in the 1950's that a neat, and discrete path for scientific knowledge development between academia and industry did not practically exist. There was a confusion on this point in policy circles. While Bush's linear roadmap for science policy did not provide for basic research in industry, it still emphasized basic research. He wrote that "this general knowledge provides the means of answering a large number of important practical problems, though it may not give a complete specific answer to any one of them. The function of applied research is to provide such complete answers."[288]

Unfortunately, this philosophy did not take into account the urgency of the Cold War, and undirected science was too slow. As a result, military contractors established their own research facilities to practice undirected research in militarily important fields. Generally, the costs for these research establishments were reimbursed by the military as a cost of business, but they were rarely attached to specific contracts. The research at these industry-based labs was conducted without government oversight, like university research, and industry was "trusted" to invest resources wisely. This trust-based system was short-lived and in 1956 the Department of Defense (DoD) established a committee to tighten oversight of industry-based basic research.[289]

---

[288] Bush, 18.

[289] Smith, *American Science Policy since World War II*, 73–77; David Asner, "The Linear Model, The U.S. Department of Defense, and the Golden Age of Industrial Research," in *The Science-Industry Nexus: History, Policy, Implications : Nobel Symposium 123*, ed. Nina Wormbs, Sven Widmalm, and Karl Grandin (Science History Publications/USA, 2004), 13–23.

The DoD suggested that all basic research be pre-approved as a limited portion of existing systems contracts instead of funding the research directly. This effort to reign-in undirected spending was disrupted in October of 1957 with the launch of Sputnik. L. E. Root of Lockheed, and R. T. Hurley of Curtiss-Wright argued before the Senate that to catch up with Soviet advances in aerospace, any changes to the Armed Services Procurement Regulation XV (ASPR XV – which defined the rules around military funding of industry R&D) should result in far more liberal funding and control of industry-based research. Ivan Getting, VP of Engineering and Research at Raytheon, was a prominent member of the science policy community and he also argued that liberalizing the funding of industrial R&D would stimulate private investment. He said that:

> Basic research within industrial organizations should be an allowed cost against all Government contract of all kinds, and that the tests to be applied are not those of the detailed technical programs being pursues, but whether the amount of money is reasonable and properly spent.[290]

Getting advocated that *all* government contracts, both fixed-cost and reimbursement, should allow contractors to charge a reimbursement for research and development. He also wanted the government to look at the financial process, not the scientific outcomes, to determine research value. This was so policy-makers could look at a more granular accounting of spend and reduced the risk of end-result sticker shock. Throughout 1958 and 1959, Getting lobbied diligently for these outcomes. One of the primary arguments used by Getting and Ernest Leathem, also of Raytheon, was that the Linear

---

[290] Asner, "The Linear Model, The U.S. Department of Defense, and the Golden Age of Industrial Research," 8.

Model required a clear, and time-consuming, path for innovation in research. Universities were too slow in innovation so industry needed the freedom to pick up the slack.[291]

By the end of 1959, the ASPR XV had been updated to allow significant new sources of funding for industry-based research and the model of Independent Research & Development (IR&D) was codified, despite the fact that it was not envisioned in Bush's conception of the scientific enterprise. However, the new process worried the Government Accounting Office. They saw IR&D as a path of uncontrolled spending. ASPR XV was not thorough on procedure and there were as many systems of billing the military for IR&D as there were participants in the program. In 1963 the DoD suggested further revisions to ASPR XV to align language and billing practices. This suggestion triggered the Defense Science Board to perform their own study into DoD spending on basic research, and their result was that the IR&D process was taking resources from the rightful recipients: universities. At this point, the government looked for opportunities to reign in research expenses.[292]

Project Hindsight - The First Interim Report

In 1963 Harold Brown was the Director of Defense Research and Engineering and he was in charge of revising ASPR XV in such a way that it implemented safeguards against abuse, did not suppress academic science, and continued to promote basic science in industry. These goal faced political headwinds. In 1963 the public was disillusioned with science and military technology due to the Vietnam war. The public's reasoning was that if the United States was unable to win a war against a tiny, undeveloped, and agrarian nation, what good was spending billions of dollars on the military when there

[291] Asner, 9.
[292] Smith, *American Science Policy since World War II*, 73–77; Asner, "The Linear Model, The U.S. Department of Defense, and the Golden Age of Industrial Research," 13–23.

were underfunded social programs? Secretary of Defense Robert McNamara felt this pressure keenly and pushed Brown for greater efficiency and cost controls over government funded research. In 1964 Brown began a series of small studies to determine the efficacy of the DoD's IR&D policy. Ultimately these formed the basis of a much larger study which Brown approved in 1965: Project Hindsight.[293]

The political motivations of Hindsight were clear in the first interim report which was released on June 30, 1966 after two and a half years of research. In it, the primary authors, C. W. Sherwin, and Col. R. S. Isenson, wrote that their research goals had been twofold:

> (1) to identify and firmly establish management factors for research and technology programs which have been associated with the utilization of the results produced by these programs; and (2) to measure the overall increase in cost-effectiveness in the current generation of weapon systems compared to their predecessors (when, such can be identified) which is assignable to any part of the total DOD investment in research and technology.[294]

The research in Hindsight consisted of a deep analysis of the innovation events in the research and development of 20 separate weapons systems. The team identified 556 events in which a necessary innovation was made. The study used three key assumptions to identify these Critical Technology Events (CTE's), or moments of undirected, directed, or technological innovation. The first of these assumptions was that these CTE's were to have occurred since WWII, since going back further was too labor intensive for the study. The second was that each CTE carried the same outcomes value, which even the authors

---

[293] Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 109; Asner, "The Linear Model, The U.S. Department of Defense, and the Golden Age of Industrial Research," 14.
[294] Chalmers W. Sherwin and Raymond S. Isenson, "First Interim Report on Project Hindsight" (Office of the Director of Defense Research and Engineering, June 30, 1966), 1.

knew was disingenuous but they did not have a better solution. Finally, that the CTE's were defined by the Linear Model phase in which they occurred, not the predominant nature of the science used. For instance, if a discovery about the fundamental properties of a metal like titanium was necessary for the completion of a system, it would be considered part of applied research or engineering if innovation was applied toward the end of a project timeline *even if* the original innovation occurred in an undirected format; it was the moment of innovation-use that determined where in the Linear Model the CTE would be counted. These assumptions provided a deterministic effect under which CTE's were structurally more likely to occur recently (within 20 years), in higher number and therefore with more total value (since all CTE's were of equal value), and in the final, applied, stages of the Linear Model.[295]

The report defined only two discrete and rigid categories of science, directed and undirected. Undirected research was defined with the following characteristics:

1. Observation of phenomena.
2. Formulation of hypothesis.
3. Design of experiment to test hypothesis.
4. Conduct of the experiment.
5. Analysis and interpretation of results within the scientist's frame of reference.
6. Report to the community.

Directed research technique, on the other hand followed a different protocol:

1. Statement of problem.

---

[295] Sherwin and Isenson, 3, 9; Chalmers W. Sherwin and Raymond S. Isenson, "Project Hindsight," *Science* 156, no. 3782 (1967): 1575–76.

2. Morphological survey of available and apparently relevant knowledge for a possible solution, or deliberate search for new knowledge leading to a proposed solution.
3. Design of experiment to test proposal.
4. Conduct of experiment.
5. Analysis and interpretation of results within the frame of reference of the problem.
6. Report to the "source" or the problem.[296]

The first of these processes is the deductive scientific method dating back to the seventeenth century. It focused on purely deductive reasoning based on observation. The second method is more inductive, but neither model the combination of induction and deduction used by scientists at the time. Instead, the authors were attempting to create categories which matched Mertonian norms.

[296] Raymond S. Isenson and Chalmers W. Sherwin, "Project Hindsight, Final Report" (Office of the Director of Defense Research and Engineering, October 1969), 14.

Additionally, the linear pathway between science, technology, and engineering was also rigidly defined in the following table from the final report (figure 4).

Table II. CLASSES OF RESEARCH AND EXPLORATORY DEVELOPMENT SUGGESTED IN PROJECT HINDSIGHT

	RXD Class
Science	Investigations in pure and applied mathematics and theoretical studies concerning natural phenomena (R).
	Experimental validation of theory and accumulation of data concerning natural phenomena (R).
	Combined theoretical and experimental studies of new or unexplored fields of natural phenomena (R).
Technology	Development of a new material necessary for the performance of a function (XD).
	Conception and/or demonstration of the capability to perform a specific elementary function, using new or untried concepts, principles, techniques, materials, etc. (XD).
	Theoretical analysis and/or experimental measurement of the characteristics or behavior of materials, equipment, etc., as required for design (XD).
Engineering	First demonstration of the capability to perform a specific elementary function, using established concepts, principles, materials, etc. (XD design).
	Development of a new manufacturing, fabrication or materials-processing technique (XD mfg.).
	First development of a complete system component, equipment or major element of such equipment, using established concepts, principles, materials, etc. (AD).

Notes: R - research
XD - exploratory development
AD - advanced development

Figure 4 - Classes of Research Defined in Project Hindsight[297]

[297] Isenson and Sherwin, "Project Hindsight, Final Report."

By 1967 the crossover between the practice of science between stakeholders made this chart both misleading and meaningless. In both the literature of the time, and the experience of researchers, there were examples of all nine of these RXD classes in *Figure 4* thriving in each of the Triple-Helix stakeholders.

Additionally, this combination of rigid definitions applied to a modern form of scientific investigation meant that almost any event would be identified as directed science in the technology or engineering phases of a system's development.

Despite its myriad design shortcomings, one finding in particular was at the core of the negative reaction to publication. Of all the innovations necessary to build immensely complex weapons systems, the authors found that "39% were performed by in-house (military) laboratories, 49% by industry, and 9% by universities." [298]

While it was not the expressed intent to denigrate or damage the prestige and funding opportunities of academia compared to industry, and the military, Hindsight did just that. The university community focused on the outcome that the American scientific endeavor only relied on university research for a paltry 9% of practical outcomes stemming from government funding. This was not a superficial insult, but an existential threat to undirected research.

The Response from Academia

The response was swift and forceful. Physicist Leonard I. Schiff, the President of the Faculty Senate of Stanford University, attacked the project on three fronts. The first was that the fruits of undirected research have been incorrectly "transmuted into routine techniques, without which

[298] Sherwin and Isenson, "First Interim Report on Project Hindsight," 9.

commonplace experimental and computational procedures would be performed as they were 20 years ago, not as they are today." His second argument was that Hindsight had emphasized the importance of capable people, and these people could only have been generated in the University setting. Finally, Schiff recognized that the research was done in such a way that it ignored the importance of communication between the scientific community, industry, and in-house military researchers. He wrote that "Such contacts with the academic scientists supported by DOD grants or contracts are among the highly valued by-products of the military basic research program."[299]

Leonid Azaroff, the Director of the University of Connecticut's Institute of Materials Science identified the core issue with Hindsight was that the research was that it only examined successful events and systems. He hypothesized how much more successful, or rapidly developed, or financially efficient a system would have been if a stronger base of science had been available. Moreover, he asked "How much would have been accomplished without the underpinnings that undirected investigations tend to provide?" Donald O. Walter of the UCLA Space Biology Laboratory also identified the retrospective nature of the project as an issue. By necessity, moving backwards through a series of events from a successful outcome to a genesis provides a deterministic and artificially error-free pathway. Walter's suggestion was a new, more balanced study, that moved in a *prospective,* rather than a retrospective direction.[300]

In 1967 Texas A&M professor of Chemistry Andrew D. Suttle addressed the Defense Science Board to express his concerns about Hindsight. He determined that the narrative of the development of the studied systems was accurate, but called it "wholly inadequate for drawing conclusions about the

---

[299] L. I. Schiff et al., "How Perceptive Is Hindsight?," *Science* 155, no. 3761 (1967): 397.
[300] Schiff et al., 398.

contributions of basic research." The board agreed that many of the events described in the study were of unequal contribution, and several events could have been split into more events still. The consensus was that there were "many important things to be gleaned from the 'Hindsight' study but that it is not the whole story."[301]

The overall frustration for the academic research community was Hindsight's obstinate ignorance of one specific and obvious fact – that none of the weapons systems could possibly have existed without basic research as a foundation. While there were several contemporary identifications of key faults in the report which made it unreliable, the greater issue was that it used Vannevar Bush's simplified Linear Model to approximate a system for which it simply did not fit. The complex science necessary to produce weapons systems was equally complex in its development path.

Project TRACES

In addition to a public campaign of resistance to Hindsight, the academic science community, and specifically the NSF, funded their own research project to oppose the conclusions of Hindsight. The project, Technology in Retrospect and Critical Events in Science (TRACES) was undertaken by the Illinois Institute of Technology Research Institute on behalf of the NSF. A smaller study than Hindsight, TRACES had a budget of only $164,000 and studied only five technological innovations which were chosen randomly from a list of twenty: the electron microscope, the oral contraceptive pill, the video tape recorder, magnetic ferrites, and matrix isolation in chemical manufacturing. The goal of TRACES was not

[301] Poole and United States. Department of Defense. Historical Office, *Adapting to Flexible Response, 1960-1968*, 116.

to directly rebut or debunk Hindsight, but to highlight Hindsight's flaws, and to emphasize the importance of undirected research.

The research technique was almost identical to Hindisght; to trace back through key innovation events and determine which were "nonmission research," "mission-oriented research," and "development and application." These categories were directly analogous to Hindsight's categories of undirected science, applied science, and technology. TRACES specifically looked back as far as the researchers were able to identify mission-oriented research, and did not choose an arbitrary (and recent) date which the Hindsight team had done.

Not surprisingly, the results of TRACES were the inverse of Hindsight. Of the 341 innovation events studied, 70% were from nonmission research, 20% from mission-oriented research, and only 10% were development and application. The results were also temporally analogous to the Linear Model. The vast majority of nonmission research (90%) was completed more than a decade before the mature technology was available. However, the study took pains to point out the coexistence and cooperation of both nonmission and mission-oriented research (reflexive output circulation) was essential to innovation in both fields.[302]

Industry Response to Hindsight

A political goal of Hindsight was to justify a reduction in public funding of private research. This goal directly threatened the IR&D system that the aerospace industry considered indispensable. On the other hand, the results of the study were also critical of academically-based undirected research, and

---

[302] Peter Thompson, "TRACES: Basic Research Links to Technology Appraised," *Science* 163, no. 3865 (1969): 374–75; National Science Foundation (U.S.) and IIT Research Institute., *Technology in Retrospect and Critical Events in Science* ([Washington, D.C.]; Springfield, Va.: [National Science Foundation] ; Distributed by NTIS, 1968).

offered a way to argue against Total Package Procurement. These outcomes were helpful to industry, so the public position of industry was carefully crafted to appear balanced. Ultimately it was critical of the study but measured in tone. The Interim Report was, after all, interim and there was some hope that appropriately applied pressure could temper the final results.

In 1967, Willis Hawkins served as the Chair of the Aerospace Industries Association of America's (AIA) Technical Management Division (TMD). This division was originally established in 1966 as a primary industry bulwark against Total Package Procurement, so it was appropriate that this committee (and Hawkins) gained responsibility for the aerospace industry's official response to Hindsight. He began by asking Harper Q. North, V.P. of TRW, to call Isenson (one of the Hindsight authors) and request the raw data he used. In the discussion with Isenson, North was extremely complementary of Hindsight on behalf of the AIA. Isenson was both gratified and surprised to hear of the AIA's positive reception since "Universities had not liked the findings that so few events in systems had come from undirected research, and he had heard of industry annoyance that Exploratory Technology had not been the source of more events." Even so, Isenson was hesitant to turn over original data since much of it was extremely candid, and he also feared it would encourage industry head-hunting. These were legitimate concerns but ultimately, he agreed to send the full file on a single Hindsight study project, the AN/SPS-48 Radar, provided that North burn it after examination. North was subsequently allowed to share the file with Hawkins who literally burned it on 3-3-67.[303]

Hawkins requested that J. C. Snodgrass, the Executive Secretary of the TMD send a formal letter to Isenson from the TMD, thanking him for his cooperation and, on behalf of "industry", making two

---

[303] Harper Q. North, "H. Q. North to W. M. Hawkins," February 9, 1967, Box 5(4), Willis M. Hawkins Papers.; Harper Q. North, "H. Q. North to R. S. Isenson," February 16, 1967, Box 5(4), Willis M. Hawkins Papers.; Harper Q. North, "H. Q. North to W. M. Hawkins: Hindsight - Task I Documents," February 16, 1967, Box 5(4), Willis M. Hawkins Papers.

suggestions: that Hindsight be continued to a final report, and that the raw data be made available to contractors for their own internal evaluation activity.

Hawkins also drafted an official response to Finn J. Larsen, the Deputy Director of the Department of Defense's Research and Engineering department, from C. R. Lowery the Director of the Aerospace Technical Council. This was to become the official aerospace industry statement on Hindsight. Hawkins sent the drafts to members of the TMD for editing, and by July, a final draft was accepted and sent. It was ostensibly complimentary of the study, but suggested some changes before the final report was complete. The AIA felt that "different conclusions" could be drawn from the raw data, specifically that many of the events were inappropriately defined as "applied science" because they occurred in the acquisition phase of contracting, but were in fact, undirected science. Moreover, the letter said "this might be an additional consideration in determining the appropriateness of the use of firm fixed price contracts and the Total Package Procurement Concept." With these ideas in mind, Hawkins suggested that Hindsight be expanded with newer, successful systems that were produced under TPP.[304]

This insight by Hawkins into the value of Hindsight as a defense against TPP complicated the official tone of the industry response. The letter was restrained and sought to influence without a risk of offense. Larsen's response took no particular efforts in diplomacy. It was peremptory and blunt. Regarding the suggestion that Hindsight be continued, Larson saw no point in this from the government position. He wrote that:

---

[304] Finn J. Larsen, "F. J. Larsen to C. R. Lowery," July 1, 1967, Box 5(4), Willis M. Hawkins Papers.

> From our point of view, the study answered a number of our questions such as: to what extent can DoD count on non-defense agencies and the commercial sector to develop, independently, the basic technology needed for our weapon systems? The HINDSIGHT study also identified technologies which have been most important in the development of our current weapon systems. These answers and findings should retain their validity for a few years.[305]

Larsen went on to suggest that if the AIA wanted additional projects in the study, they could carry on the program themselves. They did just that. In his diary Hawkins wrote that he had been given the responsibility to write "letters to all ATC members requesting suggestions on helping SPO to do good job and also asking for 'hindsight' projects I.e. successful ones with various contract schemes." He ultimately collected a set of potential studies comprised of 21 contracts within 11 weapons systems spread over 7 aerospace companies. Hawkins was highly motivated by this project due to the pressure he was feeling from TPP on the C-5 Galaxy project, and the general industry concern that the government was looking for ways to cut funding to basic research.[306]

Hawkins also took every opportunity to discuss Hindsight with Larsen in person. A few days after receiving Larsen's letter, Hawkins met with Larsen in Washington and came away with the perspective that Larsen had been disappointed in Hindsight's results and he was looking forward to moving on from it. They met again several times over the next year, and by April, 1968, Hawkins felt that Larsen looked on Hindsight "more favorably," and was willing to consider continuing the project.[307]

---

[305] Larsen; Willis Hawkins, "Address to the Lockheed Missile and Space Company Research Colloquium," 22.
[306] Willis Hawkins, "Diary Entry 7/10/1967," July 10, 1967, Box 2(1), Willis M. Hawkins Papers.
[307] Hawkins; Willis Hawkins, "Diary Entry 4/30/1968," April 30, 1968, Box 2(1), Willis M. Hawkins Papers.

Unfortunately, Larsen did not continue Hindsight with Hawkins' suggested improvements. Neither did the industry generally share Hawkins' enthusiasm for an expansion of Hindsight. He requested from industry peers the same categories of information he had seen from Isenson's AN/SPS-48 Radar file, but what he received was woefully inadequate. Ray Blaylock of Ling-Temco-Voight, for instance, sent Hawkins only a four-page outline of the A7-A project and made it clear he would only share unclassified information. This, and Hawkins' expiring chairmanship of the TMG, tempered Hawkins' enthusiasm for the project. In 1968 he sent all his work to Herm Shipley, the new chair of the TMG and told him to use his "judgement as to whether this should be introduced by you or someone else, or whether you should submit it with another letter to Dr. Larsen."[308]

However, the work did not go to waste. When in 1968 the Congressional General Accounting Office digested Project Hindsight's initial results, they launched their own investigation of how government-funded IR&D had been for the public benefit. This caused John S. Foster, who had replaced Harold Brown as the Director of Defense Research and Engineering, concern because he understood that IR&D had an important role in innovation. His position was that while policy-makers were intent on cutting costs, officials like himself looked to protect the system which had been so successful for years while still working to limit "cost overruns, performance underruns, and schedule delays." He wrote to Hawkins and Kelly Johnson with several questions and a request for examples of IR&D that had been of

[308] Willis Hawkins, "Diary Entry 10/13/1967," October 13, 1967, Box 2(1), Willis M. Hawkins Papers.; Hawkins, "Address to the Lockheed Missile and Space Company Research Colloquium"; Larsen, "F. J. Larsen to C. R. Lowery," July 1, 1967; Willis R. Hawkins, "W. M. Hawkins to H. E. Shipley," February 22, 1968, Box 5(4), Willis M. Hawkins Papers.; R. C. Blaylock, "R. C. Blaylock to G. A. Busch: A7-A Project Data," March 13, 1968, Box 5(4), Willis M. Hawkins Papers.; Blaylock.

benefit to the public. Kelly used the opportunity to plead for a return to the flexibility of the past IR&D systems and an end to TPP. Hawkins forwarded his list of potential Hindsight continuation studies.[309]

While the macro-level response of industry was measured, the individual responses indicated concern and frustration. Hawkins' personal reaction was summed up in a speech he gave to the LMSC Research Colloquium in January of 1967, just a few months after the initial report was released. Hawkins was by this time confronted with several ways the government was trying to exert control over the Triple-Helix. He was coping with Total Package Pricing, Program Evaluation Review Technique, and more than 370 Armed Services Procurement Regulations. To Hawkins' eye these all throttled innovation. But Hindsight was especially aggravating. He said:

> There have been some rather interesting evaluations that have taken place over the last few years to try to put the Department of Defense in a position to defend its research and development dollars, and I think some of you have heard of quite a few of these activities. The management schemes that have been created to evaluate contractors and evaluate laboratories are, I think, worse than useless. They get you into nothing but trouble and they don't really see the woods for the trees, or vice versa, whatever way it is supposed to go. But one thing did take place and that was the project charted by Chal Sherwin, which is called Project Hindsight. Now I have worked with Chal for a long time and I don't think I have ever been madder at anybody in my life.[310]

---

[309] Asner, "The Linear Model, The U.S. Department of Defense, and the Golden Age of Industrial Research," 17–19; J. S. Foster, "J. S. Foster to C. L. Johnson," December 10, 1968, Box 5(5), Willis M. Hawkins Papers.; W. M. Hawkins, "W. M. Hawkins to J. S. Foster," February 13, 1969, Box 5(5), Willis M. Hawkins Papers.; C. L. Johnson, "C. L. Johnson to J. S. Foster," December 10, 1968, Box 5(5), Willis M. Hawkins Papers.
[310] Hawkins, "Address to the Lockheed Missile and Space Company Research Colloquium," 18.

Hawkins' believed that Sherwin "had an axe to grind" with industry, but Sherwin's results had some merits. The frustration was with the blind acceptance of the researcher's methodological shortcomings. First, Hawkins believed that Hindsight had identified innovation events as isolated and credited to a person who understood both the entire system, and the need for innovation. This process excluded the "guy that necessarily made the invention that got them over the hump." The systems manager was credited because he understood the entire system, but the innovation would never have been made without the people who only understood a very tiny portion of the problem, but were capable of providing the insight necessary for innovation to take place. Effectively, Hawkins was saying that information flows back and forth through many conduits and processes, and identifying a single innovation event is an artificial construct because "you needed both of these guys to cause an event to take place, the chap who understood enough of the problem to see the need, and the other guys who understood enough of the technique to do the invention."[311]

Another poor design Hawkins identified was the fact that the researchers had chosen 1945 at the backstop for their research. Hawkins said that "They didn't try to go backwards any further than that and the lead time from a research event to a completed project is longer than that, so he didn't trail it back to the research event that made it all possible in the first place." This gave no credit to the basic scientific research which was often the foundation of these systems all because the bulk of the research had occurred before 1945.[312]

Hawkins' final frustration was that no one else seemed to see the obvious argument against TPP. TPP did not have bandwidth available for research cost overruns beyond the original contract. This

---

[311] Hawkins, 19–20.
[312] Hawkins, 20.

effectively placed the burden of R&D on the contractor to forecast how much innovation would be needed. Since TPP was squarely based on a Linear Model of science, there should be no basic research or innovation done after a given system moves from research into engineering development. Yet according to Hindsight, 37% of all innovation occurred *after* the project had moved into production. Hawkins said "If this does not suggest that the package procurement program is inhibiting invention, if nothing else does – this does. Nobody has drawn the two lines together that I know of but it is standing there in the Project Hindsight Report to be looked, if somebody will just look at it." It is this position that explains Hawkins initial, and unshared, motivation to continue Hindsight despite its flaws – it was a path to using the government's own data to show that TPP stifled innovation.[313]

The Military Response to Hindsight

The military response to hindsight was also conflicted between the macro-level response, and the micro, or individual, level response. It would have been insubordinate for the military's scientific leadership to speak out collectively against Hindsight, but the individual-level response resembled that of Hawkins. Hindsight was seen by military researchers and managers as an attack on undirected research which was a crucial tool in innovation. The close communication between user (the military), the contractor, and the researcher was recognized by all three actors as a necessary part of innovation. The frustration they all shared was the financially-motivated ignorance of policy makers who cared only about this conclusion in Hindsight – "that, in the systems we studied, the contributions from recent (post 1945) research in science were greatest when the effort was oriented." With this phrase, policymakers justified cutting funding to both academic, and industry-based undirected research.[314]

---

[313] Hawkins, 21.
[314] Sherwin and Isenson, "First Interim Report on Project Hindsight," 12.

Robert Frosch, the Assistant Secretary of the Navy for Research and Development, gave a speech in October of 1967 addressing the military procurement community. In the speech, he strongly criticized the contemporary cost-control practices of policy-makers. He said the government's policies and practices were "based on false principles, and incorrect assumptions, and in many cases, just plain ignorance." He suggested that the problem with TPP was that two groups are fundamentally agreeing on a total price of an object that does not yet exist. The production of that object is impossible to plan in a linear format, and must be developed iteratively. Each iteration required a network of communication between scientists, engineers, and project managers. Research, according to Frosch, was about paying people to learn something, not the physical report they ultimately produce. "When we want to apply what a [researcher] has learned," he said, "we don't do it by reading the research report, handing it to an engineer and saying "Build me something out of this." What we really do is put the engineer in touch with the [researcher]." Fundamentally, Frosch believed that stifling undirected research, stifled outcome circulation between stakeholders, and therefore stifled innovation.[315]

However, not everyone in the government agreed with Frosch. The hostile reception to Hindsight at the individual level did not surprise the policy-makers who supported the report. In March of 1967, Donald MacArthur, the Deputy Director of Defense Research and Engineering gave a speech to the Washington Academy of Science defending Hindsight. His goal was to be "very clear on what has, and has not been shown by the project so far," MacArthur started his speech with an ex post facto revision of the goals of Hindsight. He said the goals of Hindsight were twofold:

(1) How did post-1945 science and technology contribute to the development of new military equipment?

---

[315] R. A. Frosch, "Address to the National Security Industrial Association Symposium."

(2) Were DOD's requirements for science and technology relatively demanding as compared to the requirements of industry or other Federal agencies?[316]

These are quite different from the goals stated explicitly in the first interim report which says the goals were:

> (1) to identify and firmly establish management factors for research and technology programs which have been associated with the utilization of the results produced by these programs; and (2) to measure the overall increase in cost effectiveness in the current generation of weapon systems compared to their predecessors (when such can be identified) which is assignable to any part of the total DOD investment in research and technology.[317]

Having determined a retroactive goal for the program, it was easy for MacArthur to demonstrate that Hindsight had been a remarkable success. On three occasions, MacArthur assured the reader that the goal of the program was not to diminish the importance of university-based basic research compared with directed research and technology, nor was it designed to reduce funding to basic research. He said that none of the study's findings conflicted with his strong philosophical commitment to high-quality basic research. He then went on to cite the conclusion of the study, that Ph.D.'s contributed less than 2% of science: that "problem-oriented environments" are the most profitable for government investment, that an algorithm predicting the value of a system should be definable, and finally, while Hindsight had failed to create an equation linking investment and quality of

---

[316] Donald M. MacArthur, "Some Hindsight on Project Hindsight," *Journal of the Washington Academy of Sciences* 57, no. 4 (1967): 85–92.
[317] Sherwin and Isenson, "First Interim Report on Project Hindsight," 2.

output, he was sure there was a link somewhere. These were, of course, the same conclusion which had raised the ire academia in the first place. He ended the speech with a call back to the Linear Model saying:

> I think a crucial question for our colleagues who do research-on research is: "How does basic research 'feed' our applied research and exploratory technology programs?" We have to understand much more about the obvious dependence of applied research and technology on the growth of fundamental knowledge.[318]

This speech and its subsequent publication in *Science* failed to help the public relations issues of Hindsight, so the Department of Defense tried again. In April, Sherman and Isenson, the original authors, published a detailed article in *Science* attempting, again, to articulate why people should not be upset about Hindsight. Again, the objectives of Hindsight were revised retrospectively and explained as:

> One of the objects of Project Hindsight was to try to provide such answers; that is, to try to measure the payoff to Defense of its own investments, in science and technology. A second object was to see whether there were some patterns of management that led more frequently than others to usable results and that might therefore suggest ways in which the management of research could be improved. In particular, we wanted to determine the relative contributions of the defense and non-defense sectors, and, within the defense sector, the relative contributions of in-house laboratories and those of contractors.[319]

---

[318] MacArthur, "Some Hindsight on Project Hindsight," 92.
[319] Sherwin and Isenson, "Project Hindsight," 1571.

Again, the authors restated the procedures and results of the Interim Report, which relied on a reverse-engineered Linear Model that travelled backwards from event to event implying that its original pathway forward was linear, coherent, and neat. But the authors added an entire section called the *The Case for Undirected Science* which muddied their original outcomes by directly disputing the Linear Model. They made it clear that a longer timeline (pre-1945) would have added many more undirected events, and that science moves forward in a way that "is clearly not the simple, direct sequence taught by the folklore of science." They emphasized the importance of undirected research, but only when it is "co-located with, and skillfully related to an applied research and development organization." They went on to say that undirected science is not as effective as "evaluated, compressed, organized, interpreted, and simplified scientific knowledge that we find to be the most effective connection between the undirected research laboratory, and the world of practical affairs." In essence, they wrote that undirected science had its greatest value when it was directed.[320]

This effort was also unsuccessful in mollifying the greater scientific community. Nevertheless, the final report which was released in 1969 contained a lengthy, and similar, discussion of the importance of undirected science. After a great deal of coverage for the Interim Report, the journal *Science* published only a small, tongue-in-cheek article entitled *Hindsight Study Adds Kind Words for Basic Research.* The findings paraphrased in the article were that:

> (i) contributions from basic undirected research to military needs have-since 1945-been small; (ii) utilization of research findings has been accelerated when the practitioner has been working in areas related to military technology; and (iii)

---

[320] Sherwin and Isenson, 1566–67.

production of timely knowledge is achieved best when DOD funds and manages its own programs."

On the other hand, the article made it clear that Hindsight now sees a role in academic research, saying "The final report emphasizes the training value of basic research for practitioners of applied and developmental research. The article clearly demonstrated that the academic hostility towards Hindsight remained undiminished even if by 1969 the importance of the report had diminished. [321]

The Codependence of Research and Development

The Linear Model implies a relatively clean movement of knowledge from undirected research to applied research, then on to development. Hindsight, having been written in retrospect, only could trace a perfect path backwards through linked innovations. However, the path of science is generally far less clean and features stops, starts, restarts, dead-ends, recursions, failures, and successes as paths toward outcomes are developed. This confused, and complex path, along with the importance of communication between researchers and developers, was visible throughout the aerospace industry. The codependence between research and development was understood by Hawkins, but was not specific to Lockheed. Malcolm Currie, for example, who in in 1954 joined Hughes Research Laboratories as a research scientist, founded a research group studying traveling-wave tubes. This was undirected research which Currie remembered was "all IR&D." However, his goal was still practical in nature since he was convinced that traveling-wave tubes would be central to the next general of radar, which ultimately it was. The use of researchers as problem-solvers for engineers was demonstrated by Currie's influence on the Syncom project – the Earth's first geo-synchronous satellite. He remembered clearly

[321] F. C., "Hindsight Study Adds Kind Words for Basic Research," *Science* 159, no. 3813 (1968): 413–413.

how engineer Harold Rosen (the Syncom team lead) approached him to solve the issue of not having a small, but powerful broadband transmitter.

The way that happened, he and I were standing in a men's room one day, and he said, "Hey, Mal, you know, Don Williams and I, we think we can build a little satellite, to go up in geosynchronous orbit." Don Williams was the one who invented the control system and the station-keeping for that. He was a genius—he and Hal Rosen, the two of them. He said, "But it can only weigh 27 pounds, and we need a broadband transmitter. What do you think?" So I put aside some funds—or pirated them, whatever you want to say—and we built the first traveling-wave tube that went in that Syncom. Then we broadcast the Olympics from Japan in 1964.[322]

He went on to emphasize the importance of IR&D in this process, saying "So that was IR&D. IR&D built the first Syncom. IR&D built the tube that went into Syncom, I know that."[323]

Non-linear innovation, in addition to occurring outside the defined physical spaces of the Linear Model (universities, industry labs, etc.), can also be characterized by occurring in the final temporal phases of application. There are several examples of this in the development of the CL-400 Suntan and the SR-71 Blackbird at the Skunk Works under Kelly Johnson.

The CL-400 Suntan was preliminarily designed in 1954 as an aircraft of extremes. It was to fly at 100,000 feet, at Mach 2, and use liquid hydrogen as a fuel. To carry enough fuel, the plane would need to be 160 feet long – longer than a modern Boeing 737. Lockheed believed they could have a working prototype in only 18-months. For this to happen, Kelly Johnson created a schedule which conducted

[322] Oral history interview with Malcolm R. Currie., 8.
[323] Oral history interview with Malcolm R. Currie., 10.

basic and applied research concurrently. No research into the behavior of liquid hydrogen at high temperatures or altitudes existed, so the Skunk Works team built several prototype fuel tanks and ran hundreds of experiments to determine both the behavior of liquid hydrogen at various temperatures, and the optimum design for the tanks. A series of 61 ignition tests were conducted to define explosion limits on mixtures of fuel-ready liquid hydrogen and oxygen. Interestingly, only twice did these experiments produce a hydrogen fireball, and both times it was of a lower magnitude and energy dissipation than kerosene (jet-fuel). These results were unexpected since at that time, hydrogen was considered to be an extreme explosion hazard.[324]

Other areas of materials research were necessary at cryogenic temperatures. Pumps, seals, structural materials analysis, pipes, and injectors. Materials such as those used in the wing, needed to be tested hot since at Mach 2, the wing surface would reach more than 400 degrees Fahrenheit. Several 1/3 scale model wings were constructed and structural and surface materials were tested over and over in giant asbestos lined ovens. As we have seen previously, the CL-400 never flew. Johnson himself requested to cancel the project over impossible logistical issues, but the immense amount of knowledge developed was circulated to Convair to aid in the Centaur rocket program. This knowledge was also put to use almost immediately in the Skunk Works on another extreme design – the SR-71 Blackbird. This aircraft did fly.[325]

The initial proposal for the SR-71 was to augment the mission of the U-2 reconnaissance aircraft. In 1959, Johnson proposed an "air-breathing, manned vehicle that would fly faster than three times the

---

324 Rich, "Speech to Encino Men's Club"; Rich, "Speech to Rensselaer Polytechnic Institute"; Ben R. Rich and Russel C. Sessing, "Cryogenic Fuel Handling and Use in Airbreathing Aircraft," Conference Paper, October 21, 1974, Box 4(4), Ben R. Rich Papers.
325 Rich and Sessing, "Cryogenic Fuel Handling and Use in Airbreathing Aircraft."

speed of sound and at an altitude above 80,000 feet-the lower third of the stratosphere. This contrasted with 460 miles per hour for the U-2, which flew at heights of 70,000 feet." This proposed speed, at this great altitude meant that the skin temperatures of the aircraft ranged from 600-1300 degrees Fahrenheit. This required new science in petrochemical chemistry, hypersonic fluid dynamics, and perhaps most of all, material science.[326]

When the SR-71 had reached the prototyping stage, Johnson needed research assistance on two chemistry issues. This first was that there were no commercially available hydraulic oils which could be heated to over 1000 degrees and still lubricate. Kelly thought he had found a fast solution writing that he…

> saw ads in technical journals for a "material to be used to operate up to 900 °F in service." I contacted the producer who agreed to send me some for testing. Imagine my surprise when the material arrived in a large canvas bag. It was a white powder at room temperature that you certainly wouldn't put in a hydraulic system. If you did, one would have to thaw out all the lines and other elements with a blowtorch![327]

Since this was impractical, Kelly turned to an academic resource: the Penn State Department of Petroleum Chemistry, who then developed a petroleum-based oil which was liquid at all temperatures. Price was still an issue. The oil was $130/gallon and was kept in inefficient gallon containers instead of barrels to avoid costly spills.[328]

---

[326] Ben R Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane," Speech (Lockheed Advanced Development Co., October 23, 1990), 5, Box 2(6), Ben R. Rich Papers.; C. L. Johnson, "Development of the Lockheed SR-71 Blackbird," *Lockheed Horizons*, no. 9-Winter 1981/82 (1981).
[327] Johnson, "Development of the Lockheed SR-71 Blackbird," 17; Rich and Janos, *Skunk Works*, 206; Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane," 7.
[328] Johnson, "Development of the Lockheed SR-71 Blackbird"; Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane."

Another issue was the jet fuel. The heat of the aircraft in supersonic flight was so great that a fuel was needed that had a very high ignition point, and a low vapor pressure since it was also used as coolant. In refueling the fuel would be -60 degrees, and as a coolant near 800 degrees. Pratt & Whitney and Lockheed worked very closely on the development of the fuel as part of the J58 engine project. Pratt & Whitney's engineering manager William Brown had also worked with Kelly Johnson on the U2 several years before and understood the need for innovative thinking when dealing with the Skunk Works. They worked so closely that Brown "had difficulty in differentiating between "we" Pratt & Whitney, and "we" Lockheed. That is the kind of program it was." This particular power plant needed to transition from a traditional jet engine through low supersonic speeds, to a highly efficient ramjet between Mach 1 and 2 creating more than 160,000 horsepower. The challenges to development were enormous and finding a fuel that could serve many purposes was essential.[329]

Brown led a research team from Ashland, Shell, and Monsanto to research and develop the fuel needed. He later credited the success of the of engine development, fuel formulation, and lubricant engineering, which cost the government in excess of $600 million, to the lack of barriers to communication within the research group. He wrote:

> That this complex, difficult program was successful is attributable, in large part, to the management philosophy adopted by the Government people in charge. Their approach was that both the engine and airframe contractors must be free to take the actions which in their judgment were required to solve the problems. The Government

---

[329] Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane," 8; William H. Brown, "J58/SR-71 Propulsion Integration, or The Great Adventure in the Technical Unknown," Internal White Paper (Pratt & Whitney Aircraft Group, n.d.), 1–6, Box 2(6), Ben R. Rich Papers.; Johnson, "Development of the Lockheed SR-71 Blackbird," 6.

management of the program was handled by no more than a dozen highly qualified and capable individuals who were oriented toward understanding the problems and approaches to solutions, rather than toward substituting their judgment for that of the contractors. Requirements for Government approval as a prerequisite to action were minimal and were limited to those changes involving significant cost or operational impact. As a result, reactions to problems were exceptionally quick.[330]

This reflected Johnson's position on TPP and the restrictions on IR&D. For Johnson, the speed of the SR-71 development was testimony to the success of prototyping and contracting as a series of interactions, not trying to forecast cost, complexity, and troubleshooting at the very start.[331]

Innovation in an industrial location also informed further research within academia. The operation of the SR-71 at very high altitude and at Mach 3 gave insight into both the dynamics of fluid gasses, and the atmosphere itself. Prior to the SR-71 test flights, the sub-tropical jet stream was assumed to exist only around 35,000 feet. However, at the very top of the tropopause, around 70,000-80,000 feet, there was another jet stream with similar velocities and turbulence. These discoveries established a rich vein of research in university meteorology departments.[332]

Like the CL-400, he bulk of research conducted to ensure the SR-71 could fly was in materials science, specifically of the properties of titanium. The choices for the airframe material of the SR-71 were limited to stainless steel and titanium due to the significant heat faced at Mach 3. Titanium, with a

---

[330] Brown, "J58/SR-71 Propulsion Integration, or The Great Adventure in the Technical Unknown," 3, 5–6; Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane," 6–7; Rich and Janos, *Skunk Works*, 205–7; Johnson, "Development of the Lockheed SR-71 Blackbird," 12–13, 17.
[331] Johnson, "C. L. Johnson to J. S. Foster," December 10, 1968, 5.
[332] "Ben R. Rich Speech Synopsis - Supersonic Aircraft," *Interavia* 7, no. 1980 (1980): 2.

much lower specific gravity than steel, but a much higher melting point, was the choice. Unfortunately, titanium is very hard and the technology to machine, weld, and fabricate it was only at very rudimentary levels when the bid for the SR-71 was accepted by the military in 1959. The difficulty in manipulating it also meant that there was not a lot of metallurgy knowledge about titanium in the West (although the USSR had more experience with it), so troubleshooting by the research department was consistently necessary. For example, after the cockpit was prototyped, it was placed in an oven to replicate the expected temperatures of flight. Then all the titanium bolt heads fell off. This issue happened consistently and the research team identified the problem as a chemical reaction with cadmium – which was plated to the mechanic's wrenches to keep them shiny. Cadmium-coated tools were then banned from the Skunk Works factory floor.[333]

Another example of troubleshooting development issues in collaboration with researchers was that the titanium sheets for the skin wore out very rapidly when produced in the summer, but seemed to last indefinitely when made in the winter. This one stumped the Skunk Works research department for two years. Ultimately the research department identified the issue as a reaction to chlorine. The city of Burbank chlorinated its water in the summer but not in the winter. Lockheed then moved to cleaning with distilled water only and another problem was solved.[334]

There are also examples of scientific dead-ends in the SR-71 development. Titanium, while harder than other metals, was also brittle (it could shatter if you dropped it), more flexible, and expanded and contracted more with heat. The aircraft held 75,000 pounds of fuel, and would flex when full, then change shape during flight as the fuel tanks emptied. It would expand so much that the fuel

[333] Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane."
[334] Johnson, "Development of the Lockheed SR-71 Blackbird"; Rich and Janos, *Skunk Works*.

tanks that were water-tight on the ground would break in flight and cause catastrophic failure. Enormous amounts of IR&D time and money were spent attempting to resolve this problem but the Skunk Works never found and elegant solution. The fuel tanks were ultimately designed so they leaked on the ground, and only sealed when the fuselage was hot and the metal had expanded.[335]

These were only a sampling of the issues and innovations which occurred during the SR-71 development. Each problem, without a solution, would have ended the project. Many solutions simply did not work, so as progress was made toward a functional aircraft, the map looked less like the Linear Model, and more like a web of communication of knowledge, data, innovation, and luck between stakeholders at individual, departmental, and organizational scales. The issues and solutions involved in researching titanium and immediately applying that research to building an airplane created a strong feedback loop between research and development. This strength meant that from the date of bid acceptance to the delivery of the most advanced airplane ever built was an amazingly short four years.

Conclusion

Paul Forman's contention that the Linear Model is generally recognized as wrong may by accurate in academia, but it is not universal beyond that. In 2004, Thomas Killion, the head of the Army's Science and Technology division commissioned another study so similar to Hindsight, that it was called 'Project Hindsight Revisited.' The authors sought to explore innovation again using the Critical Technological Event (CTE) model from the original paper. Killion's goal was to have Hindsight Revisited

[335] Johnson, "Development of the Lockheed SR-71 Blackbird," 15–16; Rich, "The SR-71 Blackbird: Paving the Way for the National Aerospace-Plane," 6; Rich and Janos, *Skunk Works*, 213.

help guide the Science and Technology Divisions expenses. They studied four weapons systems: The Abrams main battle tank, the Apache attack Helicopter, the Javelin Missile, and the Stinger Missile.[336]

The newer research assumed, as Hindsight had, that CTE's were all of equal value, that there was a 20-year retroactive cutoff, and most importantly, that the process relied on the Linear Model, as can be seen in the schematic of the Stinger missile research map (fig. 4):

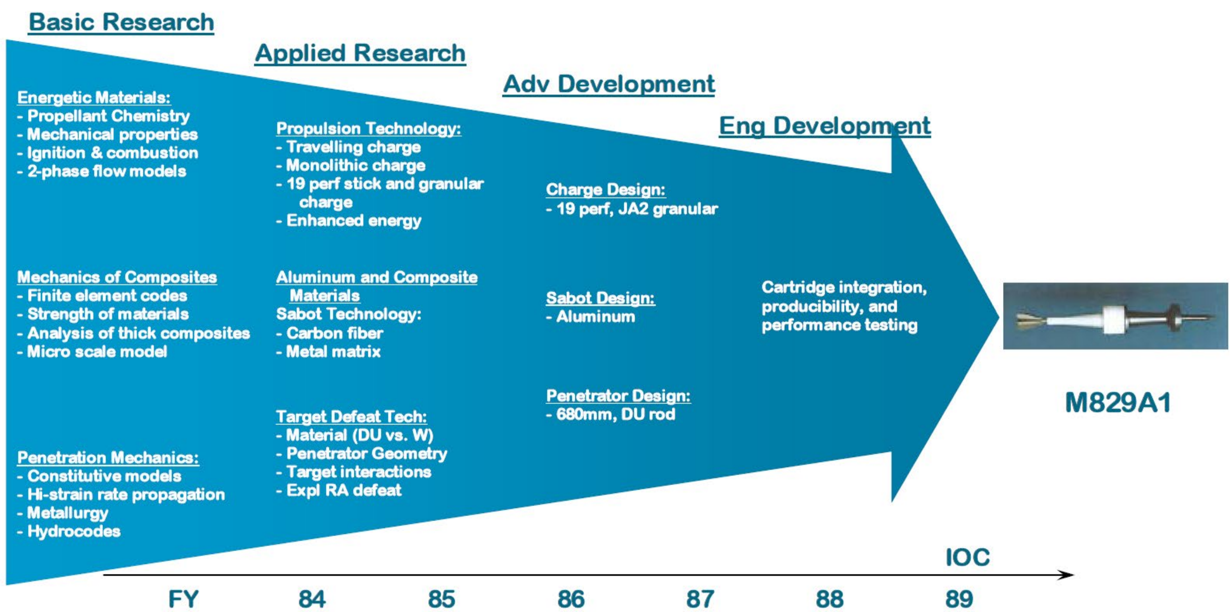


Figure 4 - Linear Model Outline of the Stinger Missile Development from Hindsight Revisited

---

336 John W. Lyons et al., *Critical Technology Events in the Development of Selected Army Weapons Systems : A Summary of Project Hindsight Revisited*, iv, 29 pages : digital, PDF file. vols., [Defense & Technology Papers ; No. 35] ([Washington, D.C.]: Center for Technology and National Security Policy, National Defense University, 2006).

These choices imply that military policy-makers fundamentally saw Hindsight as a success worthy of replication. The report was not successful in accurately describing the greater scientific enterprise, but instead in endowing military and industry research with greater prestige by which the authors justified moving resources away from undirected research toward directed and applied research. It is no great surprise, therefore, that Hindsight Revisited's results also closely matched those of Hindsight.

The authors of Hindsight Revisited recognized that they were relying on a system that had already garnered tremendous negative attention through its systemic bias against academia. To combat this, they included an entire chapter entitled 'Comparison of Project Hindsight Revisited and the Original Project Hindsight' in which the authors sought to distance themselves from the issues surrounding Hindsight. They focused on their recognition of three key characteristics of modern science that they recognized, but ignored in order to use the original Hindsight methodology.[337]

The first of was the importance of the foundational, undirected research. The original Hindsight had been flayed for diminishing the importance of undirected research, which Hindsight Revisited recognized as highly important. The authors wrote that the original Hindsight "concluded that the most useful role of science (as opposed to technology) was to explain the basis of the phenomena being studied and used by the engineers, and that the engineers rely heavily on compiled scientific data in handbooks and texts. Hindsight Revisited found that work done in basic research does not just provide

---

[337] Richard Chait, John W. Lyons, and Duncan Long, *Enhancing Army S & T : Lessons from Project Hindsight Revisited*, xix, 151 pages : digital, PDF file vols. (Washington, D.C.: Center for Technology and National Security Policy, National Defense University, 2007).

explanations for phenomena but directly supplies the foundations for the systems of interest. "This identified and attempted to re-elevate academia as a coequal member of the Triple-Helix.[338]

The second characteristics of modern science Hindsight Revisited described, but did not use, was the importance of networks of communication between actors within a Triple-Helix. They saw that in addition to electronic communication systems (which had been unavailable in 1967), modern science relied heavily on innovation networks for output circulation. These methods included "informal visits to other laboratories, exchange of staff for extended periods of time, permanent move of staff to another participant's operations, participation in integrated product or process teams, seminars, and technical meetings." Additionally, the interactions between actors in the Triple-Helix were essential. Describing this, they wrote "Close working relationships among the participants were characteristic of the development of all four systems in our studies. It is hard to imagine success without such teaming and close ties." These observations were in 2006, more than forty years after Lockheed had used then in designing the research systems in the Lockheed Missiles and Space Company (see Chapters 3 and 4).[339]

The final disagreement between Hindsight Revisited and Hindsight was the seemingly incongruous acceptance of non-linear innovation in Hindsight revisited. Even though they researched under the Linear Model, the authors knew it was not accurate. They wrote that the model they used for "..research and development rarely proceeds linearly and simply. More often than not there are many

[338] Lyons et al., *Critical Technology Events in the Development of Selected Army Weapons Systems : A Summary of Project Hindsight Revisited*, 11.
[339] Lyons et al., 10–11; Chait, Lyons, and Long, *Enhancing Army S & T : Lessons from Project Hindsight Revisited*, 27.

dead ends, recursive loops, and new starts." This is a clear description of modern science, and one which not only describes the early aerospace industry, but most of large-scale postwar science.[340]

It also describes scientific environments where the differentiations between physical locations and the directed/undirected nature of research became meaningless. This is ultimately the meaning of non-linear innovation. It is not at innovation does not occur in the order the Linear Model proposes, but that is occurs everywhere in science throughout a given Triple-Helix regime.

[340] Lyons et al., *Critical Technology Events in the Development of Selected Army Weapons Systems : A Summary of Project Hindsight Revisited*, 3.

Chapter 7

Conclusion

This chapter summarizes the dissertation and reviews the changes to norms of practice within science, and recaps how modern, Triple-helix norms were implemented in the early aerospace industry. It goes on to examine the outcomes of the research, and opportunities for further work.

The goal of this dissertation was to demonstrate through a study of the early aerospace industry that “modern” scientific practice gained primacy within the overall American scientific enterprise in the 1950’s and 1960’s. Another way to phrase this thesis is that modernist norms of scientific practice gained preeminence decades earlier than currently described in the literature of the history of science. Of course, this statement requires explication of how the norms of science changed in the 20th century, and what characteristics “modern” science exhibits to differentiate it from the preceding, enlightenment-based scientific practice.

While change usually comes slowly to intellectual and behavioral norms in science, moments of substantial normative change have occurred in response to paradigm shifts within science, and to major social events. These norms fall into two categories: norms of practice and norms of management (with some crossover between them). It is important to note that none of the norms in these sets are static, but shift into and out of eminence, evolve, and on rare occasions, disappear.

The rise of a set of norms used specifically for the management of science occurred in response to World War II. This war brought with it Big Science as a permanent part of the research landscape, and a system of managing science and scientists developed in response. Moreover, the traditional boundaries of pure science, applied science, and technology became irrelevant in modern science when all three were practiced in parallel instead of serially. This change meant that the management of science also modernized to cope with individual praxis. Since WWII, the vast majority of scientific research has been performed in directed environments using a set of modern management norms. The management of science in the early aerospace industry, is a case in point. It exemplified how the intellectual norms of the enlightenment, transformed into a modernist ethos of scientific practice, and informed the broader norms of modern science management.

These modern management norms were identified in Loet Leydesdorff and Henry Etzkowitz's Triple-Helix model. The model says that modern science is a community effort comprised of three actors: the government, academia, and industry. In any given project, or regime, the progress from ideation to objective is a web of interactions between these three actors. The authors chose the name "Triple-Helix" with the goal of conjuring an image of the double helix of DNA; twisting, interacting, and internally linking. The theory states that these complex interactions where scientific knowledge dynamically moves back and forth along the Triple-Helix and between actors are governed by four scientific norms of management: human resource circulation, the establishment of innovation networks, reflexive output circulation, and non-linear innovation.

Changes in Norms of Practice from 1930-1950

Since scientific revolution of the 17th century, praxis-based norms of the enlightenment have changed to modernist norms. This happened relatively rapidly in response to World War I, and manifested in the American scientific endeavor during 1930's. Up until this point, the norms of practice in science had remained relatively stagnant which we can see by examining Mertonian norms.

In 1942, Columbia sociologist Robert K. Merton summarized the universal norms of both scientists, and the scientific enterprise in his essay "The Normative Structure of Science." His essay was written during WWII but ignored the changes of the 1930's. Merton's description of the norms of science were persuasive but written under the presupposition that these norms were static. Moreover, he referred to these ethos as core to "modern" science in respect to the modern historical era, but modernist science they were not. His essay, like the Linear Model, is still used as a description of the scientific endeavor due to its brevity, simplicity, and idealism; sadly, not for its accuracy. The Mertonian norms were communism, universalism, disinterestedness, and organized skepticism. Only organized skepticism remains a strong institution in modern scientific praxis.

Merton's concept of communism was the idea that all scientists believed that they should have access to, and ownership of, all scientific knowledge. The concept is in direct conflict with both secrecy, and capitalistic ownership of knowledge through patents and intellectual property. In the cold war environment following WWII, this ideal inverted and secrecy became the norm. The impulse to communicate described by the Triple-Helix as output circulation existed as a pathway to innovation rather than the Mertonian norm of common ownership of truth. Additionally, the concept of common ownership was anathema to capitalism. Capitalistic ownership and financial exploitation of proprietary knowledge was core to the establishment of scientific industries and entrepreneurial universities, and supported the development of secrecy as a norm.

Universalism was described by Merton as a characteristic whereby scientific knowledge was equated with objective truth: "consonant with observation and with previously confirmed knowledge."[341] Science was universal because truth was not, in the ontology of science, subject to bias.

Universalism was closely related to disinterestedness. This norm spoke to the motivations of scientists who practice for the discovery of knowledge, regardless of what those discoveries may be.

---

[341] Merton, *The Sociology of Science : Theoretical and Empirical Investigations*, 271.

While universalism spoke to the outcomes of scientific research, disinterestedness was the ethical core of the researcher. This value, according to Merton led to a veracity and reliability of moral action found nowhere else. As we saw in Chapter 1, both universalism and disinterestedness lost eminence to indeterminacy and social praxis respectively.

The final Mertonian norm of organized skepticism required scientific knowledge to be scrutinized and rigorously evaluated by peers before it is accepted into the scientific canon of truth. While this norm retains eminence in both peer review and the attainment of academic degrees, the other three of Merton's norms no longer enjoy primacy and were supplanted in the late 19th and early 20th centuries by mathematization of science, abstraction, the primacy of application, the decline of neutrality, and the practices of large-scale science.

This is not to say that Merton was wrong. Indeed, as a snapshot in time, Mertonian norms described the day-to-day mindset of most university-based scientists prior to WWII. Merton's norms were closely allied to Francis Bacon's scientific philosophy (empiricism and observation, causality, universality, and improvement of mankind's state) and no wonder because Merton's research had been predominantly informed by the enlightenment scientific practices of the 19th and early 20th centuries. However, Merton did not identify that Bacon's causality and altruism had been supplanted by communism and disinterestedness thus demonstrating that scientific norms were dynamic and capable of evolving, albeit slowly. Even when they were published in 1942, Mertonian norms no longer described the modern scientific endeavor. What we have seen, rather, is that the intellectual and practical systems of science showed a sea change in the 1930's, but the normative practice of the majority of scientist practiced modern science in the 1950's and 1960's in both individually applied norms, and in norms of management.

Paul Forman's perspective was that the scientific enterprise was finally modernist circa 1980 when applied science gained primacy over undirected science. Forman's use of the term 'primacy' was a thoughtful and instructive one. It described a phase after which the majority of scientists conducted research in a materially different way. In Forman's case, he used the term to imply that application (or technology) was finally seen by a majority of researchers and other stakeholders as the main societal purpose for scientific discovery. Moreover, Forman suggested that the majority of scientists, across all academic environments, accepted that their research needed purpose for legitimacy. However, Forman was examining only university-based science; a single, small, sector of a broader scientific environment. The vast bulk of scientific research since WWII was conducted in industry and industry researchers had been using modern norms decades before Forman's suggestion.[342]

The primacy of application was certainly a part of modern science, but so too were changes in the individual-level norms of observation, the use of mathematics and abstraction, and the losses of causality and neutrality as scientific imperatives. The modernization of science occurred as a broad cultural shift away from enlightenment-based scientific norms and toward a scientific system of discovery that was larger in scale, and more closely aligned with the needs of stakeholders outside of the scientific community. As such, science required new norms to manage the system which were

---

[342] Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," March 1, 2007.
It is important to note that Forman defines eras in the language of historical eras. Modernity, for Forman, began with the enlightenment and within science, continued through the early 1980's when it became post-modernity. My definitions have been based on intellectual modernism rather than the broader modern historical era. For the purposes of comparison, Forman described the characteristics post-modernism in the same way I describe modernist science i.e. when basic science, applied science, and technology conflate to prioritize outcomes. He wrote that "modernity is when 'science' denotes technology too; postmodernity, when 'technology' denotes science too." (p.17).
Unfortunately Forman's timeframe is late because his study is a meta-analysis of both the history and philosophy of science and technology. At no point did he examine primary sources to see what norms scientists were actually applying. If he had, he would have seen the majority of his "post-modern" science practiced in the 1950's and 1960's.

described by the Triple-Helix model as a focus on human resources circulation, the development of innovation networks, recursive output circulation, and non-linear innovation.

Human Resources Circulation

The focus on human resources within science had its genesis in the growth of large-scale scientific projects during the interwar period and during WWII. The Manhattan Project, which involved more than 130,000 people on a single project demonstrated the possible scale of Big Science. Unfortunately, it also demonstrated the dearth of qualified researchers. This placed the stakeholders of the Triple-Helix in direct competition with each other for researchers and managers.

At the macro level, there was a surprising amount of assistance between stakeholders. Even when Willis Hawkins was at his most desperate for employees to build the LMSC, he still shared resumes with the military, and relied on other similar HR facilitators to assist him. Similarly, instead of recruiting from academia and weakening their source of young scientists, aerospace firms built partnerships with universities where industry supplies resourced, and universities supplied loans of researchers. Industry personnel also taught in the universities. There were also regular loans between industry and the military. Hawkins himself spent three years in the Department of the Army.

At the meso level, raiding of other departments within a company was common, but again so were loans of personnel. Unlike at the macro level, these loans were necessary to maintain employment. With industrial funding based on project work, loans were often the only way to keep employees engaged after one project ended and before another had begun.

For individual researchers, the military, academia, and industry all offered discrete pros and cons, each developed strategy to emphasize their features and obscure their flaws to prospective employees. Industry paid more, but limited self-direction. Academia was prestigious and offered undirected research but also had limited resources. The government offered national service (which, it must be remembered, was a strong attractor during the cold war) and job security, but relatively low compensation. However, by developing a balance of sharing between Triple-Helix stakeholders, individual scientists were able to choose the best personal fit, and still have the opportunity to work and collaborate across the entire Triple-Helix.

Innovation Networks Development

The concept of an innovation network was both a physical and a social space to support and encourage innovation. The Triple-Helix model breaks this characteristic management norm down further, describing the components: an innovation space, a knowledge space, and a consensus space. In this particular characteristic of science management, industry was the first to make this a norm. Lockheed, and in particular Willis Hawkins, Gene Root, and Kelly Johnson, recognized that innovation could not be mandated at the individual level. Instead, researchers must be placed in environments where innovation was normal. These spaces needed to establish strong communication systems between Triple-Helix stakeholders and other departments. They needed to promote undirected thinking and research with a minimal regard to development, and they needed to ensure there were ample resources budgeted so researchers did not get distracted. These were the innovation space, knowledge space, and consensus space respectively. In essence, a balance was needed to allow undirected micro-level environments to exist in larger, directed, meso-level environment.

Unfortunately, the government, by the early 1960’s, was not comfortable with the idea of undirected research (which they saw as uncontrolled spending) in a development-oriented environment. This was because the Linear Model was the foundational understanding of scientific practice used by policymakers. Changes to the procurement system, and ultimately the mandating of innovation through Total Package Procurement, was established by the government to resist IR&D which was the funding source for innovation networks in industry.

Reflexive Output Circulation

Output circulation, or the movement of scientific information back and forth between Triple-Helix actors is distinct from the Linear Model in that the information can be recursive and not just uni-directional. In the Linear Model, research moves cleanly from one research moment, to the next, ultimately moving from research, to development, to product. As we have seen, however, these artificial constructs did not exist in practical terms. The Triple-Helix model describes a far more complex world (especially in Big Science) where innovation events are not discrete but circulate discovery in multiple directions, across to new stakeholders, between parallel research projects, to dead-ends, are revived from dead-ends, etc. This creates, rather than a neat linear path for discovery, a three-dimensional spider web where information is constantly being tested, discarded, combined, stored, ignored, and ultimately moved forward to production. When viewed from a higher altitude, the Triple-Helix become so dense, the regime blends into a single enterprise called science.

This characteristic requires a robust communication system between actors at all scales. Unfortunately, a seemingly antithetical norm also characterizes modern science – secrecy. Both Merton's norms, and the Triple-Helix require strong communication for success albeit for different reasons.[343] Secrecy came as a necessity for national security, entrepreneurialism, McCarthyism, the Cold War, and the publish-or-perish culture of academia. The military especially used destruction of records, clearance systems, and immense reporting requirements to control output circulation.

[343] Mertonian Universalism vs. innovation networks.

Even so, output circulation existed prolifically between actors at all scales, but it was balanced, managed, and, by necessity, ulterior. At the macro-level output was controlled through industry and academic associations, governmental advisory boards, and military project management offices. At the meso level it was managed through loaning human resources between departments and between Triple-Helix stakeholders, and creating spaces where individual scientists could communicate safely. At the micro-level, researchers simply spoke to each other off the record.

While not included in the Linear Model, output circulation was a necessary part of enlightenment science and Mertonian normative science. The marker of modernity was that this remained a norm in the face of a culture of secrecy.

Non-Linear Innovation

The Linear Model was, and still is, an oversimplification of the scientific endeavor in the United States designed primarily to protect the resources of academia, and to streamline the underpinnings of the scientific establishment for non-expert policymakers. It has been remarkably successful at this and more than 150 years after it was first developed, it is still in use. However, the general consensus among historians of science is that the model is a political artifact, and does not modern scientific praxis where the undirected and directed science are blended and outcome oriented.

It is also important to note that the Linear Model defines not only the stages of science (pure, applied, production, use) but also where the research physically occurs (academia, industry, government/public). The concept of non-linear innovation did not only describe the idea that innovation could occur in any stage between research and use, but all the stages could occur in any of, or any combination of, the Triple-Helix locales. In essence, modern science has no content or application boundaries in R&D within any of the Triple-Helix actors.

The difficulty posed by this characterization of modern science is that funding modern research based on an inappropriate model meant that none of the Triple-Helix stakeholders felt that the others were handling public resources successfully. Industry was performing undirected research, academia was becoming entrepreneurial, and the government was suppressing innovation through efforts to mandate innovation timeframes and resource use.

By the middle of the 1960's the tension was so great between stakeholders, that the government sponsored a research project to once-and-for-all explain how innovation occurred in research. Unfortunately, the project, Project Hindsight (Hindsight), was flawed at its inception and angered almost everyone. It was based on the Linear Model, it limited its scope of study too tightly, and it used a determinism to reverse engineer neat linear pathways of R&D.

The responses from industry, academia, and even the military were swift and searing. Each Triple-Helix actor criticized the flaws of the study and gave examples of how modern science did not fit the Linear Model, nor did it fit the outcomes of Hindsight. In fact, industry, academia, and even the military demonstrated that "pure" research, applied research, production, and use were occurring in parallel within each stakeholders' physical locations. They were not differentiated as the Linear Model prescribes, so in a way Hindsight threw into sharp relief the insufficiencies of the Linear Model, and its own shortcomings. Even so, the government was thrilled at the outcome since it gave policymakers a premise to reduce funding to undirected research with seemingly uncontrolled costs.

Further Outcomes

The overarching result of this research is that modern scientific praxis became the norm decades earlier than is generally recognized. This is partially due to a reference frame error. The majority of research on modern science is on academic university-based science. The mischaracterization of time-frame is also partially because of the persistence of the Linear Model at the policy-making level, both governmentally and in academia. Both of these tropes obscure that the majority of scientific research since WWII was done in industry, and characterized new management norms. These characteristics define modern scientific praxis within Etzkowitz and Leydesdorff's Triple-Helix model.

The Role of the Meso-Scale

This research also enhances the Triple-Helix model by revealing that the four characteristics function at three discrete scales. The macro-scale describes the interactions at the large organizational level. The micro-scale describes the day-to-day interactions and practices of individuals. There is also an intermediate, or meso, scale which can be differentiated from the other scales and represents management practices at a departmental or laboratory level.

By disaggregating the influence of the meso scale in science we can also see the organizational importance of talented managers supporting talented scientists. Kelly Johnson exemplified the importance of flexible yet structured management in his fourteen principles of management. Willis Hawkins was also the architect of the organizational structure of the LMSC in its earliest years. Ben Rich was a charismatic meso-level manager of science, but he followed in Johnson's footsteps as a protégé. Nevertheless, Rich was able to maintain the strong innovation network Johnson had built all the way through to his retirement. All three leaders intuitively recognized that an insulated environment was necessary to maximize innovation. In all cases, they put aside their own micro-level research and engineering careers to step into a role of protecting micro-scale innovation strategies from macro-scale policy makers.

It is clear in the research that excellent meso-level managers were an essential part of each of the Triple-Helix actors. Levering Smith and William Raborn at the Navy's Special Projects Office and their groundbreaking work on Polaris was an example from the military of managers capable of bringing together hundreds of contractors and military scientists to produce the first submarine launched ballistic missile. Frederick Terman, the provost at Stanford, was a pioneer in partnerships with industry, and using his concept of 'steeples of excellence', built a world-class engineering department by focusing tightly on a few fields, and building protective innovation networks within those fields. He was the president of the Institute of Radio Engineers, he pushed to improve faculty compensation, founded the Aeronautics and Astronautics Departments, and the Microwave Laboratory. Terman also made human resources, specifically the hiring of exceptional scientists and managers, his single greatest priority.

Willis Hawkins also was a proponent of finding excellence in human resources, and then placing those researchers in environments tuned to maximize innovation. His battles against Total Package Procurement, the loss of IR&D, and Project Hindsight, were not to protect individual scientists, or even specific projects. Hawkins and many other stellar meso-level managers were protecting their painstakingly-constructed systems of hiring, managing, retaining, and encouraging researchers. Their systems started with enormous effort in hiring stellar employees, then creating communications systems so they could circulate output. They developed physical environments which encouraged blue-sky thought, and protected researchers from the mundane worries of resource management and excessive paperwork. While these systems were idiosyncratic to their organizations and their designers, the all displayed the four modern Triple-Helix management characteristics. A fifth characteristic also emerged, and that was a pivotal role for meso-level managers to protect micro-level research from disruptive macro-level influences, while supporting the goals and outcomes of both policy makers, and individual researchers.

How the Linear Model Persisted

Despite non-linear innovation being a modern scientific norm, the Linear Model was robust and persists in the face of its obvious inability to model science. Illumination on this phenomenon was another outcome of this study. First and foremost, the Linear Model was baked into the entire U.S. scientific endeavor through Vannevar Bush's *Science the Endless Frontier*. Bush understood that undirected science was essential because it encouraged innovation, but he also recognized that without application, undirected science would lack a long-term funding guarantee. As an academic by career and a proponent of Big Science through the necessity of war, he was sensitive to the importance of applicability in funding decisions at the policy level. As such, he adopted the Linear Model to ensure that undirected research would have a stable financial justification outside of the capitalist society at large. He then used his report, and therefore the Linear Model, as the basis for the National Science Foundation and the process of apportioning funding for science.

As a foundational tenet of science policy creation, the model is hard to erase. When you add to that its use as a tool to protect the prestige of academia, its elegance and simplicity, and its use in Project Hindsight, it is easy to see how the model has become an institution.

Publishing

Peer reviewed journal articles have generally been seen as the purview of academic scientists, yet the publication histories of industrial scientists was strong. This developed as both an effort to recruit entry-level researchers out of academia (the opportunity to publish), and for companies to co-opt the prestige that publication brought. This aspect of industrial science was even more unexpected considering the culture of secrecy which had normalized in the 1950's and 1960's.

Capitalism

The foundational economic understanding of the United States is that citizens may own property and exploit that property for personal gain. This ethos is in direct contrast with the Mertonian norm (and persistent ethical trope in science) of communism. Throughout this dissertation, the fundamental understanding of industry was that it owned the knowledge it developed. The government felt that since it paid for the research, it owned the outcomes. While delayed, academia ultimately came around to the industry perspective. Even though Vannevar Bush had recognized this as an issue in the development of the NSA and recommended that industry keep ownership of financially exploitable knowledge, the government did not agree and there were persistent tensions around this topic. The outcomes included an emphasis on secrecy, disagreements over profit-margin and when profits were realized, enforced dissemination of outcomes through legislation, and even wonton destruction of records and tools.

Industry Associations as Linking Organizations

Since the norm of secrecy prevented direct high-level exchanged of topical research information (who is studying what), industry associations became defacto linking organizations to disseminate this information. They did this by creating spaces where sensitive information could be legally, or at least quietly, shared. The spaces examined in this study were advisory board committees, DAG Committees, organization chapter meetings, conferences, and tours. Organizations such as the AIA, and individuals like Willis Hawkins, took it upon themselves to link needs with resources. Generally, this meant ensuring that the right people met the right people.

Loaning Human Resources for Output Circulation

A final theme emerged throughout this study and that was the importance of bi-directional human resources loaning systems. It appeared as an important tool in every characteristic of the Triple-Helix. Actors loaned to each other. It was normal for industrial scientists to teach classes at nearby universities. These same universities loaned academics as consultants to aerospace companies. Sometimes there were joint appointments and industry-endowed chairs. The military borrowed researchers from contractors and had project officers permanently stationed in company plants. Willis Hawkins taught at U.C. Berkeley, been effectively loaned to the Army for three years, and ultimately rose to the president of Lockheed. Even within organizations, people were loaned between meso-level structures. They moved from one plant to another, one project to another, and one division to another.

This system of loaning solved human resources issues stemming from scarcity of qualified personnel and the financial reqquirement to maintain a correctly-sized organization. However, the circulation of human resources was the lynchpin for the success of the modern scientific endeavor. In the face of a culture of secrecy, human resource circulation was the single most important conduit of communication. It allowed for consensus and knowledge spaces for individual researchers to work with other experts in the field and innovate freely. It made face-to-face output circulation possible in spite of the national security imperative.

Further Opportunities

While the goal of this dissertation was to demonstrate that the early aerospace industry was practicing what we can now identify as modern science, there are some clear issues which may concern the reader. The first of these is that the bulk of the research was focused on Lockheed. While there was content from several aerospace research firms, the majority of the research came from the archives of

Willis Hawkins and Ben Rich; both Lockheed executives. While similar levels of research into the archives of Northrop, Douglas, or Boeing would strengthen this work, Lockheed was the leading aerospace contractor in the 1950's and 1960's, and with the regular movement of personnel, and the same systems of procurement from the military, it is fair to assume that all of the major players were performing modern scientific research. This is also supported by the unified concerns voiced by the major manufacturers through the AIA when IR&D was placed at risk through Hindsight and TPP.

Another issue is the relative lack of existing research around this topic. There is a fair-sized body of work around science in the cold-war, and the insufficiencies of the Linear Model. Unfortunately, there is not a lot written around the management of science in industry at the meso, or micro-levels so I was compelled to take an oblique path to determine how this topic is currently viewed historiographically.

A further research opportunity stems from the variable primacy of norms. Again, Merton's norms imply a fixed set. However, these were supplanted over the years, but they don't go away. Disinterestedness and objectivity are no longer practical norms, but they exist as ethical goals among contemporary scientists. Causation, on the other hand, lost any merit with the acceptance of a discrete universe. These cases pose further questions. What makes some norms resilient and others disposable? How do Kuhnian paradigm shifts affect norms across all scientific fields?

There is also opportunity to expand this research by grounding it in a particular social science theory which could be used to analyze the interactions between Triple Helix actors, and that is the Loose Coupling Model. In 1976, organizational theorist Karl Weick countered the idea that elements within organizations were coherent and densely linked with the idea that linkages were variable and existed to one degree or another between all elements in a constellation of organizations. The strength of these linkages ranged from practically non-existent, to effectively adhered. The elements themselves were scalable and could be any individual person, to any group of people bound by an organizing structure. In a university, this could be a lab, research group, department, college, or even a university. The closer the coupling, the more synchronized the elements tend to become.[344]

Weick recognized that while the model was developed for educational environments, Loose Coupling Theory could aid in the analysis of the relationships between any scale of organizational structure, from an individual researcher to a foreign government. The full theory examined the causes of loose coupling, the types of loose coupling (including the relationships between ideas), the direct and indirect effects of loose coupling, and finally the organizational outcomes.

This study looked at the practice of science and science-oriented organizations in the early aerospace industry through a historical and research-oriented lens. It was my intention to find evidence that certain relationships and communication structures existed, but not to analyze these relationships in detail. There is room for further study in this area using Loose Coupling Theory as a grounding.

---

[344] Karl E. Weick, "Educational Organizations as Loosely Coupled Systems," *Administrative Science Quarterly* 21, no. 1 (1976): 1–19; J. Douglas Orton and Karl E. Weick, "Loosely Coupled Systems: A Reconceptualization," *Academy of Management Review*, 1990.