

T.C.
MARMARA UNIVERSITY
INSTITUTE OF SOCIAL SCIENCES
POLITICAL SCIENCE AND INTERNATIONAL RELATIONS

**BASIC RESEARCH IDEOLOGY: THE POSTWAR CONSENSUS
ON FUNDAMENTAL RESEARCH IN THE UNITED STATES
AND THE INVENTION OF NATIONAL INTERESTS
IN ACADEMIC SCIENCE, 1945-1957**

Master Thesis

TAHA ENES KURTULMUŞ

İstanbul, 2020

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MARMARA ÜNİVERSİTESİ
SOSYAL BİLİMLER ENSTİTÜSÜ
SİYASET BİLİMİ VE ULUSLARARASI İLİŞKİLER

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Özet

2. Dünya Savaşı sonrası dönem, bugün kavramsallaştırıldığı şekli ile bilim politikasının doğuşuna tanıklık etmiştir. Bu dönemde gelişen Amerikan bilim politikasının diğer uluslar tarafından bir standart olarak ele alınması, önemini daha da artırmaktadır.

Bu tezde (1) ABD’deki savaş sonrası bilim politikasının şeklinin; üniversite sistemi, hükümet-bilim ilişkileri ve temel bilimin bir ekonomik teorisine dair birbiriyle uyumlu bir grup savdan oluşan ‘temel bilim ideoloji’nin bir neticesi olduğunu ileri sürmekteyim. Bunun doğal neticeleri olarak ayrıca (2) başarılı ilk bilim politikası teşebbüsünün temel sebebinin 2. Dünya Savaşı olduğunu ve (3) üniversite-merkezli bilimde milli menfaatlerin olduğu fikrinin temel bilim ideolojisinin bir sonucu olduğunu savunmaktayım. Temel bilim ideolojisinin ana prensipleri bir savaş sonrası konsensusuna dönüşmüş; operasyonel uzantıları ise siyasi ve akademik grupların rekabetinde münazara ve müzakere edilmiştir. Bunu açıklarken bilim politikasının kendine özgü paradoksal mantığını göstermek üzere ‘bürokratobilimsel alan’ kavramını takdim etmekteyim.

Bunları yerine getirmek üzere temel olarak Bourdieu’nün alan teorisini kullanmakta; alanların sınırlarının nasıl değiştiğine ve bunların sosyal yapıyı nasıl etkilediğine odaklanmaktayım. Tez, bu bakımdan Bourdieucü ilişkiselliğin bilim ve teknoloji çalışmalarına taşınmasının nadir örneklerinden birini teşkil etmektedir.

Anahtar kelimeler: temel bilim, bilim politikası, federal destekli üniversite, Ulusal Bilim Vakfı, Vannevar Bush, Bourdieu, alan teorisi.

Abstract

The post-WWII era represents the genesis of science policy, as we understand it today. Its importance is augmented on account that other nations took the United States as a standard to which they measured their own efforts.

In this thesis, I hypothesize that (1) the shape of postwar science policy in the US was a result of ‘basic research ideology,’ a coherent set of interrelated propositions regarding the university system, government-science relations, and an economic theory of basic science. As corollaries, I also hypothesize that (2) the primary impetus behind the first successful development of a national science policy was WWII and that (3) the notion of national interests in university-based science was a result of basic research ideology. The core tenets of basic research ideology became a postwar consensus, while its operational extensions were debated and negotiated between groups of political and academic rivals. In explaining this, I also introduce the concept of the ‘bureaucratoscience field’ to demonstrate the unique paradoxical logic of science policy.

To carry this out, I principally rely on Bourdieu’s field theory, focusing on the shifting boundaries of fields and how these have changed the social structure. In this regard, the thesis is one of the rare attempts to bring Bourdieusian relationalism to science and technology studies.

Key words: basic research, science policy, federal grant university, National Science Foundation, Vannevar Bush, Bourdieu, field theory.

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To my parents, Hlyya and Nařıt, who gave their utmost effort that I have the time and resources to go above and beyond; to my wife, Emine řeyma, who, as my fianc back then, tolerated me unenthusiastically allocating my time with her to my studies; and to my supervisor Alim Arlı, whose support was invaluable in a period ridden with difficulties and unfortunate events. Some of these did not have to come about, but such is the nature of things political.

It is crucial that I point out that I was never a student of the institution under whose name this thesis is published. That university, which has been a home to me and whose spirit lives on in me, is no more. But life goes on. So does our will to fight.

İstanbul, August 2020

Taha Enes Kurtulmus

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Abbreviations

AAAS: American Association for the Advancement of Science
AEC: Atomic Energy Commission
CIA: Central Intelligence Agency
CIW: Carnegie Institution of Washington
CMR: Committee on Medical Research
DoD: Department of Defense
FBI: Federal Bureau of Investigation
FY: fiscal year
HEW: Department of Health, Education, and Welfare
NDRC: National Defense Research Committee
NIH: National Institutes of Health (National Institute of Health, before 1948)
NACA: National Advisory Committee for Aeronautics
NAS: National Academy of Sciences
NCI: National Cancer Institute
NDEA: National Defense Education Act of 1958
NEIA: National Education Improvement Act of 1963
NRC: National Research Council
NSB: National Science Board
NSF: National Science Foundation
OCR D: Office of the Coordinator of Research and Development
ONR: Office of Naval Research
ORI: Office of Research and Inventions
OSRD: Office of Scientific Research and Development
OSTM: Office of Scientific and Technological Mobilization
PHS: Public Health Service
PSA: Presidential Science Advisor¹
PSAC: President's Science Advisory Committee
ROTC: Reserve Officers' Training Corps

¹ This officially corresponds to the Special Assistant for Science and Technology to the United States President, who also came to be the chair of PSAC (later OST) and the Federal Council for Science and Technology. I use PSA stenographically.

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INTRODUCTION

In the 1945 Congressional hearings on science legislation, James Bryant Conant, president of Harvard University and a core member of the leadership behind the scientific mobilization during World War II, declared boldly: “If the proposals before you become law and Congress appropriates the money, we will see a flowering of scientific work in this country the like of which the world has never seen before.”¹ The legislation sought to designate the federal government as the primary patron of basic science. Having demonstrated the utility of cutting edge science and technology with impressive efficiency during the war, these words reflected the postwar spirit of the scientists at large. The underutilization of the university system was demonstrated when the federal government gave its wartime full support, and with the surrounding economic and military implications of science, the scientific community felt confident in its arguments. Already at an international competitive advantage due to its universities’ organization, US basic science widened the gap after federal support. So much so that “[m]ost of the discussions about higher education and of the changes introduced in it since the end of the Second World War all over the world have been the result of American influence.”² Not only did other nations began reorganizing their scientific infrastructure to assign the responsibility of performing basic science to their university system,³ but also imitated the American model of governmental support for research.⁴

¹ J. Merton England, *A Patron for Pure Science: The National Science Foundation’s Formative Years, 1945-57* (Washington, D.C.: National Science Foundation, 1982), 36.

² Joseph Ben-David and Awraham Zloczower, “Universities and Academic Systems in Modern Societies,” *European Journal of Sociology* 3, no. 1 (1962): 76.

³ Charles M. Vest, *The American Research University from World War II to World Wide Web* (Berkeley, Los Angeles, and London: University of California Press, 2007), 40.

⁴ This new tendency applied to science at large of course, but basic science came to be disproportionately located at universities as industry had significantly more incentive to invest in in-house applied research. It was partially in this vein that federal support was directed to basic science. This does not suggest, however, that foreign government imitating the US model of governmental support categorically limited themselves to basic science.

Basic science or *basic research* can be tentatively defined as scientific research done for the sake of science, the primary objective being the production of knowledge, hence without concern for applicability. That results may or may not be used is incidental. This conception, in contradistinction to applied science, presumes a certain purity—hence the synonym *pure science*. Not having the practical concerns of short-term returns, it is argued to be comparatively more isolated from extra-scientific influences. Despite this conception, basic science enjoyed political recognition in the aftermath of WWII once it was acknowledged as the pool of knowledge, which applied science drew from—hence the synonym *fundamental science*. After this consensus, basic science, and along with it the necessary scientific and technical labor, came to be considered an integral part of the development of industrial society⁵ and has been ingrained in national science policy. It can even be said to have given way to a certain academic doxa.⁶ Of course, even at the time scientists themselves did not need convincing that more funding for science was *desirable*; all they needed was to persuade others that it was nationally *necessary*. This second step, however, proved interesting in its scope and intensity—particularly for a mode of science that was described for being unforeseeable in its results, ambiguous in its applicability, and incalculable in its economic returns. This becomes all the more interesting when traditional government-science relations in the US are considered.

Briefly put, genuine federal involvement in higher education began in 1787 in the form of endowing land to public institutions. The interest became explicit with the Morrill Land Grant Acts of 1862 and 1890, first with land grants and then with additional funds, particularly directed at agriculture and mechanical engineering. The interest in agriculture con-

⁵ “The reservoir of expertise and basic knowledge that universities represent is an inherent component of an advanced industrial society, and the continual expansion of that reservoir through research is indispensable for its future.” Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (New York: Oxford University Press, 1993), 336.

⁶ Former MIT president Vest: “For too long, we in universities have tended to treat our federal funding as a birthright. It is not.” Vest, *The American Research University*, 28.

tinued with the Hatch Act in 1887 and the Smith-Lever Act in 1914, establishing agricultural experiment stations and the Agricultural Extension Service respectively. Later the Reserve Officers' Training Corps (ROTC) program were created for university students during WWI. Despite the length of this involvement, the relationship was never continuous.⁷ Furthermore, the content of the more direct relations, such as the A&M universities established due to the Morrill Land Grant Acts, had to do more with application than science or research per se. The federal government's direct interest in scientific research laid in the more immediate uses underpinning governmental roles, such as demography, meteorology, cartography, and military R&D, in which applied research prevailed. This came to change in the aftermath of WWII—almost immediately in its scope. The conception of science shifted to a sort of “science-as-resource,” as put by Elizabeth Popp Berman, and remained so until it slowly gave way from the late 1960s into the early 1980s to a new conception of “science-as-engine”—an economic engine, that is.⁸ Federal support of science, in this conception, was identified with concerns of (1) national defense, (2) scientific and technological progress, and (3) public health.⁹ That the identification with scientific research came predominantly in the form of pure science, except in the categorical cases of military R&D and the space race, is what makes it counterintuitive. Some 20 years after their success, Vannevar Bush, who led the charge for this new policy formation, remarked: “To persuade the Congress of these pragmatically inclined United States to establish a strong organization to support fundamental research would seem to be one of the minor miracles.”¹⁰

There are many factors that initiated and later gave boost to this process, but most of it fed into the postwar consensus that forms a systematic set of coherent arguments, mediating

⁷ Kerr, *The Uses of the University*, 39-40.

⁸ Elizabeth Popp Berman, *Creating the Market University: How Academic Science Became an Economic Engine* (Princeton and Oxford: Princeton University Press, 2012), 3. In fact, it is possible to read this thesis as a somewhat counterpart to Popp Berman's work, whose work deals with the rise of the latter conception.

⁹ Kerr, *The Uses of the University*, 41.

¹⁰ England, *A Patron for Pure Science*, 109.

most external factors. The core components of this consensus were virtually undisputed, with those very minor voices being effectively ostracized in Congress. Some of its operational ramifications were debated and in time, they became integrated rather than changing the modus operandi foreseen by it. Other components came into being later, such as the necessary relation to scientific and technical labor, and were attached to the consensus, as they had been formulated previously. This consensus that I have called ‘basic research ideology’ is the object of this study, through which I intend to understand the shifting government-science relations and the basic science revolution in national science policy from 1945 to 1957—the postwar era of science policy, of which the ending is marked by the launch of Sputnik I.

All of this has occurred based on perceived *problems* and their *official legitimation*. Accordingly, the parties involved over the struggle in defining these problems becomes an important point of departure in delineating story of basic research ideology. As put by Pierre Bourdieu:

To avoid becoming the object of the problems that you take as your object, you must retrace the history of the emergence of these problems, of their progressive constitution, i.e., of the collective work, oftentimes accomplished through competition and struggle, that proved necessary to make such and such issues to be known and recognized . . . as *legitimate problems*, problems that are avowable, publishable, public, official.¹¹

Scientists formed an important community in propounding basic research ideology and were instrumental in making it the *de facto* science ideology of the federal government. The impetus was WWII. “Scientists came to government because they were needed, and men who are needed are not without means of influence.”¹² Their immediate influence, based on applied research, was successfully translated into one of basic research in times of peace and perpetuated in the form of basic research ideology. However, despite the

¹¹ Pierre Bourdieu and Loïc J. D. Wacquant, *An Invitation to Reflexive Sociology* (Cambridge: Polity, 1992), 238.

¹² Dean Schooler, Jr., *Science, Scientists, and Public Policy* (New York: Free Press, 1971), 46.

merit in their arguments, it must be recognized that they were to benefit from the expected results, whether scientists truly believed in the ideological precepts or whether it was empirically accurate. Accordingly, such a study also requires the analysis of this professional interest—a “theodicy of their own privilege,” as Bourdieu paraphrases Weber.¹³ The same concern applies to politicians and bureaucrats of course, if not more, and their actions too must be observed in relation to their objective positions and how they frame their interests.

In Chapter 1, in order to lay out my methodology and conceptual framework, I present Bourdieu’s field theory, along with some interpretation to further specify its points specifically related to my subject; briefly treat the issue of organizational analysis; and introduce a new conceptual and empirical tool to field theory developed from my historical case.

Chapter 2 is a congestion of the entire thesis, minus the concluding section. Accordingly, readers who are interested but cannot afford the time to digest this study should read this and the final section: “The Postwar Era: A Discussion.”

In Chapter 2, I introduce the necessary background by providing a summary of American higher education from its early colonial origins to World War II, in terms of the development of research and the historical relationship between the federal government and higher education institutions. I end the chapter with a detailed account of the scientific mobilization of WWII.

In Chapter 3, I recapitulate two relevant historical cases of which the comparative analysis reveals the new possibilities for a national science policy in the postwar era, as well as the structural reasons as to why these previous cases did not succeed. I then continue with a detailed evaluation of the most important historical document pertaining to basic research: *Science—The Endless Frontier*. Besides providing us with the ‘position-takings’ of the

¹³ Bourdieu and Wacquant, *Reflexive Sociology*, 132.

WWII scientific leadership, this report also highlights all of the important points of contention that later become relevant.

In Chapter 4, I describe and analyze the political power struggle for science legislation that took place in the government, particularly in Congress, but also extending over most of the scientific community. The chapter ends with the compromise attained between the Administration, Congressional dissidents, and the leading groups of scientists, and the belated establishment of the National Science Foundation.

In Chapter 5, I try to describe the bureaucratoscientific field that came to be shaped as a plurality of federal research agencies due to the delayed establishment of the NSF. This includes National Institutes of Health, the Atomic Energy Commission, the Office of Naval Research, as well as the formative years of the NSF.

In the concluding section, I return to some of the important themes of the postwar era and the contentious issues in light of the changes that took place and put forward my conclusions.

CHAPTER 1: METHODOLOGY AND CONCEPTUAL FRAMEWORK

In this study, I focus on the postwar era of 1945-1957, defined by the end of World War II and the launch of Sputnik I, which marks the beginning of a distinct era. I do, however, make recapitulations to earlier periods as they directly relate to the postwar era, and to some particular minor episodes that provide contrasting examples to certain cases. I also relate the postwar era to the post-Sputnik era both in and at the end of the thesis, but this is much more limited.

To determine empirical scope, besides observational relevance, I made use of how important contemporary actors viewed issues to be and which of these issues actually dominated public discussion. To determine this, I made particular use of the *Science* magazine, one of the oldest peer-reviewed periodicals in the world, which includes, besides opinion sections from scientists, arguably the oldest and most comprehensive science reporting archive in the United States. To ensure that seemingly minor but important incidents or other data were not missed and to locate general trends, I made a historically near-exhaustive review of several thousand pages of science reporting. I built the outline of my narrative on secondary works that provide the broadest and most detailed accounts of the following events, based on archival resources, such as official and unofficial correspondence, interviews, and reports. I validate these and add more content through official reports, Congressional hearings, legislation, science reporting, and personal accounts. I substantiate my results with quantitative data, mostly gathered from federal agencies, but the study is predominantly qualitative on account of its aim to bring causal explanations to the development of US national science policy.

I then intertextually analyzed the entirety of my empirical material once my data collection was complete. Finally, I used this to draw causal relations and explain change through process tracing.

My primary hypothesis is that (1) the shape of postwar US science policy was caused and hence can be best explained by a coherent set of policy recommendations based on a conception of higher education and scientific conduct that I call ‘basic research ideology.’ Through this, postwar science policy effectively became the postwar consensus, or the ‘basic research consensus.’ This ideology has given way to the ‘bureaucratoscience’

field’—a necessarily paradoxical social space, which I argue to be the conceptually most useful tool to account for its contradictory behavior.

A secondary thesis, empirically inseparable from the first, is what I call the ‘war thesis of science policy,’ in a similar fashion to Charles Tilly.¹ Even though not explicitly spelled, science policy has existed long before WWII in more muted forms. Yet science policy, as it became explicit and later imported by other countries from the United States, had originated in the conditions of WWII. Certainly, that form of science policy extends beyond basic research into military R&D. However, it does not change the fact that the political introduction of a genuine science policy was a direct result of WWII. Accordingly, I assert that (2) World War II is the single most important factor in the development of modern science policy.

Another secondary thesis is that (3) the practice of government interest in improving academic science was a result of basic research ideology. This too is inseparable from the first hypothesis. Before WWII, scientific interests of states and governments were primarily carried out by bureaus and intramural laboratories. However, universities were *the* centers of basic research, which did not amount much from a governmental viewpoint. Such involvement was limited to some of the practical arts, of which scientific research (academic science) was not a primary concern.

Michel Crozier and Erhard Friedberg long ago remarked that the analytical problem of social science is not to develop an explanatory model, but to elaborate a research methodology.² Priority given to analytical means allows one to transcend the oft-cited theoretical dualisms with relative ease, or to at least postpone the verdict until after the result. Con-

¹ Charles Tilly, *Coercion, Capital, and European States, AD 990-1990* (Massachusetts and Oxford: Blackwell, 1990).

² Michel Crozier and Erhard Friedberg, *Actors and Systems: the Politics of Collective Action* (Chicago: The University of Chicago Press, 1980), 24.

strued against theoreticism, it is also prudent to take Bourdieu's warnings against methodology—the tendency to elaborate such methodology without reflecting upon empirical material.³ Accordingly, such a methodological framework requires temporary validation and then refinement via falsification through the careful construction of cases by drawing upon the objective properties of the social world, and their uses and perceptions by social actors. Through this, limited generalizations can be drawn, for “[a] particular case that is well constructed ceases to be particular.”⁴

In this thesis, I primarily rely on Bourdieu's relational methodology.⁵ Relationalism posits that the fundamental object of analysis is the relations between conceivable actors, but also between them and *things* that become, through relations, *social things*. Bourdieu's form of relationalism, or more properly his *historical relationalism*, is distinguished from others' in its conception of 'habitus' and 'field' as unique bundles of relations, which provide the analyst with means to *relate relations*.⁶ I also make significant use of organizational theory to draw from and then build upon Bourdieusian relationalism.⁷

³ Bourdieu and Wacquant, *Reflexive Sociology*, 28.

⁴ Ibid, 77.

⁵ Field theory, capital theory, and numerous other designations are used by others to refer to Bourdieu's approach but also sometimes to crop out a third of his conceptual triad. Such a use contradicts with Bourdieu's admonitions on the empirical indivisibility between the three. Accordingly, I use all such designations interchangeably to refer to his complete relational approach.

⁶ I do not intend to brand Bourdieu's approach 'historical relationalism,' but rather categorize it as part of what can be considered a methodological school, distinct from other approaches devoid of relationalism and some relationalist approaches devoid of historicity.

⁷ For an innovative integration of Bourdieusian sociology and the concept of 'organizational field' often used in organizational studies, see: Mustafa Emirbayer and Victoria Johnson, "Bourdieu and Organizational Analysis," *Theory and Society* 37, no. 1 (February, 2008): 1-44.

1.1 Field Theory or Historical Relationalism

The conceptual triad that forms the backbone Bourdieu's analysis comprises the field, the habitus, and capital. The first of these, field, is similar to Weber's 'life-orders.'⁸ A field is the bundle of relations in social space⁹ that operate according to a specific and common logic.¹⁰ In a sense, the field *is* its logic. Of course, fields are not isolated layers and any given interaction can take place in a multitude of fields, and then again in sub-fields. The same applies to actors: "[A]n agent or an institution belongs to a field inasmuch as it produces and suffers effects in it."¹¹ Accordingly, a firm would be in the economic field, but so too would a beggar, a charity, and young aspiring academics trying to make ends meet. It can be expected, however, that a few fields among the many will have a disproportionate impact on actors and that one or two field at a time will dominate a given interaction.¹² It is through the location of actors and interactions in fields that the analyst is able to explain the underlying social mechanisms that shape them and are shaped by them. As a matter of course, a field (and its mechanisms) is not supposed and superimposed on empirical phenomena, but that it is "discovered" and constructed according to them. This is why, according to field theory, the object of social science is fields,¹³ rather than relations per se.

At the risk of sounding reductive, I define habitus as the relational equivalent of the so-called individual. This is correct by definition, however, since field theory aims to register

⁸ Bourdieu and Wacquant, *Reflexive Sociology*, 17. Bourdieu's distinction between structure (through 'fields') and interactions (through actual relations) was partially formulated against Weber. Ibid, 113-114.

⁹ The entirety of relations insofar as they are connected one way or another.

¹⁰ Bourdieu, wary of connotations to theoretical explanatory models of which the dualisms he took great pains to avoid, usually averts using concepts like logic, rationality, intention, interest, and other suggestive terms. Sometimes, however, he uses them after an explanatory qualification. I follow his conceptual rejections and use them in an adjusted manner but do not always state an explicit conceptual criticism beforehand.

¹¹ Ibid, 232.

¹² "[T]he interaction is **the** visible and purely phenomenal resultant of the intersection of hierarchized fields." Emphasis in original. Ibid, 257.

¹³ Ibid, 107.

actors in the social world, whose existence in this world is relational. More specifically, it is “a system of lasting and transposable dispositions which, integrating past experiences, functions at every moment as a matrix of perceptions, appreciations and actions and makes possible the achievement of infinitely diversified tasks.”¹⁴ Accordingly, there is a simultaneous *conditioning* of the habitus by fields and the *cognitive construction* of fields by the habitus.¹⁵ There is a *relative irreversibility* to habitus, which has to do with the priority of structuring due to the chronological order of events,¹⁶ meaning that a habitus, in a state of equilibrium, becomes increasingly difficult to change with time. Like fields, habitus too carry the burden of the past. This similarity has to do with an “ontological correspondence” between habitus and field, since the social world exists in things and in minds. Habitus produced by a social world, to refer to Bourdieu’s analogy, are like fish in the water.¹⁷ The simultaneous existence of social reality in things and minds is why and how agents are able to respond *reasonably* to external stimulants. This is why “without being rational, social agents are *reasonable*—and this is what makes sociology possible.”¹⁸ Yet it is also the habitus that limits what it makes possible. This “practical logic” does not compute data but rather *precognizes*, made possible by the “sedimented situations” in the habitus. This limited logic is why (1) habitus does not produce neat regularity and (2) producing a precise science of an imprecise social reality is difficult.¹⁹ Accordingly, strategy becomes a “feel for the game,” rather than that as conceived by rational choice theorists.²⁰ Bourdieu does not, however, exclude strategic deliberations:

¹⁴ Ibid, 18.

¹⁵ Ibid, 127. As Wacquant puts it, Bourdieu is able to explain the correspondence between mental and social structures through the habitus, something has lacked in the writings of Durkheim and Marcel Mauss. Ibid, 12-13.

¹⁶ Ibid, 133-134. For example, Bourdieu contrasts an individual’s primary class upbringing and the secondary habitus produced through education. Pierre Bourdieu, “The Peculiar History of Scientific Reason,” *Sociological Forum* 6, no. 1 (March, 1991): 9.

¹⁷ Bourdieu and Wacquant, *Reflexive Sociology*, 127-128.

¹⁸ Ibid, 129-130.

¹⁹ Ibid, 22-23.

²⁰ Ibid, 128-129.

The immediate fit between habitus and field is only one modality of action, if the most prevalent one . . . The lines of action suggested by habitus may very well be accompanied by a strategic calculation of costs and benefits, which tends to carry out at a conscious level the operations that habitus carries out in its own way. [For example,] Times of crises, in which the routine adjustment of subjective and objective structures is brutally disrupted, constitute a class of circumstances when indeed “rational choice” may take over, at least among those agents who are in a position to be rational.²¹

That being said, it must also be emphasized that the structure of certain fields might be more, even significantly more calculative than others, meaning that those habitus which have come to internalize these are likelier to be more calculative. Despite not ruling out such an interpretation and even using conceptually suggestive language, Bourdieu himself does not relate this to the habitus. This phenomenon, something which is widely recognized in practice but raises ire when spelled explicitly, becomes often visible in my thesis. While it is my belief that such calculative characteristics can be structured into dispositions beyond those of the specific interactions directly related to the field in question, however, it must be also pointed out that not only is there the lack of a necessary relation between these, but they can, in fact, exist in opposite extremes.

In connection with habitus, I make use of Crozier and Erhard’s reinterpretation of the classical analysis of “role,” which they reconceptualize into “a relatively stable equilibrium state between a dominant majority strategy, on the one hand, and one or more minority strategies on the other.”²² I take this as objectified schemata produced by the positions and position-takings in the field. Not everyone devoid of power tries to change the distribution of power and not everyone powerful tries to conserve it,²³ which I explain through such schemata.

²¹ Ibid, 131.

²² Crozier and Friedberg, *Actors and Systems*, 59.

²³ Bourdieu and Wacquant, *Reflexive Sociology*, 109.

The social world is accumulated history and Bourdieu uses *capital* as an analytical tool of accumulation in lieu of power to explain power. In this framework, capital is power. Historically, it is accumulated labor. The analytical strength of this conception is derived from the ease of framing the accumulation, loss, transfer, and conversion of capital. The three fundamental forms of capital are economic, social, and informational capital.²⁴ Economic capital requires the least explication since it is, for the most part, virtually the same as what is commonly understood by economic means (certainly not limited the literal meaning of ‘capital’). Economic capital can be institutionalized through means such as property rights. Informational, or cultural, capital are acquired epistemic or artistic products; (1) sometimes in an *embodied* state, i.e. enduring dispositions, (2) in an *objectified* state, e.g. a rare cultural good, or (3) an *institutionalized* state, i.e. conferred authority in the form of credentials, such as degrees. Social capital, on the other hand, is the totality of actual and potential resources provided by membership to a stable network. Through this, individuals can mobilize the capital of others, in a sense causing a multiplier effect on their own capital.²⁵

Forms of capital emerge, dissipate, and evolve along with fields.²⁶ To these, however, must be added symbolic capital—“the form that one or another of these species takes when it is grasped through categories of perception that *recognize* its specific logic or, if you prefer, *misrecognize* the arbitrariness of its possession and accumulation.”²⁷ In this

²⁴ Initially using ‘cultural capital,’ Bourdieu later preferred ‘informational capital’ to give its generality. Ibid, 119.

²⁵ This paragraph is mostly summarized from: Pierre Bourdieu, “The Forms of Capital,” in *Handbook of Theory and Research for the Sociology of Education*, ed. Richard Nice (New York: Greenwood, 1986), 241-258.

²⁶ There are not many social scientists who operate within a framework of fixed power or resources today, but there are also few original thinkers such as Bourdieu who provide a framework of such variety not only in the quantity but also the quality of power. Population ecologists can be taken as an example that operate within fixed resources, such as: Michael T. Hannan and John Freeman, “The Population Ecology of Organizations,” *American Journal of Sociology* 82 no. 5 (March, 1977): 941.

²⁷ Bourdieu and Wacquant, *Reflexive Sociology*, 119. Loïc J. D. Wacquant remarks that the entirety of Bourdieu’s writings can be read as a quest in search of the various species of symbolic capital—that is, *the* forms of capital.

sense, symbolic power reflects whether a form of capital is active in a given social world. Such recognition provides the means to partially “manipulate” the value of capital as well. According to Bourdieu, there are three necessary steps for field analysis.²⁸ First, the field must be analyzed in relation to the field of power (i.e. the social space in which the strongest currents of power take place). Second, the structure of the field, i.e. the objective *positions* of actors and the relations between them, must be charted. Objectives, of course, do not denote phenomena independent of social construction but those that have been ingrained in the social structure to the degree of having considerable stability. For example, material factors, such as the relation of machines to labor, or materialized factors, such as the amount of money in a fat bank account. The power and value of these can be affected, but they cannot be simply deconstructed away. Third, the habitus of actors, must be discovered to explain the regularities and possibilities of their *position-takings*. *Positions* denote the places actors are located in within the social space, whereas the actors’ *position-takings* denote their action and reaction to them. Like the steps, these are empirically inseparable from each other.²⁹ The interaction between these classes of phenomena is explained through ‘double objectivity,’ i.e. the objectivity of material resources and rare social goods, and the objectification of personal dispositions, preferences, and strategizing.³⁰ However, even though the analyses of *positions* and *position-takings* are both necessary, analysis of the objective structure of social space has priority, since actors adjust their viewpoint according to their positions.³¹ Bourdieu’s formulation has been criticized to weigh too much toward structuralism and disregard interactions,³² but when one considers positions and position-takings diachronically, the given priority to structuralism

²⁸ Ibid, 104-105.

²⁹ Position-takings are also called ‘stances.’ Ibid, 105.

³⁰ Ibid, 7.

³¹ Ibid, 11, 105.

³² Emirbayer and Johnson, “Bourdieu and Organizational Analysis,” 10.

merely becomes one of chronological order, i.e. *past* position-takings having priority over *future* position-takings.³³

Empirically, determining a field, its boundaries, and which forms of capital are active in it are the same.³⁴ This is due to the circular definition between field and capital.³⁵ In this sense, capital can be construed as a sort of field-effect occurring in different modes (e.g. relations, objects, embodied attributes). This is also why “[a] capital does not exist and function except in relation to field.”³⁶ The autonomy of a field, in turn, is determined by how much it *re-structures*, or refracts, external determinants,³⁷ i.e. other forms of capital. For example, the easier it is to economically or political influence convictions in a legal system, the more wanting the legal field is in its autonomy.

Bourdieu, to avoid the conceptual complications caused by the employment of ‘intention,’ uses ‘interest’ to describe actors’ psychological ‘investment’ in the social world. This is not a rationalist conception of interest, however, which is why Bourdieu sometimes refers to it as *illusio*, to refer not to an anthropological conception of interest in and of itself but a historically condition particular interest.³⁸ This is why

³³ Despite Crozier and Friedberg’s accusation of determinism leveled against Bourdieu, there is methodological similarity between them. The correspondence can be seen in the authors’ systemic argument with Bourdieu’s positions and their strategic argument with Bourdieu’s position-takings. The similarity becomes more limited, of course, when Bourdieu’s conceptual triad is introduced. Crozier and Friedberg’s criticism, from their point of view, then becomes more understandable since these all of these three concepts are relations and embodiments of structuration. Crozier and Friedberg, *Actors and Systems*, 117-121, 305.

³⁴ Bourdieu and Wacquant, *Reflexive Sociology*, 98-99.

³⁵ This does, however, not pose a methodological problem as the explanatory definition of both a particular field and its capital are derived from social phenomena, which is why they are empirically the same.

³⁶ Ibid, 101.

³⁷ Ibid, 105.

³⁸ Ibid, 116. I find Bourdieu’s conceptualization baffling and contradictory in its distinction between disinterestedness and *indifference*, which is the purported opposite of interest. The difference between *illusio* and its lack thereof are investment and indifference. There can be no third categorical alternative to this position-taking as ‘negative investment’ is logically impossible. Accordingly, what Bourdieu means by “disinterestedness” is a mystery.

Far from being an anthropological invariant, interest is a *historical arbitrary*, a historical construction that can be known only through historical analysis, *ex post*, through empirical observation, and not deduced *a priori* from some fictitious—and so evidently ethnocentric conception—of “Man.”³⁹

By investment I mean the propensity to act that is born of the relation between a field and a system of dispositions adjusted to the game it proposes, a sense of the game and of its stakes that implies at once an inclination and an ability to play the game, both of which are socially and historically constituted rather than universally given.⁴⁰

In this sense, there are as many interests as there are fields and even more according to position-takings.⁴¹

Habitus and field, as “*two modes of existence of history*” allow to do away with time and history as realities in themselves.⁴² And along with field, together they do away with the conceptual implications of utility and consciousness by extending the sphere of interest over them, differentiating field theory from rational choice theory and economic determinism.⁴³ Finally, through the field, structures are not sacrificed in the quest for understanding relations, as it is in network theory.⁴⁴

1.2 Organizational Analysis

A relational approach, not exclusively of course, further highlights the problem of reification (and its related implications). Individuals are easily specified due to their anatomical unity but are more difficultly made into a social object. The difficulty arises in the

³⁹ Ibid, 116-117.

⁴⁰ Ibid, 118.

⁴¹ Ibid, 117. I find the inexistence of an extra-social conception of interest and its plurality due to position-takings conceptually contradictory but leave this to another prospective work since it does not have any operational bearings on my analysis.

⁴² Ibid, 138.

⁴³ Ibid, 24-26.

⁴⁴ Ibid, 114.

social embeddedness of individuals, which can be analyzed through the precepts of historical relationalism. However, a sort of entity-ness that is purported upon a bundle of relations comprising separate anatomical unities becomes not only difficult in its agency, but even whether or not it is a social object to begin with.⁴⁵ Accordingly, it becomes important to empirically justify the social existence of collective entities.

Classes constitute a good example of how such groups relationally stand. These do not exist as real groups but as a *space of relationships* that makes the formation of real groups possible, likely, unlikely, etc. Statistical analyses of the sort which are purported to reveal characteristics of classes rather reveal the structure of that space of relationships, effectively explaining the structural possibilities of real group formation. In terms of field theory, this is related to how forms and distributions of capital and homologies of power relations might make groups likelier or more durable. The appearance of such entities revolve around the consensus and coordinated action or inaction of its members. A matter of consequential importance in this regard is the differential capacity to make entities exist. One way this can occur through is by making *common sense* the explicit consensus of a group, which is naturally easier in smaller groups than in larger ones. Regardless, a group exists insofar as its representatives are authorized to speak in its name. This is because only in this way can a group be directly addressed (that is, interacted relationally) by other entities. Examples are common in the thesis, such as scientists who coalesce around a consensus and operate with one or more representatives. On the other hand, the scientific community as it is commonly understood is a *scientific community in representation*—not an actual group. The common characteristics, structural conditions, and distribution of capital make possible the formation of groups between scientists (as well as non-scientists). Accordingly, in the thesis, I try to avoid using nouns in the collective unless a sort of representation is demonstrated or at least suggested. Sometimes I qualify this

⁴⁵ It is, however, quite possible that social non-existence can be empirically conjured into social existence, becoming an object of analysis in turn

by adding the representative adjectives and sometimes just use generic nouns interchangeably with these when justified. It is also possible to speak of the scientific community at large as a group, since they have demonstrated the capacity (at times) to act as a group through professional organizations so numerous and so large to actually have a claim to nationwide representation and be outwardly represented by select members. Nevertheless, such phenomena are a rarity and scientists tend to exist as numerous large and small organized groups. This period in the history of science, however, is also unique in several occurrences of that rare phenomenon.⁴⁶

As Emirbayer and Johnson point it out, organizations are not singular actors, so their position-takings should be understood as negotiations.⁴⁷ Consecutively, this requires a more rigorous empirical analysis since it involves numerous individuals who might be informed by different positions and even *sui generis* characteristics. This is stretched further when position-takings take place in fields whose structure are in relative flux (such as in Congressional politics of which the structure may change drastically with each election). Less attention and detail can be given to such cases where there seems to be more unison and less internal challenges. As will be seen, I have painstakingly tried to follow these precepts in dealing with groups and organizations.

A group's *collective capacity* denotes its capability in integrating the different strategies and divergent interests of its members. A *collective learning experience*, on the other hand, is the learning of new ways to increase its capability for integration.⁴⁸ Besides the personal qualities of its members, informed by their habitus, the forms of capital possessed and the positions occupied also influence a group's collective capacity and, in turn, learning experiences. For example, an actual group by friendly association will radically differ from a group in the form of a professional organization. The latter would be (likely) more

⁴⁶ Most of my approach here is directly taken from Bourdieu, while some parts have been expanded and interpreted by myself. Pierre Bourdieu, "The Social Space and the Genesis of Groups," *Theory and Society* 14, no. 6 (November, 1985): 725-726, 729, 741-742.

⁴⁷ Emirbayer and Johnson, "Bourdieu and Organizational Analysis," 19.

⁴⁸ Crozier and Friedberg, *Actors and Systems*, 106, 108.

durable also due to its legal status, but this does not change that the fact that informal (not extra-formal) groups can also become collective entities. To this effect, I paraphrase Crozier and Friedberg by saying: Organized action, and not organizations, is the proper object of organizational analysis.⁴⁹ Furthermore, once groups are construed empirically, they are in practice indistinguishable from organizations in their analytical qualities. From this, I make much use of organizational theory for both organizations and real groups. The only category of exception to this are those organizational characteristics that come from durability, which might not apply to many groups.

An important point regarding organizations are external relations. Neo-institutionalist studies, underlining the social embeddedness of organizations, focus on the relations between organizations and their environment, which stands in contrast to older institutional studies that focused on organizations internally.⁵⁰ An exception to these were some population ecology studies that were very early to bring in the notion of ‘environment’ by virtue of their continuity-of-nature hypothesis.⁵¹ However, population ecological perspectives, besides being radically different in their sociological axioms and overall framework, include elements irreconcilable with later organizational studies. Regardless, approaches that focus on a relationally focused scope (external, intra-organizational, or inter-organizational) become very problematic when one considers different cases in which any one can dominate and others can nevertheless have significance. Accordingly, Crozier and Friedberg distinguish between *environment* and *relevant environment* to earmark the empirical value of external relations and couple them with intra-organizational relations.⁵² Field theory is similar content-wise. However, field theory demolishes a conceptual ‘en-

⁴⁹ Ibid, 54.

⁵⁰ Otto Hüther and Georg Krücken, *Higher Education in Germany—Recent Developments in an International Perspective* (Cham: Springer, 2018), 142.

⁵¹ Hannan and Freeman, “The Population Ecology of Organizations,” 933-936, 962.

⁵² Crozier and Friedberg, *Actors and Systems*, 81, 291.

vironment' to begin with. Social space *is* the environment. An actor or organization's position in the social structure reveals *its environment*, while fields explain what segments of that environment will become the *relevant environment*. Finally, fields and the distribution of capital then explain how an entity interacts with its purported environment.

Emirbayer and Johnson went beyond this by conceptualizing the 'organizational field' à la Bourdieu. This term actually had wide usage before this but was conceived as a more generic term independently from field theory.⁵³ According to the authors, the concept

has appealed to many organizational theorists because it offers at least one distinct advantage over other widely used environment-level concepts such as that of organizational population. Unlike the latter, an organizational field includes not just one type of organization . . . but all the organizations that play one role or another in the activity in question.⁵⁴

Different from earlier uses, this conception of the organizational field, as I have stated before, has explanatory value for how an organization interacts with other, hence for power relations. The utility of 'field' is diminished in such studies due to (1) being limited to the organizational level and (2) the lack of a complementary relational framework, i.e. the lack of habitus and capital. Furthermore, the concept's relation to conflict has been significantly deemphasized in neo-institutionalist studies where it has prospered.⁵⁵

⁵³ DiMaggio and Powell were important in spreading its use. "By organizational field, we mean those organizations that, in the aggregate, constitute a recognized area of institutional life: key suppliers, resource and product consumers, regulatory agencies, and other organizations that produce similar services or products. The virtue of this unit of analysis is that it directs our attention not simply to competing firms . . . but to the totality of relevant actors." Paul K. DiMaggio and Walter W. Powell, "The Iron Cage Revisited: Institutional Isomorphism and Collective Rationality in Organizational Fields," *American Sociological Review* 48, no. 2 (April, 1983): 148.

⁵⁴ Emirbayer and Johnson, "Bourdieu and Organizational Analysis," 2.

⁵⁵ Ibid, 3, 6. While agreeing with all these points, I find Emirbayer and Johnson's vertical application of 'field' to organizations, i.e. organizations *as* fields, with their own specific logic, unwarranted and in conflict with field theory (this is a separate discussion from the abovementioned). The authors mention that organizations refract external influences stimuli internally, just like fields do. This is correct, but has no necessary relation with the validity of the 'organizational field.' This exact situation applies to real groups as conceived by Bourdieu as well—this does not mean that these have a field-like logic as well. The authors note that Bourdieu did not write much on organizations per se, partially due to which they developed the concept. Accordingly, I believe

1.3 The Bureaucratoscientific Field

In attempting to account for the cases my thesis represents, I have arrived at the conclusion of a distinct social space: the bureaucratoscientific field. Before describing it and its theoretical corollaries, I will briefly touch upon its composite fields: the scientific and the bureaucratic.

The scientific field emerged from within the field of cultural reproduction and attained autonomy. To simplify, a distinguishing feature of this field is that “researchers agree on the grounds of disagreement and on the instruments with which to resolve these disagreements and on nothing else.”⁵⁶ A specifically scientific capital has two components: *capital of strictly scientific authority* and *capital of social authority*. While both of these emanate from the demonstration of competence, the first has a direct relation and the latter is a sort of embodiment already-possessed scientific capital (and its presumption). Accordingly, a distinction of reason is required to tell strictly scientific authority from social authority.⁵⁷ This is the simple difference, for example, of how a layperson and a doctor would recognize the competence of another doctor. However, despite the seeming similarity between the *capital of social authority* and *cultural capital*, their difference must be emphasized in the relation these two have to the social world and the types of practical authority they confer on their possessors.⁵⁸

that the authors have committed this mistake due to focusing on the lack of writings on organizations and then making a connection of organizations’ seemingly unique field-like properties to fields. If they had also incorporated Bourdieu’s methodology regarding empirical groups, they might have concluded that this can apply to all collective entities. Their references do not include Bourdieu’s papers that focus on the genesis of social groups, further suggesting that they overlooked to connect organizations to group formation.

⁵⁶ Bourdieu and Wacquant, *Reflexive Sociology*, 176.

⁵⁷ Bourdieu, “The Peculiar History of Scientific Reason,” 7-8.

⁵⁸ Bourdieu holds that statutory authority becomes stronger when the autonomy of the scientific fields is reduced. Even though this sounds intuitive, I believe that the opposite is true for many fields more often, and perhaps for all fields for most people. The increasing levels of specialization

Naturally, overcoming opponents in an autonomous scientific field through non-scientific fields excludes one from the scientific field. Similarly, the administration of science is also not of the scientific field.⁵⁹ The distinction between this and scientific is that

Academic power refers to the power of control over the administration of academic resources and the means of career influence. It is the power to preside over credentials and allocate status and as such it is a socially codified power. Scientific power is in essence the power that comes from research reputations based on scholarly publications. It is a matter of prestige directly from knowledge as opposed to the status of honorific position.⁶⁰

The distinction between the two become clearer when considering the fact that universities became the centers for scientific enquiry long after modern science had been flourishing in the works of independent societies, private laboratories, and self-contained research institutes (briefly treated in the next chapter). Once they did become its center, however, there has occurred, in a sense, a near alignment of the scientific field and the university system. What Bourdieu calls the ‘university field,’ I refer to with the oft-used term of the ‘organizational field of higher education.’

The bureaucratic field, on the other hand, comes into play as the explanatory alternative to the ‘state,’ which “makes sense only as a convenient stenographic label—but, for that matter, a very dangerous one.”⁶¹ Accordingly, the stenographic use must be qualified by its empirically dispersed structure (even though socially hierarchized). Bourdieu depicts it in the following manner:

In fact, what we encounter, concretely, is an ensemble of administrative or bureaucratic fields (they often take the empirical form of commissions, bureaus and boards) within which agents and categories of agents, governmental and nongovernmental, struggle over

(and then even cross-specialization) means that scientific competence becomes less and less of a possible option to interpret the competence of someone.

⁵⁹ Bourdieu and Wacquant, *Reflexive Sociology*, 177-178.

⁶⁰ Gerard Delanty, *Challenging Knowledge: The University in the Knowledge Society* (Buckingham: SRHE and Open University Press, 2001), 93.

⁶¹ Bourdieu and Wacquant, *Reflexive Sociology*, 112.

this peculiar form of authority consisting of the power to *rule* via legislation, regulations, administrative measures (subsidies, authorizations, restrictions, etc.), in short, everything that we normally put under the rubric of state policy as a particular sphere of practices related.⁶²

The bureaucratic field is unique in its effective consecration of “universal” social problems.⁶³ As it will be seen, this comes to play a decisive role in promoting basic research ideology into a postwar consensus. *Experts* are particularly involved in the production of official problems, which comes to be occupied by scientists and engineers for the most part in the cases involved here.⁶⁴ Policy, then, becomes an important overall guide covering a set of related official problems. However, a policy need not be clearly stated or even planned beforehand.⁶⁵ In a sense, this was and was not the case for basic research ideology. It was pushed to become the national science policy by scientists against the intentions of the executive branch, and once it had been, it was later pushed by the executive branch to become codified, which was, for the most part, then resisted by the scientists.

The field of power becomes most relevant in the context of the bureaucratic field. A sort of “meta-field,” the field of power is the social space where actors dominant in their respective fields clash with each other over which form of capital would have more legitimacy, or, in other words, the monopoly of power. The particular symbolic power possessed by the state, however, makes it the case that the field of power is often a struggle of power over the state.⁶⁶ The sort of ‘meta-capital’ associated with the field of power and the state is due to its concentration of other species of capital, which was the historical

⁶² Ibid, 111.

⁶³ Ibid, 239-240.

⁶⁴ Bourdieu’s mention of experts here was specifically meant for sociologists (or other social policy experts), but it applies to all forms of expertise.

⁶⁵ Schooler, Jr., *Science, Scientists, and Public Policy*, 30.

⁶⁶ Bourdieu and Wacquant, *Reflexive Sociology*, 18, 76, 114-115, 229-230.

result of the concentration of juridical capital (following a somewhat similar line of development of Max Weber's account of the rationalization of government through princes).⁶⁷ This is a fundamental point in the state's power to *name* things:

The concentration of juridical capital is one aspect, quite fundamental, of a larger process of concentration of symbolic capital in its different forms. This capital is the basis of the specific authority of the holder of state power and in particular of a very mysterious power, namely his power of nomination . . . Thus is a *central authority of nomination* gradually constituted.⁶⁸

The state, then, if you insist on keeping this designation, would be the ensemble of fields that are the site of struggles in which what is at stake is the power to constitute and to impose as universal and universally applicable within a given "nation," that is, within the boundaries of a given territory, a common set of coercive norms.⁶⁹

A very important historical component of the US federal government, in this regard, is the centrality of the economy bestowed by the state's authority of nomination. As Allison L. Palmadessa reports, "[t]hrough analysis of presidential discourse, I found that the dominant identity of the US from 1862 to 2015 was consistently related to the nation's superior status, predominantly in the market economy."⁷⁰ Accordingly, science policy deliberations had fundamentally always revolved around the goal of economic returns. What made the basic research ideology case unique was that the notion of economic returns had been disrupted from its traditional understanding by basic research.

Finally, Emirbayer and Johnson complement the field of power with a *space of struggle for organizational power*. This is a meso-level subset in which organizations indulge in a

⁶⁷ Pierre Bourdieu, "Rethinking the State: Genesis and Structure of the Bureaucratic Field," Translated by Loïc J. D. Wacquant and Samar Farage. *Sociological Theory* 12, no. 1 (March, 1994): 4, 9-10.

⁶⁸ Ibid, 10.

⁶⁹ Bourdieu and Wacquant, *Reflexive Sociology*, 111-112.

⁷⁰ Allison L. Palmadessa, *American National Identity, Policy Paradigms, and Higher Education* (New York: Palgrave Macmillan, 2017), 4.

similar sort of conflict.⁷¹ I use this in particular reference to the conflict between research agencies, which are by default in proximity to the field of power but often not directly involved, whereas I consider the conflict between the organizations and real groups involved in the first government-wide deliberations for science policy to be of the field of power.

The comparative method of analogical reasoning between different fields or the same field in different times is considered as a useful way of constructing objects of analysis.⁷² Besides applying this throughout the thesis, I go beyond constructing basic objects of analysis and construct what I call the ‘bureaucratoscientific field.’ It should be stressed that this notion does not signify the emergence of an autonomous field with its own logic, effectively meaning that it is not actually a field as defined by Bourdieu. It is neither a subfield. Rather, it is the social space in which a unique interaction of multiple fields occur—mutually necessary, i.e. one field’s existence necessitating the other, but still disparate in their logics, i.e. their respective forms of capital challenging each other in symbolic power. In a sense, it is the obversion of a field, or an ‘obverted field’ for the lack of a better term, since, rather than having its own autonomy, it comprises two separate field-assertions to autonomy.⁷³ Such uniqueness might prompt some to consider these to be fields in and of themselves, but I do not believe this to be the case in the strict sense. That is, in essence, how the bureaucratoscientific field works. It comprises the bureaucratic field (or political field insofar as these can be considered the same) and the scientific field. In this arrangement, the bureaucratic field is structurally more powerful (due to the field of power) but the scientific field, besides being integral to their relationship, can also command significant power in light of (but not limited to) the broader circumstances that encompass even the field of power—for example, international relations. I have arrived at

⁷¹ Emirbayer and Johnson, “Bourdieu and Organizational Analysis,” 13-14.

⁷² Bourdieu and Wacquant, *Reflexive Sociology*, 233-234.

⁷³ It can also be called a ‘composite field,’ but I believe that obversion better categorizes the field’s autonomy, or lack thereof.

the conceptualization of the bureaucratoscientific field, and through it the notion of the obverted field, only after a significant collection of data and a rigorous analysis and my belief in shortcomings of merely explaining the relevant phenomena through fields separately. In a sense, this thesis aims to explain the genesis and early history of the bureaucratoscientific field, first to have emerged in the United States.

This has several contributions. First, the obverted field is a partial answer to the problem of explaining the interaction of different fields. Second, this thesis incorporates international relations (which was of monumental importance to the bureaucratoscientific field), something very rare in the field theory literature, and demonstrates the importance of this social world often disregarded by other researchers. Third, it extends the use of the organizational field, as the emergence of the bureaucratoscientific field structurally coincides with the emergence of the organizational field of federal research agencies. I am not certain as to whether this is would be the case for other obverted fields, but I hypothesize it to be so since the field needs it to be durable. Fourth is the particular application of this concept to the cases at hand in order to better explain basic research ideology.

CHAPTER 2: BASIC RESEARCH IDEOLOGY IN THE POSTWAR ERA

Early colonial colleges were private institutions disposed to the cultivation of social and political leaders. Even though Enlightenment influences (challenging the predominant mode of education) brought along an emphasis on science and indirectly the practical arts, this role was to be maintained for a long time. A significant change occurred with the Morrill Land Grant Act of 1862, later expanded with the Act of 1890, which established durable links between the federal government and higher education institutions for the first time. The Act was aimed at the practical arts of agriculture and mechanics with an intention to improve techniques through scientific research, adding an important point of emphasis in higher education institutions. Despite its practical significance, the universities established by virtue of the Act still contained the collegiate curricula of the private colleges and their actual operations for scientific research remained dubious. Universities developed its role in research independent of the federal government, which had become institutionalized by 1890 but was regardless isolated from much of the university. In other words, research was not a preoccupation in many of its other components. This would, however, change over the decades.

By the end of the 1920s, universities had become *the* center of basic research. This was an important distinction, as the most important endeavors in applied research were being carried out in industry and the government. Internationally competitive at the same, the US university system became an international powerhouse before the interwar era was out. The departmental system was an organizational innovation that not only increased research opportunity but also put teaching and research into better harmony.

By the time the war in Europe broke out, American scientists had never commanded genuine influence in the corridors of power in Washington DC. When US participation in the war seemed inevitable to a group of leading scientists, their concern of the scientific unpreparedness of the US led them to prepare a plan for mobilization, which was well received by President Roosevelt. These scientists had participated in the scientific mobilization in WWI more than twenty years ago and had witnessed the lack of co-

ordination and authority given to the then scientific leadership and the arguably destructive risks it wrought. This effort was led by Vannevar Bush, who became the natural leader of the group due to having the closest contact to the halls of the White House and more contacts in Washington—he was the Chair of the National Advisory Committee for Aeronautics (NACA) and the Carnegie Institution of Washington (CIW). Becoming the first unofficial presidential science advisor, Bush commanded power through the President and other important administration officials, such as Secretary of Defense Henry L. Stimson. This was, of course, in addition to the organized space given to the scientists. Established by executive order, the National Defense Research Committee (NDRC) was to nationally mobilize scientists, primarily in universities, by contracting them to work on specific projects in order to bring the kind of cutting edge research to the military that it could carry out itself. Once established, the scientists quickly went on with their work, soon understanding that the current arrangements made it difficult to make a full contribution. Besides not being authorized in medicinal and aeronautic research (due to other organizational jurisdictions), the NDRC also faced organizational resistance from the Services. Bringing the matter up to President Roosevelt, the scientific leadership received the Office for Scientific Research and Development (OSRD) through an executive order, which, besides having impressive wartime legal powers in the authority of its Director, subsumed medicinal and aeronautic research and was organizationally elevated. The NDRC was also subsumed and its Chair, Vannevar Bush, had become the Director of the new organization. The OSRD made full use of its legal mandates and extended its involvement into the Services, causing resistance among some military men. While getting along with some better than others, Bush intervened on behalf of his colleagues when those who actively resisted impeded the OSRD's progress, even "punishing" some to make an example.

The scientific mobilization was distinguished by its organizational decentralization. Unlike scientists working under laypeople in other government bodies, OSRD members were privileged to be working under scientists and engineers, whom themselves were familiar with conditions conducive to a better research environment. The contract program was also a successful undertaking. A similar mode of operation was used in the National Cancer Institute (NCI) in relatively limited budget some time before the war, but the NIH, its parent organization, never became convinced of its desirability. Accordingly, the OSRD

demonstrated the first large-scale use of a contract program for scientific research—not only in the United States but also in modern history. The success of the program was to have a paradigmatic impact on postwar science policy in the US and on the rest of the world in the long run. The same scientific leadership was also responsible for initiating the development of the atomic bomb, which was later transferred to the military in the form of the Manhattan Project as necessary funds soared way beyond what OSRD could handle in tandem with its other responsibilities. They nevertheless continued to be involved in an advisory capacity. Other consequential achievements that were later overshadowed by the atomic bomb included (1) the first ever mass production of penicillin and the lowest rate of disease-related deaths recorded in the US army, (2) anti-submarine technology that stopped a possible early German victory, and (3) the proximity fuze that revolutionized rocket warfare. Scientists were not only in the laboratories but also on the field providing assistance to officers with their new weaponry and even going into enemy territory to investigate enemy technological progress. By the end of the war, the scientific leadership emerged with a prestige that was further recognized by the federal government through the wartime power's it gave. Before the end of the war and his death, President Roosevelt sent Vannevar Bush a letter asking for his recommendations on science policy. The points were (1) the dissemination of wartime research, (2) improving medicine, (3) the proper federal role in scientific research, and (4) the discovery of scientific talent. This was in fact a response of some administration officials and Bush to the science legislation debates in Congress initiated by Senator Harley Kilgore. Kilgore's introduced legislations caused wide criticism due to its lack in both war and peacetime considerations, going under several revisions by the time the war had ended. The scientific leadership already desired federal patronage for basic research, but Kilgore's attempts possibly hastened them (and administration officials) to have the President draft a letter that would reflect Bush and his colleagues' perspective on things. Accordingly, Bush established several committees, constituted of leading scientists and engineers, and prepared the famous report *Science—The Endless Frontier*. Also known as the Bush Report, it shaped the framework for science legislation debates and became an authoritative document. Very simply, the report argued that (1) public welfare needed strong industry, (2) strong industry required applied research, (3) applied research built upon basic research, and that (4) the

federal government was the only viable source of sufficient funding for basic research. This was conceived to be executed in the form of a *National Research Foundation*, which would be managed by scientists (as opposed to Kilgore's conception of a joint management by scientists and laypeople) and without political influence.

The postwar effort for federal patronage exemplified several key differences from earlier experiences. The first significant effort sprang from the scientific leadership established in the National Academy of Sciences (NAS), an organization created by President Lincoln to serve the federal government in scientific advice when called upon. Being conservatives who were always weary of the possibility of government control, the scientific leadership (in addition to their political contacts—primarily Herbert Hoover) they believed that industry was the most suitable patron for basic research. Basic research, even though consequentially unpredictable since it is predicated on discovery, was the bedrock of applications and, hence, innovation. Industry did not itself give much importance to conducting basic research, but universities were the best locale for it anyway, which was where the support was intended for. To this end, the National Research Fund was created in 1925 and marketed to industrialists for its economics returns. However, the unpredictability, long-term returns, and, most importantly, the public good dimension made it quite difficult to round up sufficient support for the Fund. Effectively defunct, the Depression dealt the final blow and the Fund was declared dead in 1932. This experience primarily demonstrated the difficulty of bringing in private interests to support a peculiar resource like basic research. An attempt soon followed on the higher echelons of the federal government. Beginning in 1933, similar ideas were voiced in the Science Advisory Board, temporarily established by President Roosevelt to advise the New Deal programs. There, Karl Compton finally convinced the “old guard” of natural scientists to accept that the federal government was the only viable alternative for patronage and that such a program would be to everyone's benefit if it was well formulated. The difficulty, however, was the presence of social scientists (primarily economists). The Science Advisory Board was not readily dominated by leading natural scientists as it was in the NAS, and the social scientists were particularly close to the Administration due to their direct involvement in the New Deal programs. This became a serious obstacle due to the natural scientists' reserve on having the social sciences be supported as well, since it would bring directly politically

relevant studies into the fold and give conservatives a reason to worry about political control. Seeing that the Administration aligned themselves with the social scientists, the natural scientists sought allies of their own in Congress, which set the political alignment leading to a stalemate that would continue until WWII. (1) In the pre-WWII circumstances, science legislation could not attain a level of priority that would trump traditionally important matters. (2) Neither did the scientists, whether natural or social, possess the influence to enforce their agenda. These two factors would be radically different in the aftermath of the war.

Debates on postwar science had begun independently among scientific organizations and groups long before the war had ended, and the release of *Science—The Endless Frontier* was publicly followed. The report promoted the aforementioned reasoning regarding basic research. It also went beyond it. First, the war had depleted a serious portion of fundamental knowledge, leaving little science for applied research, and then innovation, to be based on. Second, Europe, the traditional source of most basic science, was shaken to its core in the war and was in no shape to retain that role. Third, even at that, it was desirable that a nation be self-sufficient when it came to basic science. It was a necessary component in economic development and dependence on other countries always brought a certain risk. Fourth, the virtually universal drafting employed by the US disrupted its student body more than any other country. The government had to take measures to ensure that its scientific and technical labor would be able to renew itself by the time student draftees would come of age. Fifth, the labor issue was foreseen to be exacerbated by the fact that research facilities in industrial laboratories were far superior to ones in universities, which could possibly cause a labor displacement that would negatively affect the production of fundamental knowledge in universities. Universities were organizations that were structurally best suited for basic research and were envisioned to be the primary (but not exclusive) recipients of federal support. Research would be directly supported through contracts or grants, future and young researchers through fellowships, and research infrastructure through funding for expensive equipment, etc.

Vannevar Bush was perceived to be and in fact was the single most influential American scientist during the war, which is also incorrect in the oxymoronic sense that his adherence

to the scientific *modus operandi* meant that he left scientists to their own when it came to research.¹ *The Endless Frontier* received much attention not only because it was primarily authored by Bush, but also because it was requested by the highest political office. It was President Truman, however, who received the report, as President Roosevelt died before the war had ended. President Truman did not receive the report well, likely due to having a closer conception to Senator Kilgore on multiple points, but arguably primarily because the report stripped the President from the power of unconditional appointment regarding the Foundation's head. The report was forwarded to Congress, where the natural scientists would also head due to their differences with the White House, as was in the interwar era. It cannot be known how different things would have been if President Roosevelt was still in office, but it can easily be presumed that there would be more one on one communication between the scientific leadership and the President considering the close relationship between Bush and Roosevelt. This did not happen with President Truman.

There were five primary points of debate in postwar science policy. (1) Patents reflected an almost typical Republican-Democrat dichotomy. The first argued that giving patents to the government would stymie industrial involvement in academic research, which would then hinder the transfer of basic science into innovations, and thence economic development. The latter argued that what was paid with public money ought to remain public property. (2) The social sciences reflected the same debate it did when Karl Compton had united the natural scientists for federal patronage in the interwar era. However, the social scientists were virtually inexistant in debates this time as the natural scientists (and engineers) had monopolized the field of expertise. Accordingly, the issue was initially fought by the Administration and Congresspersons against the natural scientists and other Congresspersons. The more science legislation was postponed due to disagreements, the more scientists would agree to accept the social sciences, as it became an increasingly minor issue in consideration of the risk of a failure to establish a science foundation. (3) The issue with the most long-term significance was that of geographical distribution. Accused

¹ This simultaneity is often disregarded by scholars.

by Republicans to be a pork-barrel feature, Republicans agreed with the natural scientists that merit alone should be the determinant of funding, which would be operated through peer reviewing. On the other hand, the liberal front conceived a more equal distribution as a democratic imperative. (4) A more fundamental debate was concerning the scope of research to be supported: basic and applied. Kilgore's early bills made no such distinction, encompassing everything, whereas the scientific leadership promoted basic research. This was where universities could conduct research without the direct involvement of private interest, effectively preventing federal involvement in a game of private interests. However, it must be noted that the distinction between the two forms of research primarily emerge as one of natural sciences, even though it is still applicable to the social sciences to some extent. If exploratory studies are conceived to be basic research, then the bulk of social sciences becomes applied or a mixture of applied and basic research. Regardless, such an overlap or conflict was not explicit in the debates. (5) The control of the Foundation was the most important immediate issue. Bush was himself ready to propose a presidentially appointed Director, but the agreement that emerged from other committee members helping prepare *The Endless Frontier* were adamant in their concerns, to which Bush was convinced and in fact continued later to push for when most others were ready to compromise. The scientific leadership was not like the Old Guard who categorically shunned away federal involvement, but they also wanted insurance that it would be protected from potential political domination. They initially proposed that the Foundation choose the Director² from amongst themselves, who were first appointed by the President. Liberal Democrats rejected this on democratic grounds, arguing that the head of a federal agency had to be responsible to public office.

Congressional hearings, debates, much lobbying, and a presidential veto in 1947 delayed the establishment of the National Science Foundation (NSF), which was finally created through the National Science Foundation Act of 1950 (this period is detailed in Chapter 5). Despite much quarrel over the bill's content, President Truman recognized Bush by

² 'Administrator' was the preferred term in *The Endless Frontier*.

referring to *The Endless Frontier* in his address. The primary issues that caused debate and their (at times tentative) conclusions are as follows.

(1) The patent issue was resolved without much detail, which would have to be revisited in the following decades. Ultimately, patent could be given to industrial patrons if there support was significant with the caveat that the government would retain a license. However, I give very little attention to this issue, since the legislation remained virtually irrelevant for the given period. Basic research, as already argued by then scientists, extremely rarely resulted in patentable innovations, which is why it never received much attention until universities and the federal government increased their interest in patentable applied research during the late 1960s and early 1970s. (2) The social sciences were finally integrated in a passive manner that was agreed to be dealt with later. Conceived to be directly related to science (its history, philosophy, economics, social impact, etc.), it would continue for some time to be thematically dependent on it. (3) Geographical distribution was integrated into the final legislation but was not in a manner that would put strains in the Foundation's intent to prioritize merit. In fact, the Foundation's staff and the Board would successfully resist calls for more distribution by arguing that they would, but that insufficient budgets precluded from doing so. (4) Foundation was authorized to support basic research. However, as mentioned, the partial overlap between the natural-social sciences and basic-applied research was disregarded in the debates. The limited scope of social sciences actually meant that some amount of applied research was included. (5) All other issues became minor when compared to the control of the Foundation. It was also the reason behind President Truman's veto against the first bill passed by Congress. Convinced that this was beyond negotiation, the scientific leadership *in toto* helped prepare a bill that would be acceptable to the White House. Even though this was signed into legislation, the conservative scientists were regardless successful in establishing a strong National Science Board (NSB) that would nominate the candidates to be appointed by the President and also act as a balance in its administration.

Much had occurred in the meantime, however. A pluralist federal support system for basic research was emerging due to (1) the delay in creating the NSF and (2) the necessary implications of the disciplinary boundaries of other federal agencies. The Atomic

Energy Commission (AEC) was created in 1946 with an overarching sense of securitizing scientific knowledge (contrary to the NSF). Its creation demanded more attention due to the exigencies implied by nuclear power and was mandated with impressive powers. It was not surprising that AEC members wanted to invest in basic research related to their agenda, and they were authorized to do so. Besides investing in the relevant fields, the AEC also sought to solidify itself as the primary sponsor of related research. Before than this, however, the National Institutes of Health (NIH) were authorized to make grants for medical and related sciences. Grants had been actually utilized by a component of the NIH before the war, but both the NIH at large and other federal agencies were not convinced in its utility. The utmost success of the OSRD experience convinced them otherwise, after which the NIH was authorized to make grants in 1944. Ironically, while medical scientists were debating with other scientists whether there should be one science foundation to support all sciences or a separate foundation for medical sciences, President Roosevelt had signed legislation that would make the NIH into the kind of agency dreamt by medical scientists. This legislation received a lot less attention possibly due to the relatively small amount of research expenditure of the NIH at the time and the even smaller expenditure of its limited grant program (for cancer research). It was also less controversial due to the special social status enjoyed by the medical sciences. The perceived utilities of biomedical sciences meant that related basic research enjoyed a privileged status, which would profit the agency quite a lot in the ensuing decades.

An exceptional case was the Office of Naval Research (ONR) in the Department of Defense. The ONR was a one-of-a-kind organization that did not have equivalents in the Army or the Air Force. It was a combined result of some Navy officers who were “converted” to the idea of cooperation with civilian scientists after the OSRD experience and scientists who took part in the scientific mobilization in the Navy (also as a result of OSRD intervention). Different motivations, such as increasing civilian cooperation for nuclear research, considering basic research a worthwhile investment in and of itself, or a desire to fill the gaps until the NSF had been created, all coalesced in their practice of supporting basic research. The ONR was protected from an overarching military practicality due to being organizationally isolated from the rest of the Navy. Accordingly, its staff scientists enjoyed much flexibility in implementing their own agendas. They made use of contracts

as if they were grants (much like the OSRD), establishing the gold standard for supporting basic research. While many scientists were still concerned of a possible instability of funding (and the indirect damages this would cause), officers from the ONR even toured the country and met with scientists and university administrators to demonstrate their commitment. Primarily directed to the physical and mathematical sciences, 1946-1950 came to be regarded as a golden age for academic science due to the rigor the ONR demonstrated in fulfilling its promises to the scientific community. The ONR's generosity, both in the amount of funding and the breadth of supported research, came to be narrowed as budget cuts brought its under military scrutiny. *Relevance* became a key word, due to which the ONR retreated into more applied research and less fields in time.

The plurality of the system was defined by efforts for and against the monopolization of domains of influence. The NSF tended to be at the center of these currents. First, the Foundation was principally against the monopolization of any organization (which meant any group of people), including itself. Accordingly, the statutory role for evaluating and coordinating other federal agencies regarding research was strongly opposed by both the Foundation and the National Science Board. Supporting science required the flexibility that science did as well, and such an evaluative role highlighted age old fears that it could lead to political control. Similarly, the NSF also resisted calls for involvement in applied research. However, besides these principled stances, the NSF was also cautious due to fearing conflict with much larger agencies. Second, federal agencies (primarily the NSF) were against monopolization by others. For example, the NSF and the DoD cooperated against the AEC to break its monopoly in nuclear research. The NSF also challenged the NIH and prevented a monopoly in the biomedical sciences. Finally, retaining its mandate for defense research meant that the NSF also broke the DoD's monopoly in defense research. The harmony between the Foundation's Director Alan Waterman and the NSB was an important element in most such efforts.

Simply put, basic research ideology promoted basic research and its necessary correlates for its long-term benefits in public health and welfare, and designated universities as its center and the federal government as its only viable patron. Postwar national science policy was largely prescribed by it. It was unique in *operationally* integrating the idea that

the quality of academic science was in national interests for the first time in history. What came out of this, i.e. the basic research consensus, was made possible by the symbolic power the scientists had derived from their role in WWII. The university system subsumed even more of the scientific field, and a bureaucratoscientific field crystallized as federal research agencies and their boundaries assumed form. The value of scientific capital was redefined and scientists occupied an institutionalized position of power in the government they never had.



CHAPTER 3: EARLIER AMERICAN HIGHER EDUCATION AND THE SCIENTIFIC MOBILIZATION OF WORLD WAR II

3.1 Research and Federal Involvement in Earlier US Higher Education

Initially imitating the British, a distinct organizational pattern of college education emerged in the US by 1740, usually incorporating a strong president under the authority of an external board and a weak faculty. New knowledge, those products of the Enlightenment, took time to make their way into colleges like Harvard and Yale due to the relevance of course contents for careers and the saturation of graduates with Christianity even before they began their ministry training but began to do so after 1740. Yet even minister graduates were a minority in most colleges. Education was primarily sought for its social status—that is, to become a gentlemen—and to seek career alternatives for most of the second half of the 18th century, due to which why many did not even remain for degrees. It is no surprise then that the Founding Fathers themselves were predominantly college graduates. These institutions retained this role to the new republic after the American Revolution—this time to cultivate republican virtues. This form of Republicanism also incorporated the practical arts, embracing the Enlightenment’s ideal of “useful knowledge.” Harvard became the first republican university as this connection of literature and science to republican virtues was written into the Massachusetts’s new constitution. Yale followed this as envisioned by Ezra Stiles and the College of William & Mary by Thomas Jefferson. What had changed was that the traditional role of colleges was mostly retained but in a political ideology, whose Enlightenment influences incorporated professional learning—in the form of “useful knowledge”—that can be conceived as conflicting with the sense of propertied elitism of the gentleman.¹

¹ Roger L. Geiger, *The History of American Higher Education: Learning and Culture from the Founding to World War II* (Princeton & Oxford: Princeton University Press, 2015), 30-31, 79-81, 91-99.

Into the 19th century, the number of functioning colleges jumped from 28 in 1815 to 80 in 1840. “Like no other nation, the United States expanded higher education by multiplying small colleges.”² Yet unlike European universities, most of these resembled secondary schools, providing more access to education, but also making their role more ambiguous, which persisted throughout the 19th century. According to Roger Geiger, science had three guises in these institutions: self-improvement, useful knowledge, and the advancement of knowledge.³ Self-improvement, or its several restatements, indicated the presumed qualities necessary for being a respectable and commanding member of society, holding an office, or being involved in the implicated career alternatives. Useful knowledge, on the other hand, comprised the “productive” essence of the curriculum, in the many forms of agriculture, engineering, law, medicine, and so on. In this sense, it can be construed as being similar to ‘applied science,’ with the caveat of the ambiguity of its scientificity. The advancement of knowledge, similarly, denoted a sense of “knowledge for the sake of knowledge,” as seen in basic science, but with the same caveat. Regardless, it must be emphasized that these two latter guises were very much entangled and provided a kind of model that was contrasted with the collegiate education emphasizing self-improvement.

In the context of such conceptions of higher education, the relation between these institutions and the federal government was primarily one of a reproduction of privileged classes.⁴ This was to change with the Morrill Land Grants Acts. The doctrine of useful knowledge had become common in the 19th century with its two areas of application being agriculture and civil engineering. Mechanics institutions had been founded in the 1820s, but only the Franklin Institute of Pennsylvania endured and West Point’s emphasis on engineering diminished. This paradigm made a comeback—and an institutionalized one

² Ibid, 194.

³ Ibid, 173-175, 256.

⁴ A natural exception to this rule was the West Point Military Academy. Another related matter is Benjamin Rush’s idea of a “national university” and the later failed attempt of a federal university. See: Ibid, 102-109, 178-186.

at that—with the Morrill Land Grant Act of 1862, latter expanded with the 1890 Act bearing the same name. Each state that accepted the Act's terms was to receive 30,000 acres of federal land for each Congressperson to establish at least one college of what came to be known as the agricultural & mechanical universities. These were, effectively, were to provide educational opportunities for the industrial classes in the practical arts while at the same time providing the scientific and classical courses given in the colleges. Demonstrating the previous mixed conception of useful and scientific knowledge, it was conceived to help alleviate the falling productivity in agriculture through scientific research. The agenda ran through significant difficulties, as the compatibility of practical and liberal educations were a matter of debate, but more importantly, the scientific and technological underpinnings were not or did not become as presumed to the conceived ends.⁵ Regardless, these developments caused an important policy paradigm shift. When the Morrill Act was first introduced as a bill in 1859, it was vetoed by President James Buchanan on the grounds that the boundaries between states and the federal government ought to be preserved.⁶ It made a huge leap in the fiscal amount of federal involvement in higher education, however, and set a precedent for further similar developments.

Another important development was occurring in the direction of research—the outside world had made a connection between the success of German universities and its organization, leading to reforms trying to adopt a similar model.⁷ This placed a strong emphasis on graduate education and the integral role of research, equal or perhaps even superior, to teaching. With the dissemination of similar practices and conceptions, colleges would increasingly resemble universities and undergo internal tension, as many would consider

⁵ Ibid, 259, 281-284.

⁶ Palmadessa, *American National Identity*, 79- 85.

⁷ Ben-David and Zloczower, “Universities and Academic Systems in Modern Societies,” 48.

collegiate ideals to be under assault.⁸ Furthermore, as Clark Kerr states, the research-centered German orientation would strengthen the impact of land-grant universities.⁹

[T]hese two influences turned out to be more compatible than might at first appear. The one was Prussian, the other American; one elitist, the other democratic; one academically pure, the other sullied by contact with the soil and the machine. The one looked to Kant and Hegel, the other to Franklin, Jefferson, and Lincoln. But they both served an industrializing nation and they both did it through research and the training of technical competence. Two strands of history were woven together in the modern American university.¹⁰

By the end of 1890, research had become the foremost preoccupation of emerging universities, even though this was based on a “patterned isolation of its component parts.”¹¹ However, US universities lagged behind the international powerhouses of research. Carl Snyder’s comment on US research on the human brain would be applicable to much scientific research: “The tribute of America is conspicuous by its absence.”¹² This case can be illustrated in the field of metallurgy. Snyder remarked:

⁸ A qualification of the German influence must be made. The American conception of the German model omitted the chair and Institute centered hierarchical relationship in research that began to be detrimental to the research agenda. As Ben-David and Zloczower argues, research had the potential to be even of superior importance to teaching since it was lodged in departments. On the other hand, their contention that “What did come to the United States from Germany was something more limited: the professionalization of the teachers” does not do justice to the impact of the American perception of the German model. See: Ben-David and Zloczower, “Universities and Academic Systems in Modern Societies,” 33, 75.

⁹ Clark Kerr, *The Uses of the University*, 5th ed. Cambridge (MA: Harvard University Press, 2001), 36.

¹⁰ *Ibid*, 11-12.

¹¹ Geiger, *American Higher Education*, 326; Roger L. Geiger, *To Advance Knowledge: The Growth of American Research Universities 1900-1940* (New York and Oxford: Oxford University Press, 1986), 16. This can be conceived as a form of “loose coupling,” as it makes possible the coexistence of different and even contradicting functions and purposes of the university. Hüther and Krücken, *Higher Education in Germany*, 150. See: Karl E. Weick, “Educational Organizations as Loosely Coupled Systems,” *Administrative Science Quarterly* 21, no. 1 (March, 1976): 1-19.

¹² Carl Snyder, “America's Inferior Position in the Scientific World,” *Science* 174, no. 572 (January 1902): 59-72.

I have before me the latest volume on metallurgy, even handedly summing up twenty years of marvelous [sic] work. From almost a dictionary of names, Belgian, Netherlandish, German, English, French, Russian, I can find but two Americans, Professor Gibbs, whose contribution was most indirect, and Professor Howe. Not even a science upon which rests a billion dollar trust seems to arouse the interest of one original, inventive American mind.¹³

Not surprisingly, then, the first decade of the 20th century became an intermittent debate on the roles of teaching and research,¹⁴ as complaints about the insufficiency of time for research (due to teaching loads) was common.¹⁵ However, leading universities increasingly set a standard of teaching hours (about 8 hours a week) and incentivized research through the developments of sabbatical leaves, separate research funds, and the spread of graduate fellowships and research funds.¹⁶ By the end of the 1920s, universities had become the dominant institutions of research in the US and became more or less equals with European universities. Pure research institutions, such as the Rockefeller Institutions for Medical Research and the Carnegie Institution of Washington, which had been founded partially on the notion that the multi-purposeful nature of research universities made them less suitable for research, were inevitably assimilated into the university research system.¹⁷ These are important in that they defamiliarize us with the taken-for-granted centrality of universities to science. This was accompanied by explicit appointments of presidents who favored research. Those presidents of the leading universities, however,

¹³ Ibid, 62.

¹⁴ For example, US physicists had less time for research than their German, French, and British peers. Geiger, *To Advance Knowledge*, 71-72.

¹⁵ David Starr Jordan had made a survey with 100 leading academics, receiving similar opinions. David Starr Jordan, "To What Extent Should the University Investigator be Freed from Teaching?" presentation before the 7th Annual Conference of the Association of American Universities (San Francisco, March 1906), reprinted in *Science* 24, no. 605 (Aug. 3, 1906): 135.

¹⁶ Geiger, *To Advance Knowledge*, 70, 75-77.

¹⁷ Geiger, *American Higher Education*, 489; Geiger, *To Advance Knowledge*, 61, 65-66, 233-234. For example, US medical discoveries surpassed all other nations by the 1920s; the US contained more professional psychologists than the rest of the world combined; and US chemical research reports surpassed German ones after 1930. Geiger, *American Higher Education*, 528-529; "Chemical Research Reports," *Science* 94, no. 2434 (August 22, 1941): 182.

reached the conclusion that research should be sustained on endowments, as they lacked the extra financial resources to make sustained commitments.¹⁸ In summary, research universities had become the central institutions of scientific research, but research was tenable on the basis of endowments and what little universities could skim from other incomes. What had occurred was that the organizational field of higher education had effectively subsumed most of the scientific field—primarily that of basic science, but also of applied science. What this meant was that any organizational relations with science was in large part relations with higher education institutions. This was particularly relevant for the future of science policy.

Federal expenditures for science at the start of the 20th century was a significant amount of \$11 million—more than \$330 million when adjusted to the 2019 inflation rate.¹⁹ As previously mentioned, the land-grant institutions was the most concrete institutionalized relation between the government and universities. Then there was the National Academy of Sciences (NAS), signed into legislation a year after the first Land Grant Act and also by President Lincoln, and its operating arm, the National Research Council (NRC), created much later.²⁰ These did establish a link between the government and academic scientists, however its statute assigned a passive role to the NAS—that of giving scientific advice when requested. Effectively, the government’s primary relation to science was with its own bureaus (e.g. the Bureau of Mines, Bureau of Standards, and State agricultural experiment stations), which had a utilitarian orientation—in other words, there was no policy concern for basic science. As Geiger puts it, “[t]he fact that the scientific bureaus

¹⁸ Geiger, *American Higher Education*, 84-85, 233.

¹⁹ Official data only goes back to 1913, which would be an equivalent of \$287 million according to the U.S. Bureau of Labor Statistics and \$284 million according to the Federal Bank of Minneapolis. Accordingly, official estimates might not do justice. Non-official estimates would suggest more than \$330 million. U.S. Bureau of Labor Statistics, CPI Inflation Calculator, <https://data.bls.gov/cgi-bin/cpicalc.pl?cost1=11&year1=191301&year2=202006>; Federal Reserve Bank of Minneapolis, CPI Inflation Calculator, <https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator>; Ian Webster, Inflation Calculator, <https://www.official-data.org/us/inflation/1900?endYear=2019&amount=11000000>.

²⁰ The NAS was created during the Civil War and the NRC during World War I.

of the government did make some fundamental contributions to the advancing of human understanding of the natural world is thus almost incidental.”²¹ They were further distracted from any such inquiries due to their routine services. Similarly, the nexus between these government bureaus and academia, i.e. *the* institutional setting for basic research, was sporadic. Even the Smithsonian Institution, administered by the government, was founded in 1846 as a result of the first considerable sum of support for basic science, given by a foreigner—James Smithson. A Department of Science and the establishment of science advisory boards were suggested in 1884 and later just a board in 1908, but only did President Franklin Roosevelt in 1933 establish such a board, and did so temporarily.²²

3.2 General Trends: The New Research Economy

By World War II, leading research universities in the US became international powerhouses. Organizationally better suited for research, these (and other up and coming universities), however, would become dominant institutions. The immense postwar transformation of the American higher education system and, along with it, of the research economy, in the words of Clark Kerr, “has taken place without a revolution, for a time almost without notice being taken.”²³ According to him, this was the most important development in US history along with the first Morrill Land Grant Act of 1862.²⁴ Elizabeth Popp Berman frames this succinctly:

It is hard to overstate how much World War II changed the landscape of academic science in the United States. University research was a modest, small-scale endeavor until the Manhattan Project demonstrated the power of science and, in the process, transformed the way it was organized. Building the bomb that ended the war gave scientists a great deal

²¹ Geiger, *To Advance Knowledge*, 60.

²² Ibid, 59-60; Vannevar Bush, *Science—The Endless Frontier* (Washington: United States Government Printing Office, 1945), 78-79.

²³ Kerr, *The Uses of the University*, 34.

²⁴ Ibid, 35-36.

of public respect and influence that, in the postwar years, they would leverage into a massive new system for the support of academic science.²⁵

In addition to the pre-existing relationship between the Department of Agriculture and land-grant universities, i.e. the most explicit relationship between universities and the federal government, four new streams of research support were established: the Atomic Energy Commission (AEC), the Public Health Service (PHS), the Office of Naval Research (ONR), and the National Science Foundation (NSF). There were three patterns of federal support by the end of the 1940s. At one end were the large national wartime laboratories (most but not all with an applied concern) used by university scientists and at the other individual projects (predominantly basic science) supported by the aforementioned federal agencies. In between these were research centers concerned with specified fields of interest. The offset of the Cold War had an impact on the research economy in two ways. First, through a change in the nation's leadership of federal science. Members of the old guard who opposed the H-bomb were "ousted" from their positions of power in policy-making or were left to leave at the end of their terms. Second was McCarthyism, which had a lasting influence through legislation even after it was over. As it dominated the research economy, academia regurgitated some of its earlier concerns regarding federal support but this time much more seriously. These came in the forms of (1) distortion of science, (2) displacement of the university's functions, (3) dependence on the federal government, and (4) domination by it.²⁶

Universities were institutions of instruction in organization and in budget, which also brought along with it a period of adaptation. Research was usually conducted along with and as a part of teaching but began gaining autonomy as the research economy expanded. An increased role of research prompted researchers and academic units to specialize and organize along these. By 1955, organized research had become the norm. Regardless, the

²⁵ Popp Berman, *Creating the Market University*, 19.

²⁶ Geiger, *Research and Relevant Knowledge*, 18-19, 33, 35-37, 48-49, 59.

fact that a good portion of federal support was organized along the lines of individual projects rather than institutions caused an unbalanced development.²⁷

The conditions of the transformation were provided by World War II and the scientific leadership who successfully delivered what was expected from them, and it was they who sparked these into a deliberate effort to actualize it. Despite this, it had “developed more by force of circumstances than by conscious design”²⁸ until the early 1960s, during which institutional effort were being initiated. While these circumstances created much imbalance in many different axes, these were significantly institutionalized by the scientific leadership who evoked the precepts of basic research ideology. These created imbalances were

the wishes of individual scholars with those of their institutions; New England with the South; the sciences with the humanities; research with teaching; graduate training with undergraduate education; merit with politics; the judgment of specialists with general formulae. And yet one of the more productive aspects of federal involvement to date has been its imbalance.²⁹

These circumstances were not wholly unconscious, as they reflected the agenda of “best-science” pushed by the scientific elite but ultimately were a product of the negotiations between different individuals, groups, and organizations. The most important of those collective actors was arguably the scientific leadership who “led the scientists into war,” whose importance resided in the influence they derived from their success during the war. This is the subject of the following section.

²⁷ Ibid, 46-47, 58, 61.

²⁸ Kerr, *The Uses of the University*, 37.

²⁹ Ibid, 38.

3.3 World War II and the Greatest Scientific Mobilization in History

Mobilizing for war—the National Defense Research Committee. By the outbreak of World War II, the United States had lagged behind in terms of modern weaponry. Several key scientists were concerned in the spring of 1940 that circumstances might force the US to partake in the war while lacking such organizational preparedness in science. These were Vannevar Bush, President of the Carnegie Institution of Washington (CIW) and former Dean of the MIT School of Engineering; James B. Conant, President of Harvard University; Karl T. Compton, President of MIT; Frank B. Jewett, President of the National Academy of Sciences and the Bell Telephone Laboratories; and Richard C. Tolman, Dean of the Graduate School of Caltech.³⁰ This was also necessitated since the NAS was designated as an advisory role by statute, meaning that it could not act as an operating body.³¹ Partaking, with varying degrees, in the scientific efforts during WWI, they knew that arrangements were insufficient. Bush personally had observed the lack of scientific cooperation then and would later make genuine use of this experience. The perception of danger was augmented since contemporary research in nuclear physics pointed out that an atomic bomb was possible.³² In the existing arrangement, the military would call upon scientists when considered necessary. Bush, however, after arranging a meeting with President Franklin D. Roosevelt, argued that science was too complex that they needed scientists familiar with innovative research to have a hands-on role. President Roosevelt, having accepted Bush's proposal with relative ease for an organization with coordinative and operative roles, also accepted a condition: that they work without compensation and by retaining their fulltime positions in their institutions.³³ Not only did this make this responsibility much more amicable to these men, but it also ensured institutional connections,

³⁰ Irvin Stewart, *Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development* (Boston: Little, Brown and Company, 1948), 3-4; Vannevar Bush, *Pieces of the Action* (New York: Morrow, 1970), 31.

³¹ Stewart, *Organizing Scientific Research*, 5-6.

³² Bush, *Pieces of the Action*, 33-34, 72-75.

³³ Stewart, *Organizing Scientific Research*, 6, 26, 278-279.

some resources, and the fact that they retained a civilian status, for the debate of federal support had and would again draw fears of bureaucratic domination. This organization, the National Defense Research Committee (NDRC),³⁴ was to be chaired by Bush. Being led by civilian scientists, including all of his aforementioned colleagues, it also many members from the Services. It was originally conceived to be a coordinator between military and civilian science on basic science, necessitated by the lack of basic research conducted by the military as their focus was on the improvement of existing weaponry.³⁵ Their role was understood to be supplementary as it would not replace ongoing research efforts by the Navy and Army, and did not involve aeronautics, under the authority of the National Advisory Committee for Aeronautics (NACA). Early in its existence, the Committee decided to contract as much research as possible with other federal agencies and civilian bodies of research.³⁶ The NDRC defined its scope of activity on November 29, 1940 in a resolution in the following manner:

Resolved, that the National Defense Research Committee by reason of the order of the Council of National Defense which established it, is concerned with scientific research on and development of new instrumentalities or materials of war, or of new materials or methods to be used primarily in the manufacture of instruments of war; and of the improvement of existing instrumentalities or materials of war, or of existing material or methods to be used primarily in the manufacture of instruments of war. Where a material or method is widely used or useful in industry, in addition to its use in the manufacture of instruments of war, as for example in the case of substitute materials of wide utility, the research and development involved do not lie within the province of the National Defense Research Committee, but rather within the province of many existing industrial and scientific research agencies, and in particular, when appropriate requests for investigation or

³⁴ For the original members of the Committee and its Divisions, see: "The National Defense Research Committee," *Science* 92, no. 2395 (November 22, 1940): 484-486.

³⁵ Geiger, *Research and Relevant Knowledge*, 5.

³⁶ Stewart, *Organizing Scientific Research*, 8-9, 12-13.

research in such fields are made by government agencies, within the province of the National Academy of Sciences and the National Research Council.³⁷

The NDRC also acted as a sort of international liaison for scientific cooperation. The United Kingdom, having come in contact with German weaponry, provided useful information and experience, due to which Conant went to London to establish an NDRC office there.³⁸ In essence, the NDRC was

an ad hoc organization based upon preexisting informal networks within the American scientific community. Accordingly, it depended heavily upon the acumen of those in leadership positions and their contacts throughout the critical networks. The effectiveness of the NDRC on both of these counts has been widely recognized. In addition, the freedom and flexibility with which the NDRC operated is credited with facilitating its mission. These qualities, however, stemmed not only from its decentralized structure but also from the wartime conditions that afforded ample resources with minimal accountability. As a result, the factors that contributed to the NDRC's wartime efficacy were unlikely to endure after the cessation of hostilities.³⁹

Upping the ante—the Office of Scientific Research and Development. Despite its success, NDRC's leadership considered its mandate insufficient to fulfil other vital roles, without which most scientific effort would not realize its potential. First, between the phases of completing research for weaponry and developing these into instrumentalities were gaps, which the military was slow to carry out. This meant the NDRC required to extend its role of research over development as well. Second, the NDRC's coordinative role regarding basic research did not amount to much for the overall coordination between them, the Services, and NACA. The fact that different researchers might be working on very similar projects and the possibility of unknown accomplishments that might be useful to others necessitated this. Finally, there was no organized research being conducted into

³⁷ Ibid, 16.

³⁸ Ibid, 34.

³⁹ Geiger, *Research and Relevant Knowledge*, 11-12.

military medicine. Accordingly, Bush brought his proposal over to President Roosevelt, which was affirmed through the Executive Order No. 8807 of June 28, 1941. The newly created organization, the Office of Scientific Research and Development, subsumed the NDRC (of which Conant became the new chairperson), and along with a few other agencies, took over their contracts. It was also mandated with mobilizing the nation's scientific and technical personnel, and resources thereof. On a personal level, the Director of the OSRD became the President's *de facto* science adviser.⁴⁰ All of these gave Bush, through his Division Directors and close relationship with the President, immense authority over his mandate. This authority, by an industrial survey at the time, was found to be as such: "with the appointment of Dr. Vannevar Bush as director of the Office of Scientific Research and Development, the entire science resources of the nation, including those of the armed services, are now under the direction of one man."⁴¹ This authority, of course, was based on the recognition of his peers and the faith that it would be conducted with propriety. This propriety, however, would take a different meaning to different actors.

Scientists as uninvited guests. The technicalities of the arguments put forward by Bush and his colleagues, legitimating their civilian authority in over many bureaucrats and officers the midst of a global war, would have certainly passed over the heads of most administrators and military people. However, the management of science does not enjoy the scientificity that specific examples might, meaning that the anecdotal argumentation provided for the NDRC and the OSRD was more intelligible, hence more open to attack. Those parts that were more difficult to argue against by laypeople were those of scientific and technological complexity, where scientific capital carried more weight. To their benefit, the administrative experience of these scientists and their titles from very prestigious institutions carried much weight. This primarily cultural capital, along with other forms,

⁴⁰ Stewart, *Organizing Scientific Research*, 35. For the provisions of the executive order, see pages 36-37.

⁴¹ "Committee on Economic Trends of the Industrial Research Institute," *Science* 95, no. 2454 (January 9, 1942): 38.

coalesced into a symbolic capital that commanded much respect. However, those whose structural positions were threatened by this symbolic capital and particularly, among them, those whose field made it less likely to recognize it were natural rivals.

Despite Bush's authoritative position, the organization of science was decentralized and allowed scientific choices to be exercised by scientists alone.⁴² This was allowed for flexibility reasons as, beyond mere organizational factors, it was recognized that the centralized command of something highly specialized like science would be an impediment to research. In this sense, the scientific field from which Bush and his colleagues drew authority to non-scientists actually stipulated that Bush and other science administrators do not have the power to interfere in the business of other scientists. This independence, however, was not only individual or divisional but also organizational. For example, the NDRC asserted independence on projects from the outset and sometimes against the military specifically.⁴³ The strategic importance of scientific problems was also decided by the civilian scientists. The powers granted to the civilian scientists, accordingly, were seen as intrusion by military scientists.⁴⁴ The military field surely employs a different logic than that of the scientific field, but it is possible to interpret that the backwardness of military technology meant that there were even less overlap with the scientific field, which can be explanatory in why military scientists' positions were threatened by civilian scientists. Furthermore, the fact that many military officers in charge of military research bodies themselves were not scientists at all meant that, at the administrative level, there was an encounter of radically different habitus.

⁴² Geiger, *Research and Relevant Knowledge*, 5-6.

⁴³ Stewart, *Organizing Scientific Research*, 18.

⁴⁴ Geiger, *Research and Relevant Knowledge*, 340.

There were different means employed to exert the authority of civilian scientists. To begin with, Bush enjoyed a status as the President's informal science adviser.⁴⁵ Further, the President had asked service secretaries for support in utilizing the civilian scientists.⁴⁶ In these cases, the scientific leadership also possessed social capital, of which its mere existence can function as symbolic capital once established.

It can be said that the intrusion of both 'civilians' and 'scientists' could be considered undesirable from a military viewpoint. Even at that, however, 'scientists' was conceived as a way to lessen the perceived offense. Ambiguity in naming oneself or choosing among multiple names is not an uncommon practice of symbolic leverage.⁴⁷ OSDR had designated all its personnel as *scientists*, as the military had little respect for *engineers*. Ironically, dubbing engineering as science was to end up as a social misconception in the aftermath of these events.⁴⁸ Some disliking also came from executive officials who thought, before the OSRD had been established, that this was "a plot to fatten up the universities at government expense."⁴⁹ Contrary to this was a Congress that was helpful in securing funds. In fact, to avoid making the effort for the atomic bomb go public, Bush and Secretary of War Harry Stimson would meet with Congressional leaders when money was needed for the Manhattan District to whom secret information would be divulged, and the money would be appropriated without being opened to public discussion.⁵⁰ From an executive perspective, the NDRC and OSRD had incredible latitude in how the money was spent once it was appropriated by Congress. The NDRC, legally established by the National Defense Council, already had much autonomy in the personality of Bush. The OSRD, on the other hand, was tied to the Executive Office of the President and Bush was

⁴⁵ Stewart, *Organizing Scientific Research*, 50.

⁴⁶ Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (New Jersey: Princeton University Press, 1990) 16.

⁴⁷ Bourdieu, "The Social Space and the Genesis of Groups," 733.

⁴⁸ Bush, *Pieces of the Action*, 54.

⁴⁹ Ibid, 38.

⁵⁰ Ibid, 40, 133.

solely responsible to the Commander-in-Chief. Accordingly, many executive officials become ineffective position-wise. This is partially the reason behind their distaste. On the other hand, it should be considered that the structure of the bureaucratic field does not foresee the federal government making use of university-based science; so, a shift from in-house to extramural research was conceptually baffling and, unsurprisingly, disliked. From the perspective of Congress, in which the power of the purse resides,⁵¹ a similar conclusion might have been expected at face value. First, Congress's immediate relation, i.e. the appropriation of budgets, already gave it effective control over the expenditure—something which executive officials had some or no control over in the form of budgetary proposals. Second, the prestige of Secretary Stimson, a Republican who served in both the Republican and Democratic Parties, was a mediating factor. Third, the secrets *shared in confidence* with Congressional leaders can have a function in *establishing confidence*, making cooperation easier. Finally, and regarding the Manhattan District, Congressional hierarchy and the wartime emergency made it possible that such large sums were appropriated without the full democratic process.

A demonstration of Bush's symbolic power is perhaps best seen in his punishment of Navy Admiral Harold G. Bowen Sr. Like many fellow officers, Admiral Bowen considered the NDRC as a "conspiracy" to subsidize universities. When Frank Jewett wanted to extend civilian involvement into antisubmarine research, Admiral Bowen, as technical aid to the Secretary of the Navy, attempted to block this intrusion, which eventually drew Bush into the conflict in order to help his colleagues. He was also aware that, if others were to respect the civilian scientists' involvement and cause less trouble in the future, Bowen was to be made an example of. Accordingly, Bush successfully pushed for the establishment of the Office of the Coordinator of Research and Development (OCD), which would coordinate the effort, used his influence for the picking of his choice to lead

⁵¹ The Constitution of the United States, Article I, Section 9, Clause 7. <https://constitution.congress.gov/browse/article-1/section-9/clause-7>.

it, and effectively handed Bowen over to his rivals in the Navy by having his position changed.⁵² In the words of Harvey Sapolsky:

The disciplining of the Admiral was a clear indication of the support scientists had at the highest levels of government. Although military officers might continue to resent their participation as equals in the planning of weapon research, they could not openly resist such participation. For the duration of the war, at least, scientists had achieved the autonomy they had sought.⁵³

This was not even the end of it, as several months later, when the Manhattan District had become quite large and was to be transferred to the Department of War, under which it would be transferred to either the Army or the Navy. Bush's choice of transferring it to the Army reflected several factors, of course. The Navy's lack of respect to civilian scientists and capability for cooperation, however, was not forgotten and was influential on this decision, as confirmed later in an interview to Sapolsky.⁵⁴ Ultimately, the OSRD, primarily in the guise of Vannevar Bush, had carved out a chunk of authority where it was originally considered the domain of the military. In the words of the man himself,

orders could be given to O.S.R.D. only by the President of the United States, and he never gave any. Collaboration usually occurs only between equals in authority. This does not mean that O.S.R.D. was accepted into the top military councils where strategy was determined. It was not . . . But the agreement that was reached on this matter moved us a long way toward mutual respect, out of which only can arise genuine concert of effort in a common cause.⁵⁵

Organization and operation. After its establishment, the Director was authorized by the President to be able to reorganize the OSRD without his approval. He was also vested

⁵² Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (New Jersey: Princeton University Press, 1990), 16-18, 26.

⁵³ Ibid, 18.

⁵⁴ Ibid, 18-19.

⁵⁵ Bush, *Pieces of the Action*, 82.

with some impressive powers contained in the Second War Powers Act of 1942, such as to “acquire, use, and dispose of any real property, temporary use thereof, or other interest therein, together with any personal property located thereon, or used therewith, which the Office of Scientific Research and Development shall deem necessary for military, naval, and other war purposes.”⁵⁶ The chairs of the NDRC and the Committee on Medical Research (CMR), as well as Division Directors, had similar powers Bush had internal to the OSRD. Bureaucratically speaking, these made the OSRD powerful externally but significantly decentralized, i.e. weak, internally. Administratively, however, there was strong uniformity as Irvin Stewart was the executive secretary of the OSRD, NDRC, and CMR. The OSRD was not a bureaucratic machine and rather reflected the structure of the scientific field in its conduct of research insofar as possible in a wartime bureaucratic context.⁵⁷ An exception to this were the oaths and pledges of secrecy and the following compartmentalization due to security concerns. Security clearances did cause some serious delays, yet the operations were still remarkably quick in consideration of such limitations.⁵⁸

Achievements and impact. The operational impact of the scientists would come to have a role in basic research ideology in two ways—one directly and the other indirectly: Those in which basic science was utilized directly fed into its arguments, whereas the applied achievements simply added to their prestige and broadly to all of their arguments. The impression these left were varying, of course, as not only actual utility, but also whether they could be expressed in numerical terms or were spectacular.

Requests by the Navy and the PHS on medical research were initially limited to traumatic shock, war injuries, and infectious diseases. It did not, however, take much time to realize the breadth of the required research.⁵⁹ Soon the CMR was conducting research on issues

⁵⁶ Stewart, *Organizing Scientific Research*, 40-41.

⁵⁷ Ibid, 40-48.

⁵⁸ Ibid, 27-30.

⁵⁹ Sanford V. Larkey, “The Division of Medical Sciences of the National Research Council and National Defense,” *Science* 93, no. 2411 (March 14, 1941): 241-242.

such as tropical diseases, as this was a field significantly overlooked in the USS, and flying at high altitudes, necessitated as existing technology was insufficient in comparison with the capacity of aircraft. The most important of these, perhaps, was the development of synthetic penicillin and its mass production for the first time.⁶⁰ Through these efforts, the disease related death rate in the Army was lowered to 0.6 per thousand soldiers—the lowest in US Army history.⁶¹ This was 14.1 in World War I, during which the Army forces were considerably less extended.⁶²

Furthermore, the OSRD, through its Office of Field Service, lent its expertise to the military on the field in activities involving science and technology. It had sent about 300 scientific and technical personnel overseas and 200 in the US. By the end of the war, it had even sent 60 scientists into enemy territory.⁶³ An impressive episode is the following:

Perhaps the single most important and certainly the most colorful OFS project in the European theater was the ALSOS Mission. This was a joint Army-Navy-OSRD activity that involved sending a group of outstanding physicists, chemists, metallurgists, engineers and other scientists into the territory recently won and close on the heels of or even along with the advancing armies. Its primary object was to secure an immediate over-all picture of German scientific research in the war effort and especially to find out with the greatest possible speed what progress German scientists had made in the critical field of atomic energy.⁶⁴

Also quite important were the scientific techniques and devices developed against the U-boats and V-1 bomb attacks, demonstrating some of the cutting edge developments during the war.⁶⁵ The U-boats were arguably the decisive component of the war. As stated by Robert Millikan after WWI, one of the country's most influential scientists at the time and

⁶⁰ Stewart, *Organizing Scientific Research*, 102-107.

⁶¹ "Recent Contributions of the Preventive Medicine Service of the U. S. Army," *Science* 100, no. 2586 (July 21, 1944): 45.

⁶² Bush, *The Endless Frontier*, 46-47.

⁶³ Stewart, *Organizing Scientific Research*, 128-130, 134-135.

⁶⁴ *Ibid*, 134.

⁶⁵ Bush, *The Endless Frontier*, 12.

involved in antisubmarine warfare, submarines were *the* problem of the war and it was recognized to be a problem of physics before being one of engineering.⁶⁶ In the words of Bush, “The United States came very close—too close—to being defeated in each world war by the submarine.”⁶⁷ At the time, he even regarded that the atomic bomb could have been the only recourse had the submarine warfare had been lost to Nazi Germany.⁶⁸

Early atomic research was transferred to the NDRC and OSRD, at the behest of Bush, which was welcome as it enjoyed better funding. However, the program grew much more complex and expensive, and once circumstances revealed that developmental work could begin, it was transferred to the Department of War since they would have had an easier time getting the necessary money and the Army Corps of Engineers was better suited to carry out such large in-house development work. Besides being instrumental in the creation of the Manhattan District, the OSRD leadership continued to be partially involved and some members were personally active even after its complete transfer. Even though some scientists did not enjoy working under the aegis of a military organization, the planned program was accomplished and created the most “spectacular” achievement of the war.⁶⁹

⁶⁶ R. A. Millikan, “The New Opportunity in Science,” lecture given before the summer session of the University of Chicago, Chicago, July 1919, reprinted in *Science* 50, no. 1291 (Sept. 26, 1919): 288.

⁶⁷ Bush held the Navy responsible for this near defeat. See: *Pieces of the Action*, 70, 75-77.

⁶⁸ Bush: “As the depredations of the U-boats mounted early in 1943 there was no doubt in my mind that we were headed for catastrophe. It was clear enough that, if U-boat success continued to climb, England could be starved out, the United States could mount no overseas attack on the Nazi power, Russia certainly could not resist alone. We would be forced into a situation where only the advent of an A-bomb could alter the trend toward world conquest by the dictatorships of Germany and Japan, and as far as we then knew, the development of that weapon might go either way.” Ibid, 88.

⁶⁹ Stewart, *Organizing Scientific Research*, 120-123; Bush, *Pieces of the Action*, 58-62. Bush also represented the United States in negotiating the atomic exchange provisions with the United Kingdom, which made him a target of Churchill in a letter to Roosevelt. See: 35-36.

The origin of the problem of scientific and technical labor. As wartime conditions showed that nothing less than a nation-wide mobilization was needed,⁷⁰ as was the case for many a university, the *Harvard Alumni Bulletin* announced that the Harvard “faculty has accepted the duty of leading the university to war.”⁷¹ The unforeseen amounts of required labor was putting stress on the nation’s scientific and technical labor,⁷² which, in turn, was putting pressure on educational institutions, as more and more people were being called for necessary research. Even though the scientific leadership tried to avoid such disturbance, this was sometimes made untenable by circumstances. Incomplete conservative estimates pointed out that, as early as March 1941, 25% of all US physicists were involved in the war effort.⁷³ Conant, making a study based on the leading scientists in *American Men of Science*, concluded in October 15, 1941 that 52% of the country’s leading chemists and 78% of such physicists were working on defense work under the OSRD, industry, or other government agencies. The institutions that the OSRD had made contracts with already comprised of 98% of the nation’s physicists.⁷⁴ By the end of the war, the US was also the only allied power to indiscriminately draft all able-bodies on campus.⁷⁵ This issue, which would come to be referred as the “shortage of scientific of technical manpower,” would have much impact in the ensuing years due to the amount of

⁷⁰ For an early list of the fields of science of which the personnel situation is relevant to the defense program, see: Jewett, Frank B., and Isaiah Bowman. “Deferment from Military Service of Scientific Men.” *Science* 92, no. 2400 (December 27, 1940): 608. To the unfamiliar eye, this list can demonstrate the relevance of basic research to defense work.

⁷¹ “The Wartime Service of Harvard University,” *Science* 95, no. 2472 (May 15, 1942): 496-497.

⁷² Just some of the fields experiencing a shortage of experts were the following: “Iron and steel, non-ferrous metals, machine tools, ordnance, aircraft, marine and automotive equipment, railroad repair shops, radio and other electrical equipment, supplies and apparatus, textiles, forest products, paper, printing and publishing, chemicals and allied products, plastics, petroleum and coal products, rubber products, stone, clay and glass products,” “explosives chemists, physicists and chemical engineers,” and medicine. “U. S. Civil Service Examinations,” *Science* 94, no. 2433 (August 15, 1941): 159; “United States Civil Service Examinations,” *Science* 94, no. 2451 (December 19, 1941): 580; “The Shortage of Physicians in the United States,” *Science* 94, no. 2450 (December 12, 1941): 555.

⁷³ “Physicists and National Defense,” *Science* 93, no. 2412 (March 21, 1941): 275.

⁷⁴ Stewart, *Organizing Scientific Research*, 23-24, 58-59.

⁷⁵ “Appeal for the Deferment of College Science Students,” *Science* 102, no. 2655 (November 16, 1945): 500-501.

potential labor uprooted from their institutions, prospective or current, and in the ensuing decades as the language would become detached from the WWII experience and attached to later developments.

Pearl Harbor exacerbated all of these.⁷⁶ The requirements for physicists were lowered and the definition was made broader, the call for metallurgists were extended to junior metallurgists, and announcements were many other fields were made.⁷⁷ The Selective Service,⁷⁸ which, despite being successful for the most part, had also inducted several hundreds of chemists, leading to what was described as an “appalling shortage” of chemists and chemical engineers,⁷⁹ where “replacements for any who may be inducted are almost impossible unless other serious vacancies are created.”⁸⁰ So much so that the American Chemical Society was planning to call in retired research directors.⁸¹ Toward the end of the war, Charles Parsons, the Society’s president, wrote to Roosevelt that the US was lagging behind other European countries in protecting its scientists from drafting and that the efforts to curb this had been unsuccessful.⁸²

⁷⁶ Ibid, 262.

⁷⁷ “Scientific Positions under the Government,” *Science* 96, no. 2480 (July 10, 1942): 35; “Civil Service Examinations for Junior Metallurgists,” *Science* 96, no. 2488 (September 4, 1942): 223; “Technical and Scientific Positions with the Government,” *Science* 97, no. 2514 (March 5, 1943): 216. For the last, these were: “chemistry, geology, geophysics, mathematics, metallurgy, meteorology, physics and radio.”

⁷⁸ Many NDRC contractors were also reserve officers for the Army and Navy. Stewart, *Organizing Scientific Research*, 260.

⁷⁹ “The Shortage of Technically Trained Chemists,” *Science* 95, no. 2460 (February 20, 1942): 189.

⁸⁰ Parsons, Charles L. “Statement of Conditions of the American Chemical Society,” *Science* 95, no. 2471 (May 8, 1942): 483-484. The drafting of chemists would continue all the way into 1944. “The Drafting of Scientific Workers,” *Science* 99, no. 2570 (March 31, 1944): 258-259.

⁸¹ “The Effective Use of Scientific Ability,” *Science* 96, no. 2495 (October 23, 1942): 374.

⁸² “Technological Manpower,” *Science* 101, no. 2628 (May 11, 1945): 488.

In effect, “a contest was developing between the needs of the army, navy, air and signal corps, and the requirements for industrial research, design, production and maintenance.”⁸³ Some temporary deferment was made possible by a committee of the War Manpower Commission, but the progress of the problem necessitated a more proactive response. The OSRD created the Scientific Personnel Office, as the competition between industrial and governmental bodies devolved into one between OSRD contractors, and not even for first class scientists. For the overall coordination of scientific and technical labor, the OSRD facilitated the creation of the Committee on Scientific Research Personnel under the War Manpower Commission. The OSRD was particularly sensitive to the drafting of young scientists, as many older ones were not familiar with the cutting edge research conducted by it. Ultra-high-frequency radars were a “young man’s game,” for example, and Karl Compton intervened to prevent the drafting of 50 scientists from the MIT Radiation Lab.⁸⁴ Bush was particularly faced with opponents in the military who “could not understand how any sane man could struggle to keep young men out of the Army,” whereas Bush himself “could not understand how [they] could miss the fact that the whole practice of warfare was being revised by the laboratories.”⁸⁵

The problem of scientific and technical labor comprised three layers of urgency: (1) the insufficiency of the existing resources, (2) the lowering of standards (training and experience) to broaden coverage, and (3) emergency training programs to supply and replenish resources. The last of these further demonstrates the level of this emergency, as even the large efforts to redress the problem were insufficient. Since these efforts were focused on the specific scientific and technical needs, the educational impact was all the more visible in excluded areas. “On all sides, fundamental research, except as it relates to the demands

⁸³ R. L. Sackett, “The Supply of Technical Men to the Armed Forces and to Industry,” *Science* 96, no. 2503 (December 18, 1942): 553.

⁸⁴ Stewart, *Organizing Scientific Research*, 256-257, 259, 264-265, 271-275.

⁸⁵ Bush, *Pieces of the Action*, 288-290. Secretary Stimson gave personal assurance of assistance to Bush, but he, thinking that such interference might also draw the ire of some military men, tried to take care of these matter by himself.

of war, is being curtailed or abandoned, as scientists, technicians and students are mobilized for practical types of service,” and “liberal education has been discarded for the duration. Our universities are now instrumentalities of total war.”⁸⁶

Both Congress and the administration were appropriating large sums to this end.⁸⁷ The higher education system was flowing with intensive courses for postgraduate military training,⁸⁸ natural and mathematical sciences,⁸⁹ engineering in a verity of fields,⁹⁰ medicine in general and industrial medicine in particular,⁹¹ tropical disease,⁹² psychology,⁹³ and meteorology for the Air Corps.⁹⁴ High schools were also being asked to support the

⁸⁶ “The Work of the Rockefeller Foundation,” *Science* 97, no. 2521 (April 23, 1943): 380.

⁸⁷ In addition to the \$9 million by Congress, President Roosevelt appropriated a whopping \$116 million. “Engineering Defense Training,” *Science* 92, no. 2400 (December 27, 1940): 598; “Defense Training Courses,” *Science* 94, no. 2432 (August 8, 1941): 133-134.

⁸⁸ Compton, Karl T. “The Massachusetts Institute of Technology and the National Defense Program,” from the annual report of President Karl T. Compton to the Corporation of MIT, reprinted in *Science* 92, no. 2390 (October 18, 1940): 359.

⁸⁹ “Chemistry in College-Grade Defense Training,” *Science* 94, 2439 (September 26, 1941): 294-295; “Summer Session in Applied Mathematics at Brown University,” *Science* 97, no. 2520 (April 16, 1943): 348.

⁹⁰ “Emergency Courses to Train Engineers and Technicians,” *Science* 92, no. 2397 (December 6, 1940): 523; “Training at the New York University for Squadron Engineering Officers for the Air Crops,” *Science* 93, no. 2402 (January 10, 1941): 33-34; “A New Course in Aircraft Production at New York University,” *Science* 93, no. 2403 (January 17, 1941): 54-55; “Junior Engineering Students and the War Industries,” *Science* 95, no. 2465 (March 27, 1942): 320; “Training for Women in Aeronautical Engineering at the University of Cincinnati,” *Science* 97, no. 2529 (June 18, 1943): 548-549.

⁹¹ “War Emergency Courses in the University,” *Science* 96, no. 2500 (November 27, 1942): 488; “Postgraduate Course in Industrial Medicine at the Long Island College of Medicine,” *Science* 96, no. 2494 (October 16, 1942): 353; “The Training of Workers for the War Industries,” *Science* 96, no. 2499 (November 20, 1942): 464; P. Morales Otero, “Present Teaching Activities of the School of Tropical Medicine, in Cooperation with the Insular Department of Health,” *Science* 99, no. 2580 (June 9, 1944): 473-474.

⁹² “The Teaching of Tropical Medicine in Medical Schools,” *Science* 96, no. 2502 (December 11, 1942): 532.

⁹³ Henry E. Garrett, “Psychology and the War,” *Science* 96, no. 2499 (November 20, 1942): 465.

⁹⁴ “The War-Training Center of New York University,” *Science* 97, no. 2509 (January 29, 1943): 111; “Meteorological Training for the Army Air Forces,” *Science* 97, no. 2515 (February 19, 1943): 179.

training of technical and scientific labor,⁹⁵ and some PhDs in humanities even returned to college as freshman in order to become physics and math teachers to do this.⁹⁶

Demobilization. The more the end of the war was in sight, the more demobilizations plans accelerated. Bush made several provisions in this direction that would effectively continue many of OSRD's ongoing work as the organization itself would be slowly dismantled. Basic research was to be transferred to permanent organizations to be continued it peacetime, whereas military research would be transferred to the Services as they would form such necessary arrangements. OSRD's partial demobilization would follow Germany's collapse, and then Japan's, part by part discontinuing or transferring projects considered to not be of immediate interest to the tasks at hand.⁹⁷ "[W]e have now won the battle of laboratories," remarked President Truman. His predecessor Roosevelt had passed away before seeing the results of the Manhattan Project and President Truman, succeeding him as vice president, only learned of it after taking office. The rest of the world learned of it after mushroom clouds floated over Hiroshima and Nagasaki. Truman remarked: "We have spent \$2,000,000,000 on the greatest scientific gamble in history and won."⁹⁸

For all its instrumentality, Vannevar Bush construed the OSRD as "too autocratic" for peacetime science and was determined that it not be perpetuated once all of its affairs were in order.⁹⁹ A similar sentiment was prevalent among scientists who worked for the war effort, epitomized in the popular song "Take Away Your Billion Dollars," a song underlining the purity of science and scientists' dislike toward working with the military.¹⁰⁰ The

⁹⁵ Glen W. Warner, "War Work in the High Schools," *Science* 97, no. 2516 (March 19, 1943): 262-263.

⁹⁶ "War Emergency Courses in the University," 488-489.

⁹⁷ Stewart, *Organizing Scientific Research*, 300-301.

⁹⁸ Harry S. Truman, "Statement by the President Announcing the Use of the A-Bomb at Hiroshima" (August 6, 1945), reprinted as "The Report of President Truman on the Atomic Bomb" in *Science* 102, 2642 (August 17, 1945): 164.

⁹⁹ England, *A Patron for Pure Science*, 11.

¹⁰⁰ Sapolsky, *Science and the Navy*, 35.

song's title would prove ironic, however, that once the conditions for freedom in scientific research seemed possible, certainly having caveats of secrecy where applicable, scientists would not turn their backs on federal monies.

A central concern of social science has been taking change as a social problem. Or to phrase it in the words of Crozier and Friedberg: “[U]nder what conditions do tensions which have hitherto served to reinforce the existing system reach a level at which they begin to provoke its disintegration?”¹⁰¹ As the authors have pointed out, crises actually trigger innovative mechanisms in a minority of cases. One precondition for social change is change in existing power relations, which can also be considered as a form of *collective learning*—i.e. the production of new game models (or changing the field through new position-takings à la Bourdieu) along with their emotional, rational, and relational components.¹⁰² Such a change of power relations took place in WWII—circumstantially beyond belief, symbolically very significant, and structurally not as much yet still substantial. The atomic bomb made this recognizable to American society in its entirety, but the military and the government at its higher echelons were also aware of those accomplishments overshadowed by the bomb. The immediate involvement of civilian scientists resulted in new organizations and relevant roles for research in the Services, carrying in the habitus of some scientists who decided to remain and, along with them, their practices of the scientific field. Vannevar Bush and his colleagues, with executive support, overcame organizational rivals, which concluded in a game where their immediate military associates and the top military brass had recognized the symbolic authority of the scientists. Military officers were praising ‘science,’ now a misnomer for technology¹⁰³—ironic, given that the OSRD had designated its personnel as *scientists* due to the lack of military

¹⁰¹ Crozier and Friedberg, *Actors and Systems*, 215.

¹⁰² Ibid, 215, 222-223, 249.

¹⁰³ Harvey M. Sapolsky, “Academic Science and the Military: The Years Since the Second World War,” in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C.: Smithsonian Institution Press, 1979), 381.

respect toward *engineers*, and arguably cementing the engineering's inferior status to science.¹⁰⁴ Bush emerged as the leading figure in this effort. To fill the gap of coordinating military research and development after dismantling the OSRD, the Joint Research and Development Board was created by the Secretaries of War and Navy, to which Bush became chairperson.¹⁰⁵ Such organizational leadership was not, however, the centerfold of Bush's plans, for real change came in changing the rules of the game. Scientists enjoyed much symbolic capital in the 'field of power' and had an opportunity in genuine change through policy-making. As Robert Merton puts it, the earlier a scientist is involved in policy-making, the more influential.¹⁰⁶ At this point, Bush emerged as the single most influential figure, along with his close colleagues, in deliberations involving science. He did, however, lack something he had during his leadership—the social power derived from his boss and friend, President Roosevelt.

¹⁰⁴ As previously mentioned in Bush, *Pieces of the Action*, 54.

¹⁰⁵ Stewart, *Organizing Scientific Research*, 50.

¹⁰⁶ Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Illinois: Free Press, 1949), 169-171; Schooler, Jr., *Science, Scientists, and Public Policy*, 57.

CHAPTER 4: FRAMING THE DEBATES: *SCIENCE—THE ENDLESS FRONTIER*

4.1 Historical Intermezzo: Contrasting Earlier Cases

The National Research Fund and the botched industrial patronage of pure science.

There are two cases prior to WWII that prove useful in comparison. The first is the National Research Fund which, during the interwar era, represents a somewhat contrasting example of what the WWII scientific leadership would later attempt and is useful in demonstrating possible reasons why they went to the federal government for support. George Ellery Hale, Robert Millikan, and Elihu Root were all “diehard Republicans” who were wary of federal involvement in science. According to Geiger, the scientific establishment in this era had three pillars: authority in “best-science,” reliance on private capital, and an arrangement in which these familiar faces would be positioned and encounter each other in the different environments relating to science, such as the National Research Council, large foundations, academic associations, etc.¹

In the 1910s, George Ellery Hale led an effort to bring the NAS-NRC closer to industry in order to solicit support for basic research. Universities were best suited to conduct basic research since they did not have financial interests in results. The task, then, was to create a source for research funding—something that had to be based on endowments, as previously seen. These efforts led to the creation in 1925 of a bipartisan board overseeing the National Research Endowment—later changed to the National Research Fund. The importance of research, primarily applied, was seen in the increasing amounts of in-house research conducted by industry (1920s). The argument was that, however, applied innovations were made possible by basic research on which they were built. Herbert Hoover, Secretary of Commerce at the time, was the ideal man to campaign this notion to industry.

¹ Geiger, *To Advance Knowledge*, 99-100.

Referring to geographical frontier, now closed, he stated that “the great continent of science is as yet explored only on its borders.”² Despite Secretary Hoover’s personal and tenacious involvement, convincing enough pledges was taking its time. Initially capitalized at \$20 million, the Fund had to reduce its goal to \$10 million by 1928 in order make sure previous donors did not pull back, but also had to make the concession of reducing their previous pledges as well. Secretary Hoover had to pull back from investing time and energy into it due to his presidential nomination, which was in part behind the decision to reduce the goal. When the Fund was able to reach this in 1930 with additional support from the Rockefeller Foundation, it could not call upon all of its pledges. The first year of the Depression had set in and some firms backed away from their pledges. The fund was dead by 1932.³

Davis and Kevles’s analyses put this case into a perspective that has explanatory value for the post-WWII attempt in extending federal support over academic science. This is in relation to the arguably fundamental unreliability in firms supporting basic science. First, it has to be noted that even though the Depression might explain why the Fund could not collect pledges once it achieved its reduced goal, it cannot explain why many firms did not support it before the crisis. Those that did support it do not include railway and automobile companies. These were industries that were not convinced in the long-term returns of pure science—at least not in their industries. There were, however, other firms (such as in the chemical industry) whose interests would be more easily aligned with basic research. Davis and Kevles have argued that the patent laws at the time made such investment less likely. This brought along with it a problem of academic science—it was supposed to be conducted without interference *and* shared similarly so. The Fund’s position on this was giving prior access to any such knowledge, but the ambivalence of patent

² Lance E. Davis and Daniel J. Kevles, “The National Research Fund: A Case Study in the Industrial Support of Academic Science,” *Minerva* 12, no. 2 (April 1974): 212.

³ *Ibid.*, 208-213.

issues in the Constitution and by-laws made this problematic.⁴ Why did, then, some companies make pledges? The authors provide a structural answer and contend that it was likely not due to friendly relationships between the Fund's board members and those of the trustees and executives of the firms.⁵ The aforementioned patent problem is clear, but furthermore, the fundamental knowledge produced by basic research was going to be available to all nevertheless—i.e. a public good.⁶ It was only in monopolies or quasi-monopolies where a free rider problem of this sort could be avoided. This can be seen in the companies that supported the Fund, such as the United States Steel and AT&T, and associations that amount to the same thing, such as NELA and the American Iron and Steel Institute. The increasing belief in the long-term importance of pure science makes this conclusion likelier. The entire problem would be redefined, however, if pure science was supported by the federal government, in which there would be no concerns of free riders (or effectively none). This is specifically what Davis and Kevles argue, to which they cite the support by the National Association of Manufacturers given to Bush's agenda for federal patronage.⁷

The interwar episode of the struggle for federal patronage. As previously mentioned, academic research was predicated on endowments. Private support, however, became less likely due to both the Depression and the New Deal taxes. The temporary Science Advisory Board, created by President Roosevelt in 1933 at the suggestion of NRC chairperson Isaiah Bowman, was established to provide scientific suggestion for the New Deal programs. The Board represented the “old guard” scientific leadership who was suspicious of federal involvement. Karl Compton, pursuing his agenda of a “New Deal for science,”

⁴ It is actually quite a rare phenomenon that basic research can produce patentable knowledge. But this had not been put forward yet and had to be settled in future public discussions.

⁵ These relationships are not sufficient, as suggested by the authors' arguments, but they might be necessary (in varying degrees). The authors do not, and likely cannot, falsify this categorically.

⁶ Theodore H. Cohn, *Global Political Economy*, 6th ed. (Illinois: Pearson, 2012), 64, 121-122.

⁷ Ibid, 213-219.

overcame the conservative scientists as chairperson of the Board and convinced President Roosevelt to support non-governmental science. This was the first high-level attempt for federal patronage. Compton's plans, however, were effectively blocked by the social scientists in the National Resources Board. This second cleavage was due to the fact that the NAS and NRC was essentially clubs of natural scientists. Their perspectives and proposals reflected their dislike for the social sciences and, being *the* organization for advising the government on scientific matters, any program administered by them was likely to reflect such opinions. Primarily led by the economist Wesley Mitchell and the political scientist Charles Merriam, the social scientists responded to President Roosevelt's request of advice on the matter and argued to kill the program as it was conceived by Compton. Successive session in Congress failed to agree on a federal policy of science. This materialized in the conflict between the natural scientists, who used their influence over Congress through the NAS, and the social scientists, who used their influence over a Democratic White House through the National Resources Board.⁸

The conservatives (including Southern Democrats) in Congress shared similar viewpoints with the conservative scientists, and the same applied the liberals in Congress and the social scientists. However, it must be noted that these similar concerns were also refracted through the participants' respective fields. The social sciences were integral elements of the New Deal, and not only would investment in these be a win for the New Deal, it would also be a form of federal waste the Congressional Republicans detested so much. The natural scientists, on the other hand, proposed support for *basic research*. This was problematic for the social scientists since it, for the most part, their work fell into applied research—this was all the more problematic for the New Deal programs, since related social scientific studies were completely of an applied nature.

In the lack of a forceful imperative, a stalemate occurred and deliberations on national science policy were left unsettled until WWII. Interestingly, this was despite the fact that the Democrats were in control of the Senate and the House for this entire duration (the

⁸ Geiger, *To Advance Knowledge*, 255-259; Geiger, *American Higher Education*, 531.

73rd, 74th, and 75th Congress). The Natural Resources Board was enlarged into the National Resources Committee and prepared *Research—A National Resource*. Supporting Compton's overall idea, this proposal however (1) left no discipline out and (2) omitted some facts underlying the superiority of select research universities, suggesting a more democratic distribution of resources—an idea fundamentally at odds with “best-science” practices. The report did not have any immediate effects but became an important source of reference for the report that would: *Science—The Endless Frontier*.⁹

4.2 Science—*The Endless Frontier*

The OSRD experience was one of success, but also one that demonstrated the unpreparedness in a national emergency. As Irvin Stewart put it, “the principal conclusion from its experience is that there should never be need for another like it.”¹⁰ Therefore, the necessary federal arrangements had to be made so that it did not come to this. Interestingly, however, this direct conclusion was subordinated to the basic research agenda that was rather predicated on making up the scientific infrastructure for said arrangements. The deed was too large to be undertaken by any other organizational body, so the federal government was called upon. “It has been basic United States policy that Government should foster the opening of new frontiers,” said Vannevar Bush.¹¹ Unlike Herbert Hoover, he did not exclude the government from his allusion. His report, *Science—The Endless Frontier*, was to become the basis of the prospective partnership between the government and the universities.¹²

⁹ Geiger, *To Advance Knowledge*, 260-263; Geiger, *American Higher Education*, 531. For the people and institutions involved in *Research—A National Resource* and a summary of its specific recommendations, see: “Scientific Research and the Federal Government,” *Science* 89, no. 2302 (February 10, 1939): 121-122. For FDR's reception of the report, see: “Scientific Research Under the Federal Government,” *Science* 89, no. 2300 (January 27, 1939): 75.

¹⁰ Stewart, *Organizing Scientific Research*, 332.

¹¹ Bush, *The Endless Frontier*, +6.

¹² Vest, *The American Research University*, 40.

The man—Vannevar Bush. As the OSRD was dismantled and efforts at developing a national science policy moved forward, scientists from around the country were all ears. At the center of this agenda was the scientific leadership as it took form during WWII.¹³ Among them, however, Bush was distinguished as the leader and derived the most authority from their distinction. Bush did not make an effort to position him as such, which had to do with his structural position, but he certainly did make use of it to forward his agenda. Accordingly, a look at the man is warranted by the influence he had had on science policy. Bush, like most of his fellow natural scientists, was a conservative but did have a more nuanced understanding of social issues that invalidates attempts to situate him as a stereotypical conservative. He was anti-New Deal but was also against market domination by big business, had a positive opinion on the welfare state, and had sympathy for working class grievances.¹⁴ A true democrat, he also criticized the electoral college as being absurd.¹⁵ As an engineer, he was critical of the lack of humanities and social sciences education in the practical arts, and criticized his own engineering education for leaving his cohorts ignorant in international affairs.¹⁶ Accordingly, his intellectual versatility must be recognized in the tensions that would occur between the natural and the social sciences.¹⁷

¹³ This point must be emphasized. All members of the OSRD scientific leadership were prominent members in the scientific community before the war of course. However, most were not at the forefront as national representatives of it. Such representation was, of course, not by volition but by recognition. The NAS-NRC was not excluded, of course. It must be recalled that Frank Jewett, president of the NAS, was a “founding member” of the OSRD. The important difference was that most of those at the forefront did not come from a leadership position in the NAS-NRC.

¹⁴ Davis and Kevles, “The National Research Fund,” 13-14; Bush, *Pieces of the Action*, 3, 242-243.

¹⁵ Ibid, 310.

¹⁶ Ibid, 246, 261.

¹⁷ For example: “Alongside the course in the mathematics of electric circuits we need a course in the history of ideas. And we need that balance wherever older minds seek to help younger minds on the way of life. What is true in the college classroom is true in the kindergarten as well. Every teacher, no matter his subject, needs to remember that he is preparing most of his students for a life marked by the great duality, one part as a tiny element in a complex social structure, the other in informal relations with his fellows. As he remembers this, and puts it to work in all he does, as he leads his student to be useful, but also to find joy in life, he is a great teacher.” Ibid, 270.

In 1939, after 18 years of his predecessor's presidency, Bush was appointed as the President of the Carnegie Institution of Washington, effectively becoming responsible for one of the largest non-governmental research budgets in existence.¹⁸ He then became the Chair of the NACA. These were timely and fortunate events. Bush's colleagues—Compton, Conant, Jewett, and Richards—were all distinguished members, but Bush was not only in Washington, he was also *close to Washington*.¹⁹ Besides being physically located there, he had some relations with the White House, appropriations committees in Congress, and had learned some of the ins and outs of Washington politics. Accordingly, Bush was in the best position to organize the efforts and communicate them to the White House, effectively becoming the leader.²⁰

As Director of the OSRD, Bush was uninvolved with its internal affairs, unless deemed absolutely necessary. His primary role was taking care of the Organization's relationship with the President, Congress, and the military—i.e. making sure that the money was flowing in and that OSRD scientists were not being hindered by indignant officers. Bush had become the President's informal science adviser—effectively, the first presidential science adviser—through which he derived support. President Roosevelt had not only professional but also personal respect for the man, which had much political value when it came to trust.²¹ As Bush would put it later,

Like many a man from New England, I had snorted at the New Deal, and I had been appalled at some of F.D.R.'s political theory and practice. Together with almost every man I worked with, in uniform or out, when war came I took the attitude that loyalty to the

¹⁸ “Dr. Vannevar Bush, President of the Carnegie Institution of Washington,” *Science* 89, no. 2298 (January 13, 1939): 30.

¹⁹ Jewett also had connections to Washington, naturally, since he was the President of the NAS. He was also the President of the Bell Labs, however, which was in New York. Also considering the rather passive governmental presence of the NAS, it is understandable why Jewett was not as actively involved in Washington. Perhaps Jewett could have held more leadership through the NAS if there was time for a deliberate effort, but the perceived urgency seems to have made Bush the most logical liaison.

²⁰ Bush, *Pieces of the Action*, 34.

²¹ *Ibid*, 40, 50; Stewart, *Organizing Scientific Research*, 50.

chief must be absolute and untarnished. But there was more than this. He gave me a tough job to do, never interfered with me, and always backed me up in a pinch. What more could one ask? Loyalty of a formal sort gradually gave way to something far deeper and lasting.²²

Despite losing his strongest ally, Bush was positioned as the spokesperson of the scientists' agenda for federal patronage. As Bourdieu phrases it, this "oracle effect" allows a spokesperson to pass his words as those of the people he represents.²³ His language was authorized through the delegation of institutions, which, in this case, was historically leading the country's scientists into war and effectively President Roosevelt's request to provide him with a report detailing his recommendations. The fact that President Truman later did not receive this report well and commissioned another one did not change this. The incidents that caused some cracks in this "oracle effect" was not caused by the administration per se, but by the cleavages that occurred within the scientific community once Bush's agenda seemed to hinder some concessions that other scientists might want to give.

Origin. Postwar science was a common concern among scientists and such discussions were taking place in conferences and written pieces. It was also the case for the OSRD leadership. To this effect, Oscar Cox, the administration lawyer, arranged for President Roosevelt to write a letter to be sent to Bush, requesting advice on how to use science to improve national health, living standards, and scientific education in the time of piece. Its orientation reflected President Roosevelt's political agenda, of course, but the advent did not catch Bush off-guard, as the content of the letter was based on talks made with OSRD officials. This form of federalism disturbed the old guard, as was seen in Millikan's letter

²² Bush, *Pieces of the Action*, 45.

²³ Bourdieu and Wacquant, *Reflexive Sociology*, 147.

to Bush, who did not understand how Bush could support federal subsidization as a conservative. However, it was also the case that Bush's report represented a conservative reaction to Senator Harley Kilgore's proposal on science policy.²⁴

Committees. As Bourdieu reminds us, an objectivist theory must consider the contribution that agents make in constructing the social world in order to impose their view.²⁵ Accordingly, it must be remembered that even though the main body of *The Endless Frontier* was written by Bush, his many specific recommendations and operationalizations were derived from or directly taken from the reports prepared by the committees.²⁶ The chairs to these committees were selected by Bush, who selected members for their own committees in turn. There is remarkable uniformity in the views expressed (and the consensuses cited by some) despite the large number of people involved, either through membership in the committee or communication sought by them.²⁷ This ceases to be remarkable, however, when considering that these men (in varying degrees) shared in their membership to the scientific field, to the elite research institutions,²⁸ the mobilization experience from WWII, and some even from WWI. Disagreements, such as those voiced by some conservative scientists against federal support,²⁹ did not stand in the way of unanimous positions being adopted.

The argument. President Roosevelt's request of advice had four points: (1) the quick dissemination of wartime research (unless considered a national security risk), (2) improving

²⁴ Bush, *The Endless Frontier*, VII-VIII; Davis and Kevles, "The National Research Fund," 16; England, *A Patron for Pure Science*, 10, 15, 108.

²⁵ Bourdieu, "The Social Space and the Genesis of Groups," 727.

²⁶ These were the Medical Advisory Committee, Committee on Science and the Public Welfare, Committee of Discovery and Development of Scientific Talent, and Committee on Publication of Scientific Information.

²⁷ For members of the committees, see: Bush, *The Endless Frontier*, 37-39.

²⁸ The membership better reflected this. However, it must be noted that the Medical Advisory Committee contact almost all medical schools in the country, whereas the Committee on Science and the Public Welfare contacted a myriad type of institutions to make a balance of private and public institutions. Ibid, 41; Davis and Kevles, "The National Research Fund," 18.

²⁹ Davis and Kevles, "The National Research Fund," 18-19.

medicine, (3) reconsidering the government's role in research, and (4) the discovery of talent.³⁰ In view of these, Bush limited the scope of the study accordingly:

It is clear from President Roosevelt's letter that in speaking of science he had in mind the natural sciences, including biology and medicine, and I have so interpreted his questions. Progress in other fields, such as the social sciences and the humanities, is likewise important; but the program for science presented in my report warrants immediate attention.³¹

Economic and historical research are cited as worthy of support as they relate to the proper role of the envisioned organization, but that would be it.³² With these qualifications, the argument, so simple yet so transformative, was summarized as such: "The Federal Government should accept new responsibilities for promoting the creation of new scientific knowledge and the development of scientific talent in our youth."³³

The argument was essentially an economic one.³⁴ Applied research in industry and government had to vigorous for public welfare. However, such applied research and the consecutive development rested on fundamental knowledge—hence the synonym 'fundamental research.' Bush contended that there would be a flow of fundamental knowledge to

³⁰ Bush, *The Endless Frontier*, VII-VIII.

³¹ Ibid, V.

³² Ibid, 111. This is despite the role played by psychology in administering mental, motor, and temperamental tests, and studying sensory and motor functions, and public opinion and propaganda in Latin America. Garrett, "Psychology and the War," 465; C. C. Brigham et al., "Report of the Committee on Classification of Military Personnel Advisory to the Adjutant General's Office," *Science* 93, no. 2424 (June 13, 1941): 572-574; "War Work of the Department of Psychology of Yale University," *Science* 95, no. 2464 (March 20, 1942): 296-297.

³³ Bush, *The Endless Frontier*, 25.

³⁴ There were also references, mingled with political sentiments, to the intrinsic value of science as well. These, however, were like a drop in a bucket. For example: "Moreover, it is part of our democratic creed to affirm the intrinsic cultural and aesthetic worth of man's attempt to advance the frontiers of knowledge and understanding." Ibid, 73.

application as long as basic research was vigorous.³⁵ In this sense, “[b]asic scientific research is scientific capital,” said Bush.³⁶ This type of research, its distinction from applied research, and effectively the relation to expected returns was defined as the following:

Basic research is performed without thought of practical ends. It results in general knowledge and an understanding of nature and its laws. 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.³⁷

Bush cited the striking achievements in public health and argued that they were made possible by knowledge produced from previous basic research. The serendipitous knowledge provided by basic research was exemplified in the discovery of penicillin. Its development into mass production during the war, on the other hand, was owed to the coordinative and financial resources of the government.³⁸ Basic research was also “privileged” by the scientists due to the imbalance created by the pace of increase in applied research, further spurred by the war effort. As the Bowman Committee put it:

it is important to emphasize that there is a perverse law governing research: Under the pressure for immediate results, and unless deliberate policies are set up to guard against this, *applied research invariably drives out pure*. [¶] The moral is clear: It is pure research which deserves and requires special protection and specially assured support.³⁹

Besides this value of basic research, reliance on the basic research of other nations was undesirable due a relation of dependence and untenable due to the postwar circumstances of international science. As Bush would put it, “[i]n the nineteenth century, Yankee mechanical ingenuity, building largely upon the basic discoveries of European scientists,

³⁵ Ibid, 2, 7.

³⁶ Ibid, 2.

³⁷ Ibid, 13. For the distinction made by the Bowman Committee, along with ‘background research,’ see: Ibid, 75-77.

³⁸ Ibid, 8-9, 49.

³⁹ Ibid, 77, 81.

could greatly advance the technical arts. Now the situation is different.”⁴⁰ This was explained more broadly by the Bowman Committee (i.e. Committee on Science and the Public Welfare):

Our national pre-eminence in the fields of applied research and technology should not blind us to the truth that, with respect to pure research—the discovery of fundamental new knowledge and basic scientific principles—America has occupied a secondary place. Our spectacular development of the automobile, the airplane, and radio obscures the fact that they were all based on fundamental discoveries made in nineteenth-century Europe . . . First, the intellectual banks of continental Europe, from which we formerly borrowed, have become bankrupt through the ravages of war. No longer can we count upon those sources for fundamental science. Second, in this modern age, more than ever before, pure research is the pace-maker of technological progress . . . a nation which borrows its basic knowledge will be hopelessly handicapped in the race for innovation.⁴¹

The Bowman Committee reported that it had considered the possibility of industrial support, which did not seem viable (these reflect similar reasons aforementioned in the case of the National Research Fund). Government research facilities, on the other hand, were functionally limited and the science budget was almost completely reserved for applied research. Furthermore, there were no governmental bodies, Congressional committees, or national policy for science.⁴² What was the infrastructure upon which to build a supportive environment for basic research?

The first broad principle of the governmental organization envisioned to support basic research was to emphasize “universities’ position as the chief contributor to pure science.”⁴³ As previously mentioned, universities’ central position to scientific research was not a given and but was consolidated in time, and significantly so through its superior

⁴⁰ Ibid, 14.

⁴¹ Ibid, 72-73.

⁴² Ibid, 6-7, 15, 73.

⁴³ Ibid, 88.

research organization. As put by Robert Gordon Sproul, “[u]niversities . . . are the conservators of the above-average intelligence of the nation.”⁴⁴ The basic research part of this was consolidated here even more: (1) lack of sufficient incentives for corporate research in basic research and (2) overall competitive advantage in comparison to exclusive research institutions made it likelier that top notch basic research would be centralized in the university system. Accordingly, the formula prescribed that scientific capital could be increased through scientists and that scientists through the higher education system.⁴⁵ However, despite their comparative success, none of the universities called upon by the OSRD were able to undertake the desired scale of research by their own financial means. The research accomplished through better financial assistance, as well as a sense of urgency of course, can be seen in a comparison of research expenditures. Universities had spent \$28 million on research in 1938, whereas the OSRD contracts for universities in 1943-1944 amounted to \$90 million. Accordingly, the universities would need much more financial support to realize such unused potential, as well as expensive capital facilities that were required to do research in the first place.⁴⁶ As Geiger observers, Bush’s contention that universities’ research capacities were not fully utilized could have been debated in 1940, but not in 1945 after the war had demonstrated it.⁴⁷ An irony, though not contradicting, was that one of the reasons behind Bush and his colleagues requesting support was the damage that the war had done to the research capacity as it hindered the reproduction of scientists of several cohorts through drafting.

The fact was that, even though not used for basic research, industry had a disproportionately superior operational research capacity, which posed the danger of luring research from universities to industry. To put their research expenditures into perspective, “[t]he highest figures for the university departments . . . [were] approximately the same as the

⁴⁴ Ibid, 136-137.

⁴⁵ Non-profit industrial research institutes were considered supportable if deemed capable for basic research.

⁴⁶ Ibid, 2, 7, 51-52, 81, 102, 117.

⁴⁷ Geiger, *To Advance Knowledge*, 266.

lowest figures for the industrial laboratories and science institutes.”⁴⁸ Furthermore, despite the important gains made in the previous decades, researchers in the second tier of the top 13 research universities were only able to devote ¼ of their time to research.⁴⁹

Vest summarizes the gist of the scientists’ arguments and the unique interest the US government (vis-à-vis others) was to have, or in other words, invent, in the university system, something that has today become generic national policy:

In most countries, the national infrastructure for research and development consists of public and private research laboratories that are largely disconnected from universities. The Bush report, however, proposed that U.S. public and private universities become the national R&D infrastructure. The idea was simple—the federal government would pay for the conduct of research in universities, and these research grants and contracts would enable and directly support the education of graduate students. Thus each federal dollar accomplished two objectives—generating new knowledge and technology, and simultaneously supporting the education of the next generation of scientists, engineers, and doctors.⁵⁰

Integrating science policy and coordinating research bureaus. As previously mentioned, Bush stated the lack of governmental arrangements for research and science policy. To this effect, a permanent Science Advisory Board was suggested in order to have the overall responsibility of advising the legislative and executive branches. Integral to this role was advice on the budgets of agencies involved in scientific research. A second body would be an inter-bureau committee or council, possibly under the Bureau of the Budget, which would coordinate such bureaus.

⁴⁸ Ibid, 119.

⁴⁹ Researchers in medical schools are an exception to this last point. Ibid, 86-87, 120.

⁵⁰ Vest, *The American Research University*, 25.

Conceiving a National Research Foundation. Bush and his colleagues conceived a national foundation to develop and operate the necessary programs prescribed by their contentions. Bush and the Bowman Committee dubbed this the ‘National Research Foundation’ and the Moe Committee⁵¹ the ‘National Scientific Research Foundation.’ The only departure in Bush’s recommendations from any of the committees was whether the envision programs would operate under a single agency. The Medical Advisory Committee had a greater fear of federal domination and put forward the idea that a separate ‘National Foundation for Medical Research’ be founded. Bush had to painstakingly convince them into a unified agency, which rather ended up being mentioned along with the idea of a separate foundation, as the Committee stated that such an arrangement was also acceptable. Even though there might additional political reasons behind Bush’s push (since the creation of more than one agency for similar purposes would be likelier to face resistance in Congress), I believe that the reason was a legitimate concern for having the number of agencies created for science be minimum, since divisions into strict disciplines would retard science.⁵²

Government support of science was to have five principles: (1) stable funding, (2) being run by scientifically trained individuals, (3) supporting outside institutions and not conducting in-house research, (4) guaranteeing internal control to supported institutions, and (5) Foundation responsibility to the presidency and Congress.⁵³ The purpose was defined as the following:

The National Research Foundation should develop and promote a national policy for scientific research and scientific education, should support basic research in nonprofit organizations, should develop scientific talent in American youth by means of scholarships and fellowships, and should by contract and otherwise support long-range research on military matters.⁵⁴

⁵¹ The Committee on Discovery and Development of Scientific Talent.

⁵² Bush, *The Endless Frontier*, 7, 59, 69, 147; England, *A Patron for Pure Science*, 16-17.

⁵³ Ibid, 26-27.

⁵⁴ Ibid, 28.

Members of the Foundation would be nominated by the NAS and appointed by the United States President. These members, in turn, would appoint its Director, who would be its executive officer. The Foundation's organization, at the outset, would consist of the Division of Medical Research, Division of Natural Sciences, Division of National Defense, Division of Scientific Personnel and Education, and Division of Publications and Scientific Collaboration. The Division of National Defense, in addition to scientists, was to include representatives of the Secretaries of War and of Navy. The form of science support was a contract, which would be conducted without bidding (as it was conceived as detrimental to scientific rigor) and would cover the costs of research but not be profitable for the institution. It is important to point out that these contracts were conceived to be made through the institutions and not individual scientists themselves, which would be one of the significant differences in the report and the realized postwar arrangements. The different title of 'contract' and 'grant,' however, does not amount to anything in and of itself, as the definition of the contract in *The Endless Frontier* was effectively a grant anyway.⁵⁵

Contracts for research were not the only form of support. There were several types of block grants. It was suggested that the Foundation give matching fluid grants to institutions—the fluidity of these resided in the decision of allocation of these funds to the institutions (this was another component that turned out to be radically different). Another form of block grant was grants for capital facilities, which might or might not be used commonly between multiple institutions. Finally, grants-in-aid were conceived as discretionary grants to enable less well-to-do institutions to build up research capacity. Another form of support was fellowships. These were to be post-doctoral research fellowships and senior research fellowships similar to sabbaticals, which would possibly be conducive to the basic-applied transition of research.⁵⁶ Besides the internal merit to fellowships, their argument was based on two things. First was the shortage of scientific labor experienced during the war. As Bush would later put it, “[s]cientific personnel became so scarce they

⁵⁵ Ibid, 26-32, 109.

⁵⁶ Ibid, 89-92.

even took in biologists and made radar experts out of them.”⁵⁷ Second, the already-at-hand body of scientists was at risk due to the disruption by the war—in fact, the US suffered more from this than other countries due to its drafting policy.⁵⁸ Even in 1941 the decrease in the country’s PhD was at an all-time high point.⁵⁹ Accordingly, even though the bulk of the concerns centered around graduate education and beyond, it extended down to undergraduate education in the form of scholarships in a concern to spur scientific talent. Even though graduate and post-graduate fellowships were predicated on a ‘best science’ understanding, scholarships, contrary to these, were to be geographically proportionate. This made sense since (1) the achievements of high school graduates could not reflect scientific qualifications as they did of researchers and (2) it would create significant inequality based on the possibly institutionalized educational deficiencies of certain regions. There are two related points that merit attention since they demonstrate the nuance and implications of basic research ideology and are repeated by many other proponents in the coming decades. The first, pointed out by the Moe Committee, is that the concern for trained labor actually encompasses scientific needs, so scholarships and fellowships should not be so much that it takes away talent from other needs. The second is that merit, and not sex, race, color, creed, or need, was to be the basis of scholarships and fellowships. This final point stands in an interesting relation to other components of basic research. The position against ‘discrimination’ is a necessary logical conclusion of the argument of merit. In fact, even though the notion is not explicitly mentioned in the report, the conception of merit is integral to the definition of discrimination. Even though not surprising, given its scope, the report does not make a value based judgment against such discrimination, whereas (1) science itself is mentioned to have intrinsic value that is recognized

⁵⁷ Bush, *Pieces of the Action*, 138-139.

⁵⁸ *Ibid*, 150-151.

⁵⁹ Edward A. Henry, “Doctorates in Science,” *Science* 99, no. 2578 (May 26, 1944): 431-432.

by the democratic creed and (2) the defense-related implications of its program are predicated on the presumption that this is necessary to secure democracy from threats, such as the fascist forces they had fought in the war.⁶⁰

The concern for scientific education extended even below the graduate level. Bush and the Moe Committee thought that a lot of potential was wasted at the high-school level or even earlier than that.⁶¹ The increased efforts of high schools for scientific and mathematical crash courses during the war should be recalled. Accordingly, the report called for more attention to be directed to secondary education but did not spell out explicit policy suggestions as this would exceed its scope.⁶²

Three other roles of the Foundation worthy of mention are those related to the dissemination of knowledge, industrial liaising, and the civilian management of military research. The first includes the promotion of the international flow of scientific information, which considered to be integral to basic research, through international conferences, fellowships, and scientific attachés. The second includes governmental effort also promote the applicability of basic research. The argument was based on the fact that most industrial research capacity was located in a small number companies and that small companies were not in touch with research. As scientific advances did not reach all industries equally, non-profit “research clinics” were to be created for the purpose of acting as a liaison between small companies and cutting edge research. The third was the one most directly related to the OSRD experience, which was could not be achieved later due to the amount of power it would displace (in comparison to other successful components).⁶³ Even though the Foun-

⁶⁰ Bush, *The Endless Frontier*, 3, 18, 73, 132, 148.

⁶¹ *Ibid*, 20-21, 140.

⁶² “There has been a growing feeling among members of collegiate departments devoted to teaching in the sciences that introductory instruction in secondary schools in the sciences has been below a desirable standard. The present war emergency has suddenly turned the spotlight upon this condition.” “The Shortage of Technically Trained Chemists,” 185.

⁶³ This was made clearer later by Bush, in his autobiography-like work, when he mentioned that the civilian scientists would be effectively taking over some of the functions of the Joint Chiefs of Staff. Bush, *Pieces of the Action*, 78-79.

dation was controlled by civilians, it was argued to have authority to initiate military research. This was not construed as a replacement to ongoing military R&D—the Services were to be concerned with improving existing weapons, while civilian scientists would work on long-term weapon development. This was arguably the most dissimilar component of the entire proposed organization.⁶⁴

Calculating risks. The fear of federal domination was a powerful theme in all previous related debates and was particularly strong among the old scientific elite, which reflected the age-old American aversion to governmental control. As the Bowman Committee put it, it was wrong to think that “any program is better than no program.”⁶⁵ Land-grant universities and the state support given to universities demonstrated that governmental domination could be avoided. Of course, this did not invalidate prudence, due to which measures had to be taken against the introduction of paternalism. Besides installing competent and recognized scientists into it, the Foundation was to not have any influence on the internal policy of the universities it entered into contracts with—the only exception was a demand for increased institutional emphasis on research.

Another risk, secondary to that of political domination, was financial. Bush stated that the fear that federal support might drive out other forms of support had to be accepted. It was thought that this could be partially prevented by initiating support modestly and increasing with time (specifically mentioned for medicine). Regardless, the total amount would have been greater, much greater, in the case of federal support, of which the alternative was construed to be insufficient. This was, however, predicated on the condition that the Foundation be able to provide a stable flow of funds.⁶⁶

⁶⁴ Bush, *The Endless Frontier*, 2-3, 13, 16-17, 23, 27-28, 68-69, 101-102, 107-108.

⁶⁵ *Ibid*, 74.

⁶⁶ *Ibid*, 25, 57-58.

Reception by the administration. This was the substance of *Science—The Endless Frontier* in short.⁶⁷ It was not, however, received well by New Deal administrators and President Truman himself. They considered it to fall short of the “democratic purposes a Federal research agency should accomplish.” The President, then, heeded the advice of his officials to forward the report to Congress without comment and to appoint a committee himself to look into the matters.⁶⁸ Besides implicitly reviving the cleavage between natural and social scientists, the report’s best-science approach, derived from the scientific field, was conflicting with democratizing agenda of the Democratic Party. In the following chapter, I turn to such conflicts that arise out of different fields, of which the boundaries were being pushed, and out of different positions informed by the social structure, which would fluctuate due to elections. The ensuing position-takings negotiated the outcome in the light of these.

⁶⁷ The Bowman Committee did not study patent laws as they were notified that Bush would prepare a separate report for this. For relevant remarks by Bush and the Committee, see: Ibid, 3, 16, 103-106, 111. Additional parts that have not been mention include releasing wartime research, Civil Service reforms for scientists, in-service science training for officers, and other minor issues.

⁶⁸ Davis and Kevles, “The National Research Fund,” 23-24. Truman later appointed a committee headed by George F. Zook, which prepared the massive six-volume report *Higher Education for American Democracy*, also known as the Truman Commission Report, which was much more equalitarian. The report, however, covered issues that significantly exceeded the research-centered Bush Report.

CHAPTER 5: CONGRESS AND COMPETING VISIONS FOR THE FUTURE OF SCIENCE POLICY

Despite the initiation of science policy discussions at the administrative and bureaucratic levels, Congress became the primary social space of the competing visions for the future of science policy. This was a result of the overlap of several factors. The cleavage between natural and social scientists had several points to it, but the politically most salient one was the correspondence between the social scientists' agenda and the New Deal. Its implications of 'big vs. small government' and 'direct societal benefits' effectively translated into a political alignment in Congress. Science policy legislation had been put forward explicitly for the first time after the interwar period by Democratic Senator Kilgore, reflecting such implications in its centralized conception of science management. The natural scientists, enjoying the backing of the Roosevelt administration, had effectively responded to this with *Science—The Endless Frontier*. The succession of Harry Truman, however, resulted in a loss of much of the cooperative relations these scientists had with the administration and, in effect, with New Dealesque Democrats. Accordingly, they channeled their efforts to the Senate and the House, which provided ground for political patrons with different views. These included Democrats whose science policy conception resembled the conservative scientists due to having people from the Roosevelt administration who closely worked with them. Additionally, there were Republicans who would naturally tend to support the proposal with the less amount of federal involvement. Congress would have already played a part in science legislation, but the facts that the Democratic administration ceased to be a given ally to the scientists' agenda and that science legislation debates had been already initiated there by Senator Kilgore and meant that Congress was to become the primary social space for conflict. Accordingly, the structure of Congress in particular and of the government in general are the most important determinants that structure the conflict. The OSRD experience and the organizational field of higher education, on the other hand, reveal the most important factors that determine the symbolic capital that were active in the deliberations.

Cohen, March, and Olsen have suggested four variables (each a function of time) that might have an impact on organizational decision-making: a stream of choices, of problems, of energy from participants, and a flow of solutions.¹ In terms of science legislation, some of these resembled previous experiences. The problems surrounding federal support were significant in this regard, with the exception of patents—previous attempts had not gotten so far to have even genuinely debated this matter and National Research Fund was devoid of federal implications. The solutions and the disagreements these revealed were also similar for the most part. Yet the similarities in substance do not say much in regard to their *streams*, which, this time, were very much under the influence of the structure of Congress and the distribution of symbolic power as a result of WWII. These were primarily reflected in the stream of choices (including choices to determine participants) and the stream of energy from participants—as it would be seen, the scientific community, even in its moments of dispute, had never stood in political unity as much as they had now.

The principal debates. According to England, the most controversial topics in the early debates for a proposed science foundation were “patents, the social sciences, geographical distribution of federal research funds, basic research versus applied, and control of the foundation.”² The patent issue reflected Democratic concerns that a public investment would end up in private hands against the public interest, whereas conservative concerns centered on fear that this would result in the government stifling innovation. While the sides worked out a compromise that tended to award patents to private business but retain a free license for the government to protect public interest, it did not in practice amount to much, as basic research was fundamentally unlikely to provide directly patentable research. For those applied research that were supported by other federal agencies, patenting regulation was ad hoc as no such oversight agency was established after WWII. As basic

¹ Michael D. Cohen, James G. March, and Johan P. Olsen, “A Garbage Can Model of Organizational Choice,” *Administrative Science Quarterly* 17, no. 1 (March, 1972): 3.

² England, *A Patron for Pure Science*, 5.

research ceased to be the sole supported type of research by the proposed agency, patenting became more prevalent by the 1970s.³ Accordingly, I do not pay attention to the Congressional debates surrounding patenting as it has no empirical significance for the scope of this thesis.

The issue surrounding basic vs. applied research did not actually amount to any importance in and of itself, as the scientist proponents of the foundation were adamant in an agenda of basic research. On the other hand, the issue of social sciences in a sense reflects this, as much of social scientific research would be applied in orientation, or, rather, a close mixture between the two. In substance, the conflict was reminiscent of the interwar episode (Compton vs. social scientists). The participants and their symbolic authority, however, were radically different. The natural scientists emerged as major contributors to the war effort and the comparatively minor contributions by social scientists were overlooked at best or completely forgotten at worst.

The issue of federal domination retained its concerns.⁴ However, that these were brought before the legislative branch in the form of a proposal this time presented political concerns to those in power. While many provisions internal to the foundation were acceptable, the issue of presidential authority over appointments proved to be much more sensitive. For the most part, the Democrats insisted on the president appointing the foundation's director, whereas the conservative scientists argued to the contrary. The latter was spearheaded by Bush, which later became a divisive issue among scientists. This was despite the fact that Bush had leaned toward a presidential appointment (at the suggestion of the Bureau of the Budget) before the release of the *Endless Frontier* but reverted at the behest of some committee members.⁵

³ Popp Berman, *Creating the Market University*, 100-101.

⁴ The social sciences issue was also connected to this in its more explicit relation to politics, values, and ideology.

⁵ Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945: A Political Interpretation of *Science—The Endless Frontier*," *Isis* 68, no. 1 (March 1977): 22.

The issue of geographical distribution was arguably the one that had the most long-term salience. This reflected, whether directly or indirectly, a fundamental clash between the scientific and political fields—a best-science approach on the one hand and a concern for public welfare on the other. This dilemma was actually already present in universities, per the organizational field of higher education, but became accentuated when the body politic was called to support these institutions with their taxes as a whole. As Kerr put it,

The great university is of necessity elitist—the elite of merit—but it operates in an environment dedicated to an egalitarian philosophy. How may the contribution of the elite be made clear to the egalitarians, and how may an aristocracy of intellect justify itself to a democracy of all men?⁶

Kilgore’s earlier attempts on science legislation. Senator Harley M. Kilgore was the first to put forward science legislation in the process that led to the establishment of the National Science Foundation. A lawyer by training and later a judge, Kilgore was had no scientific education. His interest in science legislation arose from a Congressional hearing on developing a New-Deal-like science policy that envisioned a centralization of science to benefit the public, which also had factual mischaracterizations regarding the national picture of research. This was followed by a hastily written bill proposed by the Senator, the Technology Mobilization Act of 1942, which was also ignorant of the wartime conditions and put the OSRD in serious danger.⁷ Acknowledging its immaturity and building on it, Senator Kilgore then proposed the Science Mobilization Act of 1943, which envisaged the creation of an Office of Scientific and Technological Mobilization (OSTM), garnering much attention. In its organization, the Office would put scientific laypersons in charge of science and include provisions quite detrimental to big business. Such early

⁶ Kerr, *The Uses of the University*, 91.

⁷ The American Chemical Society criticized the S. 607 for being “totalitarian.” “The American Chemical Society and the Kilgore Bill,” *Science* 97, no. 2524 (May 14, 1943): 438.

attempts reflected a contention that scientific research was being monopolized by big business and was becoming detrimental to innovation, particularly for small businesses.⁸ Besides opposition from scientists, Kilgore's 1943 bill was opposed by industry and the military, as the proposed civilian oversight included access to classified information.⁹ Scientists communicated their displeasure in large numbers. For example, while some opposed it generally, the American Association of Cereal Chemists opposed it on grounds that it might confuse the war effort, whereas the Electrochemical Society also opposed regarding peacetime conditions.¹⁰ Soon the American Association for the Advancement of Science (AAAS), "the most broadly representative scientific organization in the United States," having about 25,000 members and a combined membership of 500,000 through its affiliated organizations, recommended to Congress that neither the bill itself nor any other form containing similar provisions be accepted.¹¹ Senator Kilgore further tried to redress concerns in another bill in 1944, even inviting the OSRD leadership in its final revision to no avail.¹² As put by Daniel J. Kevles:

The differences between Bush and Kilgore boiled down to a basic issue: Kilgore wanted a foundation responsive to lay control and prepared to support research for the advancement of the general welfare; Bush and his colleagues wanted an agency run by scientists mainly for the purpose of advancing science.¹³

Bush had already expressed his wish in 1943 that Kilgore would invite some of the scientific leadership to get their advice. Nevertheless, this is what President Roosevelt had done

⁸ An important point to notice is that this stands in significant contrast to what the scientists had been arguing for basic science was considered. Industry focused on applied research and this tended to be a feat that could be accomplished by large businesses. Basic research, however, was significantly removed from the interests of either.

⁹ Kevles, "The National Science Foundation," 7-12.

¹⁰ Willard Berry, B. C. Burgess, H. F. Prytherch, and B. W. Wells, "The Mobilization of Science," *Science* 97, no. 2528 (June 11, 1943): 528; "The Electrochemical Society and the Kilgore Bill," *Science* 97, no. 2525 (May 21, 1943): 461; "The American Association of Cereal Chemists and the Kilgore Bill," *Science* 98, no. 2540 (September 3, 1943): 213.

¹¹ "Resolution of the Council on the Science Mobilization Bill (S.702)," *Science* 98, no. 2536 (August 6, 1943): 135-137.

¹² Kevles, "The National Science Foundation," 15-16.

¹³ *Ibid*, 16.

in the following events. After conversing with Bush, Oscar Cox had a letter prepared and forwarded to presidential adviser Harry Hopkins. The President then signed and sent the letter to Vannevar Bush, asking him to prepare what was to become *Science—The Endless Frontier*.¹⁴

5.1 Scientists Arrive in Congress

The 1945 Joint Hearings and the scientific community. It was previously incomprehensible that scientists would mobilize in unity for any form of legislation. Yet this is specifically what happened. What many scientists had been pondering during the war became an open discussion before it had even ended. The AAAS organized its “Research After the War” symposium, presenting many valuable suggestions, and others followed.¹⁵ Some time later came declarations endorsing Senator Magnuson’s bill (prepared with Bush’s help), a similar conception, or coming out against more centralistic ones.¹⁶ Scientists, as a community, would cease to exist as passive agents in the political arena and the fruits of their efforts would lead to arrangements that would give them a humble yet actual position within the field of power.

In the meantime, all three main political actors (Kilgore, Magnuson, and Truman) wanted the OSRD to continue until a science foundation was established. By October, there were two science legislation proposed on defense research, one on industrial research, and two

¹⁴ Ibid, 16-17.

¹⁵ Lawrence K. Frank, “Research After the War,” *Science* 101, no. 2626 (April 27, 1945): 433-434; Chauncey D. Leake and Donald Slaughter, “Freedom for Scientific Work,” *Science* 102, no. 2654 (November 9, 1945): 473-474; “Research Institutions of Biology,” *Science* 102, no. 2656 (November 23, 1945): 524. The final referenced work provides a unique and, to the best of my knowledge, a singular stance regarding the question of federal support.

¹⁶ “The Magnuson Bill,” *Science* 102, no. 2656 (November 23, 1945): 524; Sidney A. Harris, A. H. Hersh, Frank Hovorka, Frederick R. Mautz, and Harry Goldblatt, “The Promotion of Science by the Government,” *Science* 102, no. 2658 (December 7, 1945): 581-582; Physical Society of Pittsburgh, “Report on Science Legislation,” Report accepted at the 200th meeting of the Society. Reprinted in *Science* 103, no. 2674 (March 29, 1946): 383-384; “The Proposed Society for Freedom in Science,” *Science* 94, no. 2439 (September 26, 1941): 304-305.

on a science foundation. One of these last two was cosponsored by Senators Kilgore, Pepper, and Johnson (S. 1297). President Truman did not voice support for any one of these bills, but his call for supporting social sciences and an emphasis on a more controlling foundation made him implicitly stand closer to Kilgore's position. Standing against this was the Magnuson Bill (S. 1285). Prepared at the behest of Democratic Representative Wilbur D. Mills, the same version was introduced in the Senate by Democratic Senator Magnuson. One of the two people who had prepared this was Oscar M. Ruebhausen, who, along with Oscar Cox, had prepared the content of President Roosevelt's letter to Bush. Therefore, it was no surprise that this bill closely mirrored the Bush Report. Accordingly, Bush decided to put his weight behind this bill. This bill also caused bitter feelings in Kilgore, who felt double-crossed after Bush's associates helped draft the bill.¹⁷

On October 8, 1945, the first and arguably most important Congressional hearings on science legislation began at the Senate Office Building. The excitement for the future of science could be seen in Conant's remarks toward the end of the hearings: "If the proposals before you become law and Congress appropriates the money, we will see a flowering of scientific work in this country the like of which the world has never seen before."¹⁸ Witnesses included Vannevar Bush, James B. Conant, Karl T. Compton, Frank B. Jewett, Alfred N. Richards, Isaiah Bowman, Henry Allen Moe, J. Robert Oppenheimer, Lee Alvin DuBridge, James V. Forrestal, Lewis L. Strauss, and many more scientists, university administrators, federal government officials, and ranking military men. Among the witnesses, Jewett was the only person who objected the formation of the proposed foundation. Many of the witnesses' emphasis went toward ensuring that federal support did not encroach on academic freedom. The Magnuson Bill foresaw a National Science Board (NSB), whose members would be suggested by the NAS and then appointed by the President. It was this Board that would then appoint the foundation's director; contrary to the

¹⁷ "Joint Hearings in the Congress on Science Bills," *Science* 102, no. 2649 (October 5, 1945): 346-347; England, *A Patron for Pure Science*, 10, 25, 372; Howard A. Meyerhoff, "Scientific Research Bills before the United States Senate," *Science* 102, no. 2652 (October 26, 1945): 412.

¹⁸ England, *A Patron for Pure Science*, 36.

Kilgore Bill, in which the President appointed the director and an advisory board to be chaired by the director. Accordingly, the majority favored the Magnuson Bill on this point. On the other hand, the majority also favored including social sciences, which was part of the Kilgore Bill.¹⁹ Another point that made the achievement of a foundation more difficult was the Administration's dislike of taking away the President's power of direct appointment. Regardless, the scientific leadership still easily carried the momentum of WWII and were insistent on their framework, meaning that a compromise could not be achieved immediately.

To meet the demands of the majority of scientists, Kilgore drafted a new bill (S. 1720). The social sciences provision was retained as the majority were already for it. The NSB was included to a balancing element to the director, yet it was still to be appointed by the President, as well as the foundation's administrator and even division chairs. Furthermore, the proposed National Science Foundation was to have supervisory power over other research supporting federal agencies. As the previous bills, funding was allocated not just for research but also for *development*. Finally, 25% of R&D funding was to be shared between the states 25% was to be given to non-profit institutions, such as private universities. Even though part of the funding was earmarked for an equitable distribution, this still made a large sum of the total amount and was in contradiction to the scientific leadership's notion of best science.²⁰

Kilgore's compromise bill and the surprise Mills Bill. After the Truman Administration showed their strong support for the Kilgore Bill, Isaiah Bowman formed the Committee Supporting the Bush Report in order to lobby for a bill that reflected the arrangements

¹⁹ Jewett even later instigated Senator Willis to introduce a bill countering a federal science agency. See: England, *A Patron for Pure Science*, 26, 42-43; "S. 1777," *Science* 103, no. 2667 (February 8, 1946): 164-165. For Bush's comments on Jewett's dissent, see: Bush, *Pieces of the Action*, 64.

²⁰ Howard A. Meyerhoff, "Science Legislation and the Holiday Recess," *Science* 103, no. 2662 (January 4, 1946): 10-11; "S. 1720," *Science* 103, no. 2663 (January 11, 1946): 39-44.

prescribed the report. Those scientists who were not concerned with Bush's terms and/or who thought that small concessions could have been given for the sake of having a science foundation formed the Committee for a National Science Foundation.²¹

Under pressure from Bush's associates and part of the scientific community, Kilgore went forward for a further compromise.²² Using S. 1720 as a base, the disputed points were discussed by Kilgore, Magnuson, the Committee Supporting the Bush Report, the Committee for a National Science Foundation, and the AAAS to create a new bill. The new bill, S. 1850, cosponsored by Kilgore, Magnuson, and others, was revised a few times more under pressure from Bowman's committee, yet it was still fundamentally the bill it was based on. It would include a social sciences division but would be initially devoted to science studies. Partial distribution of funds to states was to remain the same. Most importantly, the administrator was to be appointed by the President after consulting with the NSB, which was also to be appointed by the President, in addition to some division chairs. The Foundation would similarly have oversight power over other agencies. The Committee Supporting the Bush Report became convinced that the presidency could not be limited in its power of appointment and that this was acceptable if a strong NSB was effected in order to balance the President-appointed administrator. The AAAS Council also voted on the bill to take an official stance, which was affirmed by 230 to 10 votes. The National Association of Manufacturers was the only organization to oppose S. 1850, which had previously supported the Bush Report but could not find Kilgore's patent provisions acceptable.²³

²¹ England, *A Patron for Pure Science*, 36-38.

²² For a concise summary of postwar science legislation effort until the drafting of the compromise bill (S. 1850), see: Howard A. Meyerhoff, "The National Science Foundation: S. 1850, Final Senate Bill," *Science* 103, no. 2670 (March 1, 1946): 270-273.

²³ Howard A. Meyerhoff, "Compromise Bill for a National Science Foundation," *Science* 103, no. 2668 (February 15, 1946): 192; England, *A Patron for Pure Science*, 39-43; "Text of the New Kilgore-Magnuson Bill," *Science* 103, no. 2669 (February 22, 1946): 225-230; Howard A. Meyerhoff, "S. 1720 vs. S. 1777," *Science* 103, no. 2667 (February 8, 1946): 161-162; Committee Supporting the Bush Report, "Statement Concerning S. 1850," Statement mailed to the President

This was the breadth of the acceptability the bill enjoyed among its most directly involved parties. Despite his disapproval for some parts, Bush too found the compromise bill far more acceptable than its original form. The 79th Congress, however, was very much occupied with some controversial legislation regarding labor and price controls. Due to some circumstances and reports, the Bush-Bowman group became convinced, through their associate John H. Teeter, that President Truman might lean toward accepting a bill resembling the original Magnuson Bill—summer recess was on the horizon and the administration only had the atomic energy act to show to their constituents, and even that was facing some difficulties in Congress. Furthermore, Senate Republicans this time attacked the “centralization and control of science” and “science czar” that such legislation would bring into effect, and released a minority report describing their criticism. Teeter was afraid that the foundation would be filled with associates of a personal rival, playing to the fears of conservatives by point at some of Kilgore’s industrial ties and to the fears of the scientists by arguing that the Senate delay could effectively kill S. 1850. A few days before its issuance, however, an event happened that caught many off-guard. H.R. 6448 was introduced to the House by Democratic Representative Wilbur D. Mills, which was in essence a modified version of the bill prepared by him and introduced by Magnuson. A Southern Democrat, Mills was also a fiscal conservative. The bill was referred to a generally conservative subcommittee and a hearing of two days was quickly prepared with amicable witnesses. Bush gave his blessing in testimony by remarking that the bill fulfilled “the needs of the country better than any other piece of legislation I have seen for the purpose.”²⁴ The Bush-Bowman group’s tactic was to get quick House approval of the Mills Bill and, considering the Senate delays, force the Senate to consider the original Magnuson Bill (which was still on the Senate calendar despite the compromise bill) instead of S. 1850. Kilgore found himself in a position leaning to compromise even more than before. Nevertheless, coming to understand that S. 1850 did have more chance in the

and Congress (23 April, 1946), reprinted in *Science* 103, no. 2679 (May 3, 1946): 558; Howard A. Meyerhoff, “The Senate and S. 1850,” *Science* 103, no. 2680 (May 10, 1946): 589-590.

²⁴ Watson Davis, “Scientists Divided,” *Science* 103, no. 2684 (June 7, 1946): 688.

Senate than reported, both Teeter and Bush urged action to be taken regardless of the bill chosen.²⁵

The compromise bill of S. 1850 came under fire from Senate Republicans and proposed amendments on several points. In the Senate debates, “Magnuson had to answer charges that he had deserted the great Dr. Bush,” while he portrayed Bush and Bowman as being converted to the compromise.²⁶ So did Kilgore. Senator H. Alexander Smith’s first amendment was on replacing the foundation administrator with a board that would effectively be dominated by the NAS. It was voted into a tie, effectively meaning its rejection.²⁷ Further, he argued that defense research had to be divorced from a foundation of basic science—defense research was applied research.²⁸ This was also argued for social sciences, pointing out these were applied research. Senators Smith and Hart also criticized the geographical distribution of funds as the “pork barrel feature.” While Senator Smith was against undergraduate scholarship (but not graduate fellowships), Senator Magnuson, essentially sharing the same view on almost all points, argued that the wartime depletion of scientific labor necessitated it. Regardless, Senator Smith appealed to the authority of *The Endless Frontier* in defending the amendments passed (effectively except for the last point).²⁹

Social sciences were eliminated from the bill, but other amendments by the Republican Senators were rejected but even they voted in favor of it. Other Republicans and some Southern Democrats voted against it and it was passed 48 to 18. To show support, the

²⁵ England, *A Patron for Pure Science*, 45-51; Howard A. Meyerhoff, “H.R. 6448,” *Science* 103, no. 2684 (June 7, 1946): 687-688.

²⁶ England, *A Patron for Pure Science*, 52.

²⁷ The complete removal of the administrator was even too far considering the standard set by *The Endless Frontier*. If one is to speculate, it could be said that this could have been amended in the line of the Magnuson Bill since half of the Senate already supported this measure.

²⁸ This is arguably the only inconsistent point in the overall argument from *The Endless Frontier*. These two can be theoretically integrated, but the report’s conception of the initiation of long-term defense research projects seems impossible to be divorced from applied research. Ironically, this is also the most direct legacy in the report from the WWII experience.

²⁹ *Ibid*, 51-56.

AAAS Council even took a renewed vote, favoring it this time 233 to 10. Even though Teeter had hopes to bring the bill further into the line of the Magnuson Bill, both him and ardent supporters of the compromise bill urged the House to move quickly before the session ended. Some White House officials tried to appeal to the President to help accelerate the process, but House Representatives already declared that there was not enough time left to consider the controversial sides of the bill. Soon, *Science* magazine, which issues obituaries after the passing of prominent scientists, wrote an obituary for the National Science Foundation.³⁰ In it, Meyerhoff remarked: “It is possible that Dr. Bush was similarly misled, but the fact remains that he must personally assume the burden of responsibility for the introduction of a bill for which his own representatives arranged the hearings on May 28.”³¹

5.2 The Truman Veto and the Final Compromise

Too much compromise? Truman’s veto. The scientists’ divided front was an important factor in the failure to pass science legislation in the previous Congress. Accordingly, Bowman proposed that Conant, as president of the AAAS, lead a new effort to organize a unified front. There was still optimism in the air, which soon dissipated after Republicans took control of both the Senate and the House of Representatives. Republicans converged on many points with the conservative scientists, but not only was S. 1850 not of this conception, there was also the possibility that fiscally conservatives might categorically stand against science legislation. This was exacerbated by the founding of the Office of Naval Research (ONR)—a research office effectively connected to the OSRD experience that was heavily investing in basic research. In such circumstances, Conant created the Inter-Society Committee for a National Science Foundation. At the same time, Bush continued

³⁰ Ibid, 56-57; Howard A. Meyerhoff, “S. 1850 in the House,” *Science* 104, no. 2690 (July 19, 1946): 48; Howard A. Meyerhoff, “Obituary: National Science Foundation, 1946,” *Science* 104, no. 2692 (August 2, 1946): 97-98.

³¹ Ibid, 98.

to be involved in talks with related parties. The changes brought by the midterm election called for a new bill, which Bush was discussing with the White House. These were primarily based on the issue of presidential appointment. Bush was very optimistic of a bill being passed in this session due to his talks with White House officials, but correspondence between the two sides suggests that President Truman's insistence of a presidentially appointed director was either communicated unclearly or not communicated at all. Hence, Bush's calculations were based on imperfect exchanges and/or impact receptions.³²

The Senate took action toward a compromise, however, this time in the other direction. The Smith Bill (S. 526), by Senator Smith, was cosponsored by Republicans and Democrats alike, and even designated the NSF as the successor to the OSRD.³³ According to the bill, the President was to appoint 48 members to the Foundation and designate a temporary chair. The members would then choose members to an executive committee, who in turn would select a chair. The executive committee would select the director of the foundation, which was to be confirmed by a majority of the NSF members. National defense was to be included, but not social sciences. The best-science approach would be maintained, but in the case where geographical distribution did not encroach on it, geographical distribution would be applied (e.g. if contestants for scholarship or fellowships were of equal ability, they would be distributed to widen the distribution). Finally, the oversight function of the NSF was envisioned similar to previous bills.³⁴ Republicans and outside supporters pressured Senator Smith to not give concessions on S. 526. A related issue was the amendment on the geographical distribution of research funds, which was also a success of the lobbying efforts of land-grant university presidents.³⁵ This issue revealed a difference in interpreting the best-science approach. As described by England,

³² England, *A Patron for Pure Science*, 62-68.

³³ *Ibid*, 69.

³⁴ Alexander H. Smith, Guy Cordon, Chapman Revercomb, Leverett Saltonstall, Warren G. Magnuson, and J. W. Fulbright, "Proposed National Science Foundation Act, 1947: S. 526," *Science* 105, no. 2721 (February 21, 1947): 191-194.

³⁵ England, *A Patron for Pure Science*, 73-75. This was also partly a demonstration of their collective capacity in focusing their pressure on Congresspersons in an organized manner.

The debate over geographical distribution revealed two different conceptions of the foundation: on the one hand, an agency to support fundamental research in institutions of demonstrated strength; on the other, an agency to foster the development of new centers of scientific education and research throughout the nation.³⁶

Most important was the amendment that gave the President his power to appoint the NSF's director. This also gave the President the sole power to remove the director (and not to the foundation members), which was caused by a clerical error and was too impractical to reverse at that point, possibly leaving it to the conference of the two houses.³⁷

A poll by the Conant-led Inter-Society Committee revealed that scientists were (or, arguably, had become) very flexible in details of NSF as long as it was founded. The bill, however, faced slowness in the House due to Republicans Representatives and Jewett's detrimental influence (it is important to consider Jewett's influence along with the Republican takeover of the House). Ultimately, the House released H.R. 4102, which was significantly different from the Senate bill. The director was to be appointed by the foundation and not the president, geographical distribution was not to be included as a formula, and the NSF was to have a stronger federal supervisory role in order to avoid duplication of efforts. The bills were sent to conference and S. 526 was accepted by both houses essentially in the way prepared by the House of Representatives, giving minor (and comparatively meaningless) concessions to the original Smith Bill. This bill, remarkably different than Kilgore's compromise bill, was then sent to the White House.³⁸

Then came the veto. Despite the few concessions by the finalized Smith Bill (i.e. patent provisions, geographical distribution, divisions), the lack of control made it unacceptable to the executive branch. This was even beyond the issue of appointment. Not only could the President not appoint the director, but also the chair of the part-time board who was to coordinate overall government science policy through an Inter-Departmental Committee

³⁶ Ibid, 75.

³⁷ Ibid, 75-76.

³⁸ Ibid, 77-80.

on Science. The fact that an un-appointed prominent scientist would outrank members of the Cabinet was no surprise unacceptable. Considering the overwhelming influence of the NAS in this arrangement, Meyerhoff argued that the bill would have given too much power over science to too few scientists. Pointing out that the Inter-Society Committee had been overlooked when drafting the bill, which had the broadest base among US scientists among the directly involved parties, he asserted that the Committee takeover leadership in legislation. Bush had much more influence over the bill, but once it was in the hands of the House Republicans, things went south, himself considering it unfortunate that Congress had refused the “courteous gesture” of allowing presidential appointment. According to England, the Administration’s Bureau of the Budget was likely decisive on Truman’s veto, even though some of its officials actually stood against the veto (which were not communicated to the White House). Such dissent argued that the President still had supreme personal control over the Inter-Departmental Committee and also that funds could not be transferred to the Foundation from other agencies without his approval. Furthermore, it was noted that the President never made it clear to Congress that he would veto such a bill. In fact, when the President wrote to Senator Smith after the veto, he did not state whether he would have approved the original Senate bill.³⁹

The final push. The Inter-Society Committee for a National Science Foundation was organized under the AAAS and included Conant, Compton, Detlev Bronk, Harlow Shapley, Talcott Parsons, and even a founding member of the Committee for Supporting the Bush Report. Considering that Bowman had asked Conant to takeover the leadership and that the Committee included prominent committee members from *The Endless Frontier*, Bush

³⁹ Geiger, *Research and Relevant Knowledge*, 27-28; Howard A. Meyerhoff, “The Truman Veto,” *Science* 106, no. 2750 (September 12, 1947): 236-237; England, *A Patron for Pure Science*, 81-82. It is certain that Bush had indirectly contributed to this, but accounts suggesting that the content of the bill reflected Bush are incorrect. For example: Geiger, *Research and Relevant Knowledge*, 27; Dael Wolfle, “The Inter-Society Committee for a National Science Foundation: Report for 1947.” *Science* 106, no. 2762 (December 5, 1947): 533.

had lost most of his prominent scientists allies for legislation (it is noteworthy, however, that he still held more influence during the drafting of the Smith Bill, which also has to do with the Republican Congressional takeover). The Committee had representatives from more than 75 scientific organizations and a majority of its representatives supported a presidentially appointed director, the inclusion of scholarships, and the social sciences—the same was expressed by the majority of US scientists in three separate polls. Like Bush, Chairperson Edmund E. Day expressed their wish for the support of basic research, without referring to practical applications.⁴⁰

Dael Wolfle partly attributed the Inter-Society Committee's failure (regarding the Smith Bill) to its lack of political involvement and its readiness for compromise. Such position-takings would become much more important after Senator Robert A. Taft became chair of the Senate's Republican policy committee, which made the establishment of the NSF a political issue. Another problem, however, was the decreasing Congressional interest in science legislation—particularly in the House. "From the 1947 veto until the spring of 1950, scientists' attention focused mainly on the House, where, to the rising frustration of the proposal's backers, parliamentary bottlenecks and indifference prevented any forward movement."⁴¹ The third attempt of science legislation was this time effectively killed due to delayed hearings and conservative concerns amplified by the existence of other federal organs. Many scientists lost hope, but the fact that the bill (S. 2385/H.R. 6007) was quickly drafted and without dissent among scientists brought it really close to being passed. Chairperson Day and his colleagues were persistent in their will to push.⁴²

If the Democratic takeover of the 81st Congress was any hope for passing a bill acceptable to both the Administration and the scientists, it was not much—the coalition between Re-

⁴⁰ Ibid, 529-531.

⁴¹ England, *A Patron for Pure Science*, 84.

⁴² Dael Wolfle, "The Inter-Society Committee for a National Science Foundation," 532-533; England, *A Patron for Pure Science*, 83, 89-90, 92; Dael Wolfle, "Inter-Society Committee for a National Science Foundation: Report of the Meeting," *Science* 107, no. 2775 (March 5, 1948): 235.

publicans and Southern Democrats, Jewett's influence, McCarthyism, and the Un-American Activities Committee stood made things difficult. The Senate, knowing that things would be difficult in the House, passed their bill without any discussion and amendments, hoping to take care of it after it would go to conference. This was Senator Democratic Senator Elbert Thomas's bill S. 247, which included the social sciences, emphasized the development of new science centers, wide geographical distributions of scholarships and fellowships, patent provisions linked to public interest, and an executive director to whom the board shall only have an advisory relation. In the House, science legislation became for the first time an object for far-right attacks, conjectured upon some of the foundation's backers having ties to Communist causes, such as Harlow Shapley, and backed with quotes from Scripture and Rudyard Kipling's Recessional. In the hearings, John W. Anderson, president of the National Patent Council, said that they were "much alarmed by the persistent efforts of . . . coldly fanatical men, alien-inspired, believed to have as their objective the stifling of that incentive to invent and produce which has given to America its phenomenal industrial growth," and remarked that the efforts for a National Science Foundation seemed to fit with Kremlin's pattern of underground operatives. The hearings almost took a month and the introduction of a "clean bill" took another. Democratic Representative J. Percy Priest introduced H.R. 4846 with an anticommunist affidavit for scholarship recipients but was nevertheless blocked by fiscally conservative Republicans and Southern Democrats.⁴³

The social space of Congress, having provided different groups of scientists with different allies in the past, now became a potential threat to the mere existence of the foundation through the conservative bloc, and predicaments proved extremely difficult to overcome had the scientists not become a united front supporting the Administration and liberal Democrats. Under the assault of fiscal conservatives, Bush himself backtracked from his predictions in *The Endless Frontier* and said that current federal funds for basic research were enough and that the NSF would simply assume some of such responsibilities. Losing

⁴³ England, *A Patron for Pure Science*, 93, 95-97.

his usual long-range serene attitude, he proceeded to lobby for H.R. 4846. The Inter-Society Committee, at this juncture, was in constant touch with the Administration and relevant members of Congress. Legislation proceeded under the shadow of the Cold War and amid McCarthy's accusations. To convince the fiscal conservatives, the proponents of the bill had to concede a \$15 million cap, which could be removed later on. Furthermore, the House added amendments of a loyalty oath and an FBI investigation for foreigners, and passed it to conference. Scientists were furious at these security provisions—the FBI did not like their new responsibility either. Ultimately, the most objectionable features of the House version of S. 247 was removed in conference and accepted by both houses—that is, except for the affidavit for scholarships.⁴⁴

Soon, the President approved the National Science Foundation Act of 1950, recalling *The Endless Frontier* in his statement. Years had passed since the introduction of what some might have considered an uncontroversial issue, reflecting scientists' different visions for the future, fluctuating power relations in Congress, and the prerogatives of the executive branch. Both the Republicans and the Democrats, however, conceived the Foundation to have some supervisory role—from the perspective of the scientists, an overall cooperative role at worst and some budgetary suggestions to the Bureau of the Budget at best. President Truman was satisfied with, among other things, retaining the power to hold an official accountable to the elected office of the President. This was not a black and white issue, however. Bush, when recounting the events years later, remarked that the vetoed bill had in fact been worth it, considering that the Foundation's Director was finally made responsible to the Board, to which members would be presidentially appointed with the consent of the Senate, and in consideration of suggestions by the NAS and other academic organizations.⁴⁵

⁴⁴ Ibid, 98-105.

⁴⁵ Ibid, 30, 106; Bush, *Pieces of the Action*, 65-66; Geiger, *Research and Relevant Knowledge*, 27-28.

What I called the ‘bureaucratoscientific field,’ that significant overlap between the essential fields of science and bureaucracy, had been set in motion during WWII and became crystalized during postwar science legislation—primarily in relation but not limited to the National Science Foundation. I turn to this in the following chapter.



CHAPTER 6: THE BUREAUCRATOSCIENTIFIC FIELD

6.1 “Filling the Gaps:” The NIH, the AEC, and the ONR

The irony of the late establishment of the NSF resulted in other organizations to partially undertake its conceived role. The Atomic Energy Commission (AEC) had been building multi-million dollar particle accelerators, the National Institutes of Health (NIH) was near-monopolizing the health sciences, and the Office of Naval Research (ONR) was becoming the effective patron of academic science, save for the health sciences. There were even officials in the White House who thought that, in the case of a failure to create the NSF, the executive branch could take other measures to coordinate other science-related agencies in order to achieve the aims of the bill. The developments taking place in these organizations had been developing, for the most part, at the independent judgment of their scientist-administrators. Regardless, they certainly had their commonalities. For example, the NDRC’s contract method could be seen in their project grants.¹

As previously mentioned, universities became the dominant social spaces for scientific research, due to which the organizational field of higher education has great significance for explaining the scientific field. The bureaucratoscientific field, however, created more diversity in this organizational field’s environment, in effect expanding the scientific field’s borders. Including numerous committees, boards, study groups; the most important organizations here are the agencies that are uniformly or partly responsible for supporting (and to minor degrees conducting) scientific research.

The National Institutes of Health. The Public Health Service was the federal organization responsible for public health, under which the National Institute of Health was the center

¹ Ibid, 27; England, *A Patron of Pure Science*, 57-58; Bush, *Pieces of the Action*, 39.

for government-led biomedical and public health research. Research, however, was intramural, meaning that this had little to no direct effect on academic researchers. Despite this, the creation of the NIH in 1930 was an important shift in the PHS, which was to be accompanied by increased attention to basic science.²

The National Cancer Institute (NCI) was created in 1937 under the NIH (which would later be re-designated the National *Institutes* of Health in 1948) and was authorized for extramural research in the manner of ‘best science.’ This was a queer innovation at the time and, even though not completely embraced by the NIH, weakened its antipathy toward the practice. The NIH’s resistance was completely shattered when the OSRD experience demonstrated the superiority of cooperating with university-based science through funding, yet, lacking a legal mandate, it could only award grants for cancer studies. Accordingly, the PHS took it to itself to convince the government for the necessity of research and institutional grants to non-governmental institutions, which was passed by Congress and signed into legislation by President Roosevelt on July 3, 1944.³

The NIH was able to expand its programs by initially transferring medical contracts from the OSRD, which followed after it became clearer that the NSF could not be established immediately. Prior to this, the PHS agreed with the Bush Report for the most part, but also stated that the proposed foundation should not infringe on the PHS’s authority to support its related sciences and that federal agencies have representation in the NSF. NIH Director Dyer made this clear to Congress in the 1945 hearings by testifying that they had the legal authority to supporting medical research. This, of course, stood oddly in relation to the medical researchers at the time who wanted the creation of a separate foundation for themselves. The NIH had not yet been recognized as the primary patron for health related sciences in the scientific community. Regardless, Bush successfully (but also painstakingly)

² Donald C. Swain, “The Rise of a Research Empire: NIH, 1930 to 1950,” *Science* 138, no. 3546 (December 14, 1962): 1233-1234.

³ Geiger, *To Advance Knowledge*, 263-264; Swain, “The Rise of a Research Empire,” 1234-1235; “Legislation on the Scope of the U. S. Public Health Service,” *Science* 100, no. 2585 (July 14, 1944): 25-26.

convinced the Palmer Committee and non-governmental medical administrators on condition that the NSF's establishment would not take long. There are many combinations of events that could have occurred had a separate foundation been proposed. Nevertheless, the NIH filled the vacuum and rose in its budget and support, eventually becoming *the* federal patron for health related scientific research. Similar to the logic underlying the proposal for the NSF, the PHS also sought to compensate for the lack of competent medical researchers through fellowships as well.⁴

Like the rest of the scientific community, medical researchers too feared the dangers of proximity to politics, which eventually became its greatest asset. Whether basic or clinical research, the presumptive understanding that it contributed to public health was a niche not possessed by other basic research, leading to much more generous appropriations and less intrusion into its domain for the most part. Medical schools were finally to become the scientific research centers as they were conceived by Abraham Flexner.⁵ The NIH's appropriations (1938-1958) are listed below in Table 1.⁶

⁴ Ibid, 1235-1237; "Proposals for a National Research Foundation," *Science* 102, no. 2656 (November 23, 1945): 539-541.

⁵ Geiger, *Research and Relevant Knowledge*, 27; John R. Thelin, *A History of American Higher Education* (Baltimore: Johns Hopkins University Press, 2004), 273.

⁶ While federal statistics provide basic research as a separate item in the aggregate, the NIH does not provide any studies on disaggregating for itself. Since numerous centers and institutions under the NIH can be involved in both basic and clinical research or just one of these, it is impossible to disaggregate them without relying on other sources.

1938	\$464,000	1945	2,835,000	1952	56,928,000
1939	464,000	1946	3,415,000	1953	58,610,000
1940	707,000	1947	8,075,000	1954	70,799,000
1941	711,000	1948	24,626,000	1955	81,151,000
1942	700,000	1949	28,540,000	1956	98,509,000
1943	1,278,000	1950	52,714,000	1957	177,816,000
1944	2,735,000	1951	64,755,000	1958	210,423,000

Table 1: NIH Appropriations (1938-1958)⁷

The Atomic Energy Commission. The AEC was conceived as a means of transferring the knowledge gained from nuclear research into peaceful economic use. Concerns of domination in the AEC, seen in the deliberations of all such agencies, outweighed all others. This was not due to the breadth of the potential risk, which was rather the NSF's province, but the amount of security provisions thought necessary to cull any breaches of knowledge that could have resulted in the development of atomic bombs elsewhere. The lines between basic research and secret research overlapped too much according to scientists, which prompted them to lobby against disproportionate measures.⁸

Different bills had radically different approaches on several points. For example, in regard to research, S. 1717 allowed research to be conducted as long as safety regulations were followed, S. 1463 limited research that released an amount of atomic energy relevant to military or industrial use, and S. 1557 mandated a license to be only given to qualified institutions, which could be categorically revoked if deemed necessary. In regard to the dissemination of research results, S. 1717 distinguished between basic research and technical information, establishing the first as free from restriction and putting the latter under

⁷ National Institutes of Health, the NIH Almanac, <https://www.nih.gov/about-nih/what-we-do/nih-almanac/appropriations-section-2>.

⁸ "The Atomic Energy Act," *Science* 102, 2653 (November 2, 1945): 441.

the Commission's supervision albeit with liberality. S. 1557 distinguished between fundamental research and militarily relevant research, similarly, issuing freedom to the first but placing the latter under the supervision of the Commission and the Services. S. 1463, on the other hand, put all information without distinction under the full authority of the Commission (even above the Espionage Act).⁹

Proposed by Democratic Senator Brien McMahon, S. 1717 was amended a few times and, after a year of hearings, the Atomic Energy Act was signed into law on July 26, 1946 (also taking over the Manhattan District and other related properties and contracts). The AEC was to be run by five full-time members appointed by the President. The full-time condition indirectly precluded active military officers from becoming members and in effect the possibility of military domination. The Commission's general manager was also to be appointed by the President but this time at the suggestion and consent of the Senate. A General Advisory Committee was to be established so to advise the Commission on scientific and technical matters, whose members were also to be appointed by the President. Finally, a Military Liaison Committee was to perform an advisory and consultative role in cooperation with representatives from the War and Navy Departments.¹⁰

The articles directly related to research provided that the following programs be established:

(2) A program for the control of scientific and technical information which will permit the dissemination of such information to encourage scientific progress and for the sharing, on a reciprocal basis, of information concerning the practical industrial application of atomic energy as soon as effective and enforceable safeguards against its use for destructive purposes can be devised.

⁹ Howard A. Meyerhoff, "Domestic Control of Atomic Energy," *Science* 103, no. 2666 (February 1, 1946): 134-135.

¹⁰ *Ibid*, 90-91, 94.

(3) A program of federally conducted research and development to assure the government of adequate scientific and technical accomplishment.¹¹

The Commission was authorized to make intramural as well as extramural research—the latter through contracts and loans. There were significant operational controls, however, due to the risks nuclear atomic research presented. For example, the Commission was to (1) own all facilities that could produce fissionable material, with the exception of research labs below the capacity for producing fissionable material sufficient for atomic weapons, (2) own all fissionable material and distribute according to research and other purposes, and (3) reserve the right to license the manufacturing and use of energy utilization devices except for those of research and medical purposes.¹² All of these suggest that research was privileged in its legal prerogatives. The Act further solidified this, after similarly qualifying it with some necessary but general security limitations mentioned for non-research purposes, by saying that “the dissemination of scientific and technical information relating to atomic energy shall be permitted and encouraged so as to provide the free interchange of ideas and criticisms essential to scientific progress.”¹³ The problem, however, was that these statements were not operationally defined and left to the authority of the five-person Commission membership. For example, the law did not explain how and according to what the Commission would (1) support, contract, and make arrangement for scientific research, (2) distribute fissionable material, and (3) license the manufacture, production, or use of devices for the utilization of fissionable material.¹⁴ These made the Commission very powerful. As put by E. Blythe Stason,

Although the history of recent years reveals many instances of somewhat similar but less sweeping delegation of discretionary regulatory powers, no other peacetime enactment even approaching in importance the Atomic Energy Act has conferred anything like as

¹¹ E. Blythe Stason, “Law and Atomic Energy,” *The Annals of the American Academy of Political and Social Science* 249, Social Implications of Modern Science (January, 1947): 90.

¹² Ibid, 91-93.

¹³ Ibid, 94.

¹⁴ Ibid, 95-96.

much uncanalized power upon an administrative . . . In this regard the Atomic Energy Act occupies a unique place in jurisprudence. It is a juristic innovation rendered necessary by the impact of one phase of modern science on the economic and social order.¹⁵

Writing soon after the passage of the Act, Stason added that at that point of the development of atomic research and technologies, it was not possible for Congress to prepare a bill that could successfully regulate it in detailed written form. Regardless, the fact was that, “[i]n very large measure, the immediate future of research in atomic energy [was] dependent upon the wisdom of five men acting outside the range of legal rules, principles, or standards.”¹⁶ However, this also meant that the White House could regulate these operational gaps through executive orders.

The two research settings where national security was an overarching issue were the AEC and the Defense Department, in which case both were lacking in necessary procedures to ensure academic freedom. For example, the Loyalty Order (instituted via an executive order), was criticized for seeking an individual’s loyalty to the country (and not tangible security issues) through an investigation into beliefs and attitudes.¹⁷ Even the NAS rejected to administer the AEC’s fellowship program in which even fellows with non-secret work were subjected to FBI investigations.¹⁸ Despite this, the AEC had committed itself to a big program of supporting university research outside of the national laboratories.¹⁹

The last among the gap fillers was the Office of Naval Research, which emerged in an unpredictable manner and, in a sense, a sort of successor to the OSRD in a manner distinct from the NSF. Ironically, the NSF was to follow the example of the ONR’s success to fulfill its own legal mandate.²⁰ This is the subject of the following section.

¹⁵ Ibid, 95.

¹⁶ Ibid, 96.

¹⁷ “Civil Liberties of Scientists,” *Science* 110, no. 2851 (August 19, 1949): 177-179.

¹⁸ “Statement of the National Academy of Sciences on the AEC Fellowship Program,” *Science* 110, no. 2868 (December 16, 1949): 649, 651, 670.

¹⁹ Geiger, *Research and Relevant Knowledge*, 22.

²⁰ See: Ibid, 25.

6.2 ONR: the “Enlightened” Research Patron

The Office of Naval Research (ONR) was arguably the agency most responsible for establishing the postwar links between science and government and became the ideal federal patron to be compared to other agencies. Even though it did not and structurally could not have maintained this role, the ONR operated as a unique agency in circumstances that allowed it to function effectively without or with minimal supervision.²¹

6.2.1 *The Road to the ONR*

In 1946, General Eisenhower, Chief of Staff of the Army during the Truman administration, released a memo recognizing that the military’s dependence on science and technology was at a level that could not be duplicated by the Services themselves.²² This recognition had become prevalent and the Services, accordingly, were looking for the means and the people to institutionalize R&D. Disregarding the events involving Admiral Bowen, the Navy had always been the military branch most accommodative of scientists.²³ It was to continue to be so, albeit this time not exactly as a part of this pattern but a combined result of the OSRD legacy and accidental circumstances.

The ONR was a result of Admiral Harold Bowen’s postwar efforts to bring the Navy into the nuclear research game, which had been previously blocked by Vannevar Bush. As aforementioned, Admiral Bowen himself had been blocking Frank Jewett’s efforts to bring civilian scientists into antisubmarine research in the Navy, due to which Bush intervened ensured the participation of civilian scientists. It was as a result of their participation

²¹ See: Sapolsky, *Science and the Navy*, 4; Geiger, *Research and Relevant Knowledge*, 24.

²² Ibid, 13-14.

²³ Sapolsky, *Science and the Navy*, 11-13.

that the Office of the Coordinator of Research and Development (OCD) was found in the Navy to coordinate between it and the OSRD. This Office, against which Admiral Bowen's actions in part caused the Navy to be "punished" away from nuclear research, was where Bowen would later recruit the scientists for the ONR for nuclear research. These scientists were called the 'Bird Dogs' due to their role of ferreting out tasks during the WWII coordinative responsibility. They themselves had originally developed a plan (that was nearly identical to the eventual ONR) to coordinate with civilian scientists on weapons.²⁴ The concern was that civilian scientists' interest in research relevant to the Navy would diminish and that industry could not be relied on due to their profit incentive. Aware of President Roosevelt's personal interest in the Navy, the Bird Dogs arranged an appointment to present their plan, but the President passed away, not having seen their proposal, like *The Endless Frontier*.²⁵

Admiral Bowen, through a growing relationship with Under Secretary of the Navy Forrestal and the regard of his special assistant Lewis L. Strauss, later returned to influence as Chief of the newly created Office of Research and Inventions (ORI), which subsumed the OCD (including the Bird Dogs). Created (May 19, 1945) soon after President Roosevelt's death, the Admiral conceived the ORI as his base to move into nuclear research—now "converted" to the necessity and desirability of civilian scientists. He capitalized on some public criticism of the Army to attract nuclear scientists and even sent Captain Robert Dexter Conrad around the country to university presidents to accept Navy contracts for their scientists. Bowen was making use of the Bird Dogs' plan and had decided to include all forms of basic research (even though focusing on nuclear research). Sapolsky concludes that that likely had to do with Bowen's desire to attract scientists for prospective needs. Nevertheless, the Bird Dogs were surprised how swiftly and determined the ORI embraced their perspective, considering that both Bowen and Conrad used to be against

²⁴ The influence of the OSRD experience and the overlap with Bush's views are obvious.

²⁵ Ibid, 9, 20-23.

basic research. There were two obstacles in front of Bowen, however. First, his organization did not have Congressional legitimacy (it was established by an executive order). Second, the ORI was not yet authorized by the Navy to undertake nuclear research. For the first, the Bird Dogs prepared a bill that was quietly passed in Congress with the support of Lewis Strauss and Forrestal, among others. The bill, Public Law 588, gave the organization its new and lasting name: the Office of Naval Research (August 2, 1946). Bush, Compton, and other leading scientists also embraced it. The second did not come to pass, however, as Bowen's rivals within the Navy (to whom Bush had handed him over previously) gave the authority to the Chief of Naval Operations for Special Weapons (also on grounds that nuclear propulsion should not be researched under a decentralized unit). The irony was that, "[t]he Office of Naval Research, Bowen's secure organizational base for the creation of the nuclear navy, was essentially an organization in search of a mission from the day it was created."²⁶ Initially conceived by the Bird Dogs for cooperation with civilian scientists, then as a means for nuclear research by Bowen, the ONR was to return to its cooperative arrangement with civilian scientists.²⁷

In such a predicament, however, there was the fact that Public Law 588 made the ONR independent from much of the Navy—even isolated it. Effectively, there was the possibility to take initiative in steering it ahead. Captain Conrad, who was responsible for the university research program, hoped to link the two primary divisions of the ORN—the science branch program, composed of academic disciplinary sections, and the program branch, meant for the application of research and development of naval weaponry and technologies. However, Conrad soon retired and control of the ONR was mostly left to its civilian scientist staff. Its first Chief Scientist, Alan T. Waterman, was significant to this effect and was to even play a larger role in the future.²⁸ The ONR, besides its contracting methods, had two in-home laboratories, of which the Naval Research Laboratory partially

²⁶ Ibid, 28-29.

²⁷ Ibid, 23-28, 35; Sapolsky, "Academic Science and the Military," 381-384.

²⁸ Sapolsky, however, also notes that Waterman's role is exaggerated and that it were the staff scientists who set the agenda. Sapolsky, *Science and the Navy*, 49.

conflicted with the ONR's organization, since it performed research that was being contracted by the ONR. Regardless, the ONR's emphasis on basic research is demonstrated by its early budgets. For example, less than 10% of its \$86 million appropriation for its contract research program (FY 1946) went to naval science applications. The rest of the funds, even made larger by transfers from the AEC, was used to fund research, buy equipment, and train scientists in the physical sciences (as well as, to a lesser degree, the biomedical sciences).²⁹

6.2.2 The Contract Research Program

Prior to federal support becoming widespread, universities and administrators voiced concerns that federal support would not be stable (e.g. Karl Compton and the MIT Corporation).³⁰ To the opposite end, the ONR concentrated to assuage prospective anxieties. Captain Conrad made promises to university administrators and these were communicated to Harvard, MIT, Chicago, Caltech, and UC Berkeley by Admiral Bowen. Administrators preferred contracts over grants (unlike academics) since it gave somewhat more control to the university and financial rewards, and the ONR was authorized to only make contracts anyway (defense research agencies could not make grants until 1959). Regardless, the contract was made to resemble a grant (like the OSRD), in which no concrete quid pro quo took place as would be expected from a contract. This was, in a sense, a mean to satisfying both universities and scientists.³¹ Nevertheless, despite any concerns and the ONR's genuine efforts to placate them, the reality was that "[t]he competitiveness of the American university system alone guaranteed that there would be willing takers for the research support the Navy had to offer."³²

²⁹ Ibid, 47-50.

³⁰ Ibid, 40.

³¹ Ibid, 40-43; Sapolsky, "Academic Science and the Military," 385-386.

³² Sapolsky, *Science and the Navy*, 40-41.

Captain Conrad was the initial designer of the ONR contract, who also worked in the OCRD during WWII. Most projects supported by the ONR in its Contract Research Program, basic research in nature, sought no utility for the Navy and their proposals were evaluated by scientists. It also lessened the burden on scientists by requiring less reporting than other agencies; secrecy was not much of an issue due to the emphasis on basic research, which was conceived as unclassified and publishable. The ORN also actively sought to support graduate students through having them included in research proposals. When the ONR gained the right to make grants in 1959, it did not even feel the need to use it. Finally, the ONR demonstrated its administrative flexibility and commitment with the lengths to which it went to fulfill promises to university administrators.³³ For example:

In 1948, as part of general reductions imposed upon the military, ONR'S university research appropriation was cut in half from \$22 million to \$11 million. Rather than reduce its support of university research, however, ONR drew from the reserve set aside for projects continuing over several years in order to maintain the dollar flow to the universities and permit the award of new projects. It then pressed for a supplemental appropriation to extend the average term of its contracts which had had to be reduced because of the expenditure of the reserve. The unfunded years of approved projects were considered "moral obligations," if not legal obligations, in subsequent budget presentations. Congressional appropriations over the next few years were sufficient to keep the university research program growing, but were not enough to add to the average length of the contracts they supported. With the surge in military appropriations that accompanied the Korean War, a supplement of \$20 million was obtained on the pledge that these funds would be used only for contract "longevity." At the next reduction in its budget, however, ONR again dipped into the longevity reserve for a compensating amount. Risky budget tactics, perhaps, but given their purpose, actions certain to be appreciated in the universities.³⁴

³³ Ibid, 43-48; Geiger, *Research and Relevant Knowledge*, 24.

³⁴ Sapolsky, *Science and the Navy*, 46-47.

6.2.3 Who Sets the Tune? The ONR's Proper Relation to the Federal Support of Basic Research

The ONR's acquired role in basic research was not due to senior officers or even Admiral Bowen but primarily the several different motivations that had the same aim of supporting research (e.g. supporting basic research per se, acting in lieu of the NSF since it could not be established yet, ensuring the Navy's proximity to civilian scientists). Accordingly, there were disagreements as to the permanence of the ONR's commitment to basic research, but this had to operational effects in the meantime.³⁵

Intragovernmental rivalries. The ONR was naturally related to military research and was even legislatively responsible for the "coordination" of scientific research in the Navy. Despite this, it shied away from this responsibility due to the likely conflict it would spark with other and more established Navy bureaus. This is somewhat ironic, however, as the ONR later made a small attempt to monopolize authority over basic research over the entire military (not applied research and development), which was blocked by the Air Force's territorial instincts. On another level was the ONR relations with civilian agencies (such as the AEC, NIH, and State Department), which varied according to how much their operations overlapped or conflicted. This was particularly visible during the creation of the NSF, in which the ONR's support for it was qualified by perceived threats. The ONR and NIH had held talks to prevent NSF from "infringing" on their roles and, while even as the ONR gave administrative support to the NSF during its formation, the Navy passed a resolution calling for the pluralistic support of science. Ironical and probably fortunate for the NSF was that seven out of ten of the Foundation's top officers were to be of ONR or Navy connection—even Alan Waterman, ONR's first Chief Scientist and the first Director of the NSF in the future, was part of discussions in limiting NSF's role.³⁶ One appreciates

³⁵ Sapolsky, "Academic Science and the Military," 384-385; Sapolsky, *Science and the Navy*, 52.

³⁶ *Ibid.*, 52-56.

Vannevar Bush's prudence in light of his warnings that neither the armed forces nor the related civilian agencies should have a right to vote in the proposed foundation due to their special interests.³⁷

The issue of relevance. A classic reference used to demonstrate the military's relation with basic research conceived broadly comes from Secretary of Defense Charles E. Wilson, saying that it is not the military's intention to support "research on why potatoes turn brown when they are fried."³⁸ This was the mood that slowly prevailed after what is considered to be a golden age of academic science, between 1946-1950—free from questions of practical relevance or of equity in geographical distribution.³⁹

The two in-house laboratories of the ONR (the Naval Research Laboratory and the Special Devices Center) were actually interested in projects directly relevant for the Navy, but it can be easily said that the ONR as a policy was oriented toward supporting basic research. This came under scrutiny by the Navy for the first time after President Truman cut the Navy's budget in 1949. To this effect, the Navy's General Board took the ONR from the Secretariat (where it was isolated) and placed it under military supervision. However, the ONR was saved from further pressure as attention was directed away due to the Korean War. Yet this could be only temporary, as the ONR did not have the luxury of being isolated anymore. Regardless, it is a testament to the basic research consensus that the General Board, even when coming down on ONR's basic research policy, apologetically acknowledged its value and only qualified it with relevance.⁴⁰ These trends can be seen below in Table 2.

³⁷ Bush, *The Endless Frontier*, 57.

³⁸ Sapolsky, "Academic Science and the Military," 387.

³⁹ Sapolsky, *Science and the Navy*, 57; Sapolsky, "Academic Science and the Military," 386.

⁴⁰ Sapolsky, *Science and the Navy*, 59-61; Sapolsky, "Academic Science and the Military," 387-388.

FY	Contract Research Program	R&D total	ONR total	Contract research transfers to ONR from other agencies
1947	\$22.3	\$43.8	\$45.9	-
1948	14.2	32.6	34.7	-
1949	19.8	42.3	45.4	\$10.8
1950	20.5	41.6	44.9	5.7
1951	50.2	77.7	82.2	12.5
1952	35.7	67.2	73	16.7
1953	33.7	59	65.2	19.1
1954	28.1	53.2	59.3	11.2
1955	30.4	53.7	59.7	12.7
1956	32.2	72.4	79.1	15.5
1957	29.5	85.5	93	21
1958	51.9	80.2	88.3	19.3

Table 2: ONR Funding History, 1947-1958⁴¹

In light of the circumstances, the ONR became politically more sophisticated—or, in other words, underwent collective learning. It developed a “two title policy” in which projects would be registered both with their original title and a title deemed more appropriate to the Navy (justifying basic research projects for their value to the Navy was called “painting projects Blue”). They also decreased support for controversial areas and developed better relations with Congress. Furthermore, they actually tipped the balance toward more applied research in order to avoid intervention. Regardless, Secretary Wilson, the ONR’s foremost antagonist, was to remain as President Eisenhower’s Secretary of Defense until the launch of Sputnik I. Wilson, not against basic research in and of itself, was against its

⁴¹ The ‘R&D total’ includes the Contract Research Program as well as intramural laboratories conducting applied research and development. The ‘ORN total’ includes the aforementioned plus administrative expenses. Funds for contracts transferred to the ONR are not part of the appropriation. Sapolsky, *Science and the Navy*, 132.

inclusion into the DoD. The ONR, bereft of its old status, invented a rationale of the “listening post” against Wilson, which was less of an invention than an adaptation of the tenets of basic research ideology—the prospective uses of basic research were could not be predicted, hence the ONR was to act as a ‘listening post’ if it wanted to adequately survey basic science for potential promises to the Navy. Nevertheless, ONR officials conceded to further limiting the scope of supported basic research in time. However, when another wave of budget cuts were to be endured under President Eisenhower, Sputnik I not only stopped the budget cuts, as did the Korean War, but made the Administration endorse budgetary increases “pulled out of the air.”⁴²

As Harvey Sapolsky puts it, “notwithstanding its inception as a bureaucratic accident, ONR deserves to be remembered as America's most effective patron of science.”⁴³ Criticism of military sponsorship was deflected due to the satisfaction universities had with ONR contracts. The significance of the ONR is better understood when compared with the NSF. The Contract Research Program had an appropriation of \$22.3 million in its first year, an amount the NSF would surpass only in 1957—10 years! The ONR’s annual appropriation for basic research would continue to be larger than the NSF until then. As a testament to its success, its structure was being replicated in other agencies (e.g. the Office of Army Research and the Office of Aerospace Research) and its alumni were being recruited as well (e.g. the NSF and the CIA). Practically functioning as an “Office of National Research,” the ONR slowly decreased its support for basic research and increased expectations for relevance where it was given.⁴⁴ Where it slowly left, the NSF slowly picked up.

⁴² Sapolsky, *Science and the Navy*, 63-66, 68-70; Sapolsky, “Academic Science and the Military,” 389.

⁴³ Sapolsky, *Science and the Navy*, 38.

⁴⁴ Geiger, *Research and Relevant Knowledge*, 25; Sapolsky, *Science and the Navy*, 38-39; Sapolsky, “Academic Science and the Military,” 380, 384-385.

6.3 The Formative Years of the National Science Foundation

As previously mentioned, not all capital holders try to increase or conserve their share.⁴⁵ For Bush, this was the case when it came to the operating the National Science Foundation. Pushing limits to ensure that it was created “correctly” was one thing and continuing to exert significant influence after the fact was another. Accordingly, Bush told President Truman to leave him off of the list for the NSB, since he would be perceived as a dominating figure among scientists, and proceeded to remove from the list suggested by the NRC the names of Compton and Richards similarly, but also the names of scientists he regarded to conflict with the best-science approach. Truman’s final list of appointments were effective a board able to resist political intervention from the executive branch and had an allegiance to basic research. These included but were not limited to Chester I. Barnard (President of the Rockefeller Foundation), Detlev W. Bronk (President of Johns Hopkins University and of the NAS), James Conant (President of Harvard University), Charles Dollard (President of the Carnegie Corporation of New York), Lee A. DuBridge (President of Caltech), and Charles E. Wilson (President of the General Electric Company) who would later become Secretary of Defense under President Eisenhower and give the ONR a hard time. Conant, having been elected chair at the Board’s first meeting, appointed a committee to specify the qualifications for the Director. In fact, its appointment was agreed on advance to ensure that the Director would share their basic scientific and other academic concerns.⁴⁶

The committee suggested three “desirable” and two “indispensable” qualifications:

The director should “be or have been a practicing scientist of recognized ability, or at least should have had experience in administering scientific enterprises or personnel”; should understand how the national government works; and should understand institutions of higher education and American research. He must be able to organize and manage “an

⁴⁵ Bourdieu and Wacquant, *Reflexive Sociology*, 109.

⁴⁶ Bush, *Pieces of the Action*, 302; England, *A Patron for Pure Science*, 115, 119-123, 384.

institution of moderate size but of high class personnel” and must “be adept in the management of ‘public relations.’”⁴⁷

After the suggestions received by the committee were eliminated and passed on to the Board, the Board made few revisions and finalized the ranking with almost complete unanimity. At top was Detlev Bronk; eight were composed of people from universities and research institutes (one or two having ties to industry); only one was from a federal-military agency. Initial ideas revolved around Bronk’s appointment. However, Bronk’s condition that he would bring in substantial military research was not well received by the Administration, who went over to the third name suggested: Lloyd V. Berkner from the CIW. Berkner also had close ties to Bush and ended up dropping out of the list after accepting the presidency of Associated Universities, Incorporated. Among the list was Alan Waterman, the only person to hail from a federal agency in the list. He was experienced in particularly what the NSF was to do and was a name that even disparate figures like Harlow Shapley and Bush agreed on (he had been Bush’s choice at least since 1948).⁴⁸ Besides this, of course, Waterman had also been a division member of the NDRC and later chief deputy to Compton in the Office of Field Service of the OSRD.⁴⁹

Waterman’s principal demand was to not have the NSF barred from assisting defense research. This was due to his concerns of the Cold War and that his years of experience could go to waste in this regard if he was not allowed to contribute (there were talks of the possible reestablishment of the OSRD, suggested by Bush, which would have put such a role in jeopardy). Circumstances suggest that this reassurance might have been given by the Bureau of the Budget even if not certain. Regardless, after these talks, President Truman’s decision to appoint Waterman was delivered to the NSB on March 9, 1951, and was received with unanimous approval. Waterman, having considered the ONR as a surrogate to the NSF, recruited key personnel from the ONR to help him move the wheel.⁵⁰

⁴⁷ Ibid, 123-124.

⁴⁸ Ibid, 124-126.

⁴⁹ Stewart, *Organizing Scientific Research*, 56, 129.

⁵⁰ England, *A Patron for Pure Science*, 126, 130, 132-133.

6.3.1 *The Bureaucratoscientific Field in Flux—Negotiating Boundaries*

The context in which the NSF began organizing itself was quite different from that when it was conceived by Bush, Kilgore, or anyone directly involved in its debates. During these, Waterman had been supporting the Bush group, although quietly.⁵¹ Once it was created, the NSF started operating in the unoccupied, or less occupied, spaces of the bureaucratoscientific field.⁵²

Against monopolizing: “federal coordination”—another word for developing a national science policy. Waterman, early in his appointment, insisted that the NSF would not evaluate federal programs and other agencies. A few reasons can be recounted. First, the NSF was very much an infant in comparison to other agencies and even smaller in comparison to the DoD’s involvement in applied research and development. Such a situation would invite possible conflicts between agencies. Besides this, however, was the fear that such coordination would entail possible political control. Such coordinative or supervisory roles⁵³ necessitated a somewhat explicit definition of the government’s national science policy. This would necessarily bring into being a controlling mechanism due to its categorizing, standardizing, and quantifying criteria (e.g. in reviewing other agencies’ project evaluations). Finally, this extended the NSF’s role way beyond what was basic research. This resistance was an awkward situation for the Administration since it was the NSF’s statutory responsibility. However, that Waterman and the Board acted as a block in this regard made it possible to resist the Bureau of the Budget’s pressure. As part of this, but also the fact that other agencies had occupied much space, the NSF expressed

⁵¹ Ibid, 131.

⁵² See: Geiger, *Research and Relevant Knowledge*, 19.

⁵³ These two different terms are used sometimes interchangeably but sometimes to indicate degrees

that while it would take the lead in supporting basic research, other agencies were welcome to sponsor research in fields directly related to them. Ultimately, the NSF policy-in-action would be to avoid developing a clearly stated national science policy.⁵⁴ Ironically, the Bureau of the Budget, responsible with policing agencies' budgets, ended up being (to a small degree) involved in developing evaluative criteria for the supervision of research agencies. Out of this effort came the distinction of programmatic and non-programmatic research—distinct from that of the dichotomy of basic and applied research:

This distinction differed from the conventional one between basic and applied, since occasionally programmatic research was basic —e.g., “research in nuclear cross sections which is essential to the development of improved nuclear reactors”—while some applied research was not essentially related to the programs of sponsoring agencies—e.g., much medical research. “In general,” the paper said, “programmatic research is the kind of research which an agency can justify as being an integral part of a program to solve an immediate agency problem. It is characteristic of such research that it can be planned in some detail and that the desired results of such a project can be related closely to a previously agreed upon goal.”⁵⁵

The NSF demonstrated its stance to Congress as well, as it rejected Congressperson Dodge's call for a study in R&D, arguing that development was beyond its responsibilities (an argument fit for the circumstance). However, the perceived possibility of the NSF monopolizing basic research caused fears of a drop in basic research support among scientists and drafts of an executive order to this effect attracted much anger from agencies, forcing the Bureau of the Budget to make changes. Finally, the NSF's role of general federal support for basic research was approved by the Executive Order No. 10521 in March 17, 1954. It was a success from the NSF's viewpoint. The Foundation's policy

⁵⁴ England, *A Patron for Pure Science*, 131, 149-150, 181, 184-188.

⁵⁵ *Ibid*, 190.

development and evaluation role was defined to be cooperative rather than regulative and they proceeded to focus on their proper role of supporting research.⁵⁶

Against others monopolizing: disciplinary and research scope. As Emirbayer and Johnson have mentioned, a *space of struggle for organizational power* can be considered as a meso-level subset of the *field of power*.⁵⁷ This is an important part of the bureaucratoscientific field and was the case regarding postwar federal science support. The Bureau of the Budget, for example, wanted to exclude medical research funds from the NSF, which was successfully thwarted by Waterman and the Board. Most such funding was already carried out by the NIH. Furthermore, just like other agencies “conspired” against itself before and during its establishment, so did the NSF cooperate with the DoD to break the AEC’s monopoly on nuclear research. Finally, the Board did not want defense research to be excluded from its agenda (despite the opposition from NSB Chair Conant). Regardless, the Board was also wary of undue military influence. In conclusion, they ended up not creating a defense research division due to (1) the secrecy issues it would pose and (2) the further evaluative roles it would bring regarding federal military research. Their existent influence of civilian scientists over defense research through the membership of Conant, DuBridge, Loeb, and Waterman in the President’s Science Advisory Committee (PSAC).⁵⁸

⁵⁶ Ibid, 195-196, 198-201, 203, 311, 353-355; Geiger, *Research and Relevant Knowledge*, 160-161.

⁵⁷ Emirbayer and Johnson, “Bourdieu and Organizational Analysis,” 13-14.

⁵⁸ England, *A Patron for Pure Science*, 135, 143-144, 147-148, 292-297.

6.3.2 *The ‘Who’ and the ‘How’ of Supporting Research*

Project and institutional grants, “bigger” science, fellowships, and other forms of support. The Board approved project grant as the usual form of research support and set overhead compensation at 15%.⁵⁹ Despite the plurality of research support in the government, the NSF was the second agency to do so through grants after the PHS. A grant approach was thought to be more conducive to basic research (rather than contracts) and project grants to the NSF’s mission (rather than institutional grants) since funding was primarily meant for research and not education in general (the fundamental relation between research and education would amplify this role later on).⁶⁰ For research proposals,

The sequence of events was: an independent evaluation of each of fifty-eight proposals by at least three expert reviewers: a two-day meeting of a screening panel of eleven consultants expert in the fields represented; a meeting of the newly formed Divisional Committee regarding general policy and program within the field of the Division; and consideration by the full staff of the Director.⁶¹

These would then, in summary form, be presented to the Board, who would give the final decision on all proposals. A proposal would be rejected without a review if it was (1) applied, (2) in clinical medicine, or (2) deemed incompetent.⁶²

The NSF did not dabble in the Big Science that characterized the enormous laboratories operated or funded by the AEC. It did, however, begin supporting “bigger” science after 1954, which caused concerns that this would squeeze out support for individual research. Fields like oceanography, geophysics, and atmospheric sciences demanded much more

⁵⁹ Overhead expenditure are the expenses that cannot be itemized but only calculated at an aggregate beyond a specific project, such as utility bills and infrastructure maintenance.

⁶⁰ Ibid, 162-164.

⁶¹ Ibid, 166.

⁶² Ibid, 165-166, 174.

expensive equipment.⁶³ The Board adopted the following principles regarding large basic scientific undertakings:

1. The NSF should recommend as a national policy the desirability of government support of large-scale basic scientific facilities when the need is clear and it is in the national interest, when the merit is endorsed by panels of experts, and when funds are not readily available from other sources.
2. A national astronomical observatory, a major radio astronomy facility, and university research installations of computers, accelerators and reactors are examples of such desirable activities for NSF.
3. Funds for such large-scale projects should be handled under special budgets.⁶⁴

The final provision was particularly added against the possible risk that these might pose to individual research grants. To their credit, the Foundation and the Board did not allow such concerns to come true. This can be seen below in the budgetary growth of the contract program in Table 3.

Divisions	1952	1953	1954	1955	1956	1957	1958
Basic research grants							
BMS	\$0.8	\$0.8	\$2.0	\$3.6	\$4.8	\$7.4	\$8.5
MPE	0.3	1.0	2.0	4.4	4.7	7.6	9.4
Social sciences	-	-	-	-	-	0.3	0.6
Antarctic	-	-	-	-	-	-	0.9
Research facilities							
BMS	-	-	-	-	0.1	0.9	1.0
MPE	-	-	-	-	0.4	4.5	5.0
Education							
Fellowships	1.5	1.4	1.9	1.8	2.1	3.4	5.6
Other	<0.1	<0.1	0.2	0.3	1.4	10.9	13.7

Table 3: NSF Obligations, 1952-1958 (in millions)⁶⁵

⁶³ Ibid, 279-280.

⁶⁴ Ibid, 282.

⁶⁵ Ibid, 222.

The NRC already possessed its machinery to administer fellowships. Accordingly, the Foundation decided to enlist their help and avoided duplicating it. This was also a specifically recommended provision of the Moe Committee in *The Endless Frontier*. Here, Waterman struck a balance between quantity and quality rather than giving less stipends to more students. The Foundation also departed from previous federal fellowship programs by investing in first year graduate students, reflecting an aim to attract talent into science and engineering. Finally, Conant had been pushing for undergraduate scholarships since *The Endless Frontier*, which finally came to be included in the Foundation's budget request in 1953.⁶⁶

Finally, as part of its legal responsibility regarding scientific info, the Foundation helped some important periodical publications in crisis, made Russian science more accessible in English, and provided international travel grants.⁶⁷

Disciplinary and geographical distribution. The natural and biomedical sciences dominated all forms of support by the NSF. The amounts and percentages of research support can be seen below in Table 4. In the context of its “permissive but not mandatory” role regarding the social sciences, the Foundation slowly began in 1954 to support it with some internal pressure. This support, including fellowships, was only field “convergent” with the natural and mathematical sciences, moving beyond this limitation in 1958.⁶⁸

⁶⁶ Ibid, 145-147, 168, 170; Bush, *The Endless Frontier*, 147.

⁶⁷ England, *A Patron for Pure Science*, 175, 177.

⁶⁸ Ibid, 170, 232-233; Harry Alpert, “Science, social psychology, and the National Science Foundation,” *American Psychologist* 12, no. 2 (1957): 95-97.

Divisions	Amount	% of total
<i>Biological and Medical Sciences</i>	\$28,126,312	47.7
Developmental biology	1,573,232	2.7
Environmental biology	2,477,660	4.2
Genetic biology	2,365,100	4
Metabolic biology	1,482,350	2.5
Molecular biology	6,142,780	10.4
Psychobiology	3,411,450	5.8
Regulatory biology	5,866,825	9.9
Systematic biology	3,142,105	5.3
General	1,664,810	2.8
<i>Mathematical, Physical, and Engineering Sciences</i>	\$29,553,300	50.1
Astronomy	2,733,530	4.7
Chemistry	8,083,800	13.7
Earth sciences	3,334,820	5.7
Engineering sciences	4,917,050	8.3
Mathematics	3,834,200	6.5
Physics	6,523,900	11.1
General	126,000	0.2
<i>Social Sciences</i>	\$1,323,450	2.2
Anthropology and related sciences	722,400	1.2
Economics	93,300	0.2
History and philosophy of science	66,450	0.1
Sociology	182,100	0.3
Socio-physical sciences	259,200	0.4
Total	\$59,003,062	100

Table 4: NSF research grants, FY 1952-1958⁶⁹

A similar concentration of funds was also occurring in geographical and institutional distribution. 9 Northeastern states received 32%, 12 North Central states 32%, 16 Southern states and the District of Columbia 19%, and 11 Western states 16% of research funds by

⁶⁹ England, *A Patron for Pure Science*, 258.

1954 (the remaining went to American territories and researchers abroad). The concentration in elite institutions was obvious in the upper echelons and still relatively high a little down the list, which also signaled concentration in the Northeast, East North Central, and Pacific Coast regions.⁷⁰ Such institutions whose researchers obtained grants of more than \$100,000 can be seen below in Table 5.

1. University of California, Berkeley	\$292,400
2. Yale University	245,750
3. Harvard University	231,000
4. University of Wisconsin	218,920
5. University of Illinois	211,600
6. University of Chicago	189,000
7. University of Pennsylvania	173,100
8. Princeton University	131,300
9. University of Minnesota	129,680
10. Washington University	127,200
11. Purdue University	125,000
12. Indiana University	122,900
13. Columbia University	119,900
14. Pennsylvania State University	116,400
15. Johns Hopkins University	112,400
16. California Institute of Technology	106,500
17. University of Washington	106,500
18. Northwestern University	103,500

Table 5: Universities Receiving NSF Research Grants of \$100,000 or More (by 1954)⁷¹

⁷⁰ England, *A Patron for Pure Science*, 203-204.

⁷¹ Ibid, 204-205.

While a concentration also was present in the universities from which baccalaureate graduates received NSF fellowships for their graduate studies, this became even more highlighted when these students landed in the institutions of their preference. This can be seen below in Table 6.

1. Harvard University	171
2. Massachusetts Institute of Technology	163
3. University of Chicago	146
4. California Institute of Technology	110
5. Princeton University	103
6. University of Illinois	102
7. University of Wisconsin	102
8. University of California, Berkeley	96
9. Columbia University	78
10. Yale University	74
11. Stanford University	67
12. Cornell University	58
13. University of Michigan	48
14. Johns Hopkins University	45
15. University of Minnesota	40
16. University of California, Los Angeles	35
17. Ohio State University	32
18. Purdue University	30

Table 6: Twenty Institutions Receiving the Most Amount of NSF Fellows (by 1954)⁷²

Such distributions attracted criticism of the obvious sort, to which Waterman responded with the circumstantial difficulties and the inherent necessities of the situation. He argued that the Foundation was not funded sufficiently to be able to support less well-to-do institutions and regions without taking money away from where it would be used most productively. In regard to fellowships, the Foundation had even less control of the situation.

⁷² Ibid, 206-207.

The BA graduates who obtained fellowships already hailed from some colleges as well as prestigious universities—this was fundamentally different, however, from the institutions to which they ended up going (which the most amount of disparity existed). To enforce equality here would be to force the students into institutions they did not prefer.⁷³ The policy of best science naturally favored those perceived to have more potential, and both the Foundation and the Board concurred that distribution could come only after a satisfactory amount of quality had been ensured. As Conant put it in the NSF's first annual report: "In the advance of science and its application to many practical problems, there is no substitute for first-class men. Ten second-rate scientists cannot do the work of one who is in the first rank."⁷⁴ This also suggested, not always but most of the time, that other distributive measures (i.e. disciplinary) could also encroach upon the quality of research. As Louis Levin, program director for regulatory biology, put it:

There seems little doubt that 'masterminding' research by allocating money to specific areas, whether they be called programmatic work, the filling of gap areas, or by other names, results in work of lower quality than if the problem is chosen by the investigator only because he has an interest in it and has ideas about how to attack it . . . the overriding criterion is that of scientific merit.⁷⁵

⁷³ Ibid, 203-204, 230-231.

⁷⁴ Ibid, 227.

⁷⁵ Ibid, 261.

THE POSTWAR ERA: A DISCUSSION

By World War I, the organizational field of higher education had subsumed much of the social space of the scientific field in the United States. By World War II, its universities had become an international powerhouse in part through its organization, which was significantly more favorable to research than that of its European counterparts. However, neither the US nor other countries had a national support system that resembled the post-war order of things. This was made possible, then likely, and then achieved through an amalgam of unexpected events, conscious efforts, and unintended consequences. It took place in the field of power, where the scientific leadership successfully produced an official problem, and *nominated* and legitimated the status of basic research through the federal government. Comparatively powerless in the field of power, and especially regarding the sort of science that has no practical implications in real-time, scientists enjoyed a historically unique amount of symbolic power that the looming bureaucratoscientific field, in addition to the prestige retained from their WWII involvement, delivered. The cultural, informational, and scientific argumentation (effectively the effort to redefine the value of the relevant species of capital) to this legitimation is what I call *basic research ideology*; and its statist (juridical, administrative, and bureaucratic) consecration is the *basic research consensus*. How did this come to pass? Simply put,

In general, the struggles center not so much on costs and benefits but on the definition of the problem, i.e., on the question of which rationality is to apply. The partners are only too well aware that once this definition has been established, the orientation of the eventual decision will already have been tightly structured.¹

The definition of the problem can be formulated as it had historically occurred, but for analytical purposes, it can also be abstracted into a form that becomes applicable for many,

¹ Crozier, and Friedberg, *Actors and Systems*, 204.

if not all, national contexts. It is possible to frame a much more detailed argument in geometrical form, but I confine myself to its essence. Basic research ideology can be broken down in the following way:

1. Basic research and its necessary correlates, i.e. its dissemination, practitioners, and facilities, must be retained in sufficient quantities for public health and welfare.
2. The United States must be “self-sufficient” in the amount of basic research done in order to avoid the risk of depleting the pool of fundamental knowledge.²
3. Basic research requires favorable treatment because “applied research drives out basic.”
4. The federal government is the only entity without special interests large enough to support basic research sufficiently.
5. Basic research is fundamentally best performed in universities.
6. *Therefore, the US federal government must finance basic research and its necessary correlates primarily in universities to promote public welfare.*

However, a new rationality will become ineffective unless there are groups to represent it and there are structural accommodations.³ Both of these came together—the OSRD and wider scientific leadership and the empirical demonstration of their distinction, i.e. World War II. Of course, a sort of rationality for the federal support of science (broadly conceived) came into being after World War I, but lacked an authoritative leadership and the breadth of the structural changes brought by WWII. These latter changes became visible even before the war ended, as exemplified by Kilgore’s early science legislations, but such

² This has more implications for countries whose basic scientific literature is disproportionately devoid of their native languages and creates possible difficulties in its production and the transfer of basic into applied science.

³ Ibid, 206.

immediate manifestations highlighted the lack of representatives in harmony with the scientific field. The free market approach that preceded this, on the other hand, lost some influence after Herbert Hoover's bid for the presidency and, more importantly, were structurally hindered by the free market itself. The OSRD experience tilted the balance overwhelmingly in favor of federal patronage and decisively shaped the scientific leadership, whose *basic research ideology* became the mantra of science policymakers. Basic research ideology became the bedrock of much of the bureaucratoscientific field until "compromises" with applied research were increasingly structured in during the late 1960s and early '70s.

To make one very important point clear, the scientific leadership carried a *capital of social authority* rather than a *capital of strictly scientific authority* when it came to the precepts of basic research ideology. A mathematical proof or the demonstration of nuclear fission were radically different in their epistemic certainty from that of the relation between basic research and public interest. This does not amount to saying that the thesis was wrong—based on a good number of anecdotal evidence and inductive reasoning, it was and is quite convincing. And if there were a group of experts who actually observed part of the hypothesis in action and therefore had some insight, it would be scientists and technologists. The irony of the almost absolute priority given to applied research and development in the OSRD was that they had witnessed in pieces the transformation of basic into applied knowledge. Regardless, the most convincing part was the authority that the scientific leadership enjoyed, in part due to their insight granted by their scientific competence and largely due to the respect they commanded. The details of the programs they proposed were disputed, but their essential claims, of which the strictly scientific legitimization politicians could not fathom in the slightest, were accepted faithfully. By this way did the scientists partially shift the legitimacy of the species of capital in the field of power. This is how basic research ideology became the basic research consensus, until it slowly dissipated after the late 1960s.

The impact went significantly beyond the United States, however. The federal grant system that basic research ideology had facilitated became the international standard and attracted much attention. In 1963, during the second "golden age" of basic science in the US, OECD countries met with the representation of their science (or equivalent) ministers

and urged more involvement in science policy.⁴ Jacques Monod, French biochemist and Nobel laureate, himself later suggested that France adopt the US departmental system in addition to its federally supported postdoctoral fellowship program.⁵ It was further telling that Monod, as well as Lwoff and Jacob, all Nobel laureates, had received approximately \$200,000 for about 10 years from the US federal grant system.⁶

There were, however, also things in some of the leading scientists' agenda that did not make it into the fold. Most conspicuous was Bush and his colleague's desire to instill civilian oversight for cutting-edge military research. This had all the operational barriers a science foundation had (e.g. Republican criticisms of enlarging the government, relative unimportance in comparison to other controversial legislation), but it additionally had two factors making it more difficult to attain. First, the idea was related but categorically different to the assertions of basic research ideology. It is no surprise that this did not coincide with what President Roosevelt asked in his letter to Vannevar Bush. Accordingly, the priority given to the proposed foundation on the legitimation of basic research ideology made civilian oversight secondary. Second, while the Foundation had opponents and proponents, the idea of civilian oversight had one less proponent and one more opponent—the Services. Scientists were able to “trespass” into military territory before due to an emergency and explicit presidential support, but circumstances were different then. Another item on the agenda was a body responsible in evaluative and advisory capacity. This also had a difference of priority attached to it, but in a sort of reversed manner. Unlike civilian oversight, this was conceived to be an organizationally separate unit—the Science Advisory Board—but ended up being established as part of the NSF. What finally caused this

⁴ S. Santhiran, “Chronicle,” *Minerva* 2, no. 2 (Winter 1964): 263. For the meeting that occurred the year after, see: “Chronicle,” *Minerva* 2, no. 4 (Summer 1964): 531.

⁵ V.K.M. “Jacques Monod: Further Comments on French Universities,” *Science* 150, no. 3704 (December 24, 1965): 1701.

⁶ Daniel S. Greenberg, “Foreign Research: Reuss Criticizes Projects Supported by the United States,” *Science* 151, no. 3707 (January 14, 1966): 180.

role to be part of the NSF was an ironic one—Republican notions of small government (treated below).

Discussing the principal debates and some conclusions. Each of the five principal issues regarding federal patronage of basic research followed different paths. However, most take a different form when the issues are expanded to cover more than the NSF. Most studies, of which the most important have been cited in this thesis, limit the discussion to the NSF because these were conceived in relation to it. However, that conception presumed that the NSF would have a virtual monopoly over basic research—otherwise the efforts to protect it from political control would become meaningless. Even though the AEC was significantly oriented toward applied research, there was a strong element of basic science in nuclear research and this necessarily took some amount of basic research out of the jurisdiction of the NSF. The same applied to the NIH. Accordingly, these issues must be dealt with as they relate to the bureaucratoscientific field.

The issue of federal domination was the most important immediate issue. Presidential control, from the perspective of the OSRD leadership, corresponds to the federal support of *basic research*, since (1) applied research was not included and (2) the NSF was to lead the civilian effort in military research. The issue, however, devolved beyond the Foundation once the plurality of federal support began crystalizing. First of all, the NIH, through the PHS and the Department of Health, Education, and Welfare (HEW), was already under presidential control. The extension of research grants over the entire NIH was established more easily because (1) there was not a question of creating a new agency and attracting the ire of Republicans, (2) there were not conflicting proposals on which different groups clashed, (3) the PHS acted as a united front (which was organizationally easier anyhow), (4) the PHS's perspective aligned with that of the Administration, (5) medical sciences enjoyed a privileged status from the lens of public interest, and (6) research grants were

already present in the NIH but simply limited to cancer related studies.⁷ The Atomic Energy Commission was also under presidential control, significantly more than others, which was exerted through its members who were incredibly powerful under their legal mandate. This brought less dissent than the NSF due to two reasons. First, the security risks posed by atomic research put the AEC on a completely different level of discussion. Second is the issue of the limited energy and attention of participants. Even though there was the involvement of renown scientists (through petitions against an overly securitized agency), administrator-scientists, particularly the OSRD leadership, were rather occupied with the NSF. This also highlights the difference between scientists and administrator-scientists, particularly in terms of social capital, scientific capital of social authority, and political capital (the latter were much more familiar with the corridors of Capitol Hill). The only exception to presidential control, and for a time being, any external control, was the ONR. Despite the element of chance, the ONR is still a legacy of the OSRD at large. The circumstantial events that led to the creation of the ONR bear the imprint of the transformed fields and social structure. The fact that ONR staff scientists all agreed to support basic science even though disputing its reasoning suggests this. Basic research ideology was a significant part of this reasoning, although not completely, and effectively operated as if it categorically adhered to the basic research consensus.

The NSF too came to be under presidential control and witnessed the strongest clash between different fields. Here, however, there is an interesting irony. The Democratic Administration and their likeminded Congressional allies were criticized by conservative Democrats, scientists, and Republicans, who challenged presidential appointment. However, the final bill Republicans shaped (H.R. 4102) effectively extended presidential control even further by giving the supervisory role to the NSF. At the fiscal level, this was a conservative measure to ensure that the NSF would evaluate other agencies to ensure that they would not be wasting money. However, this invited pressure from the Administration

⁷ My conclusions must be supported with more data, however.

to carry out this role, which was resisted due to a desire to avoid conflict with other agencies and the possible interference it might bring into scientific research. The Bush Report had already envisioned this as a separate body in a manner acceptable to scientists, which rather ended up as an unresolved issue that annoyed the NSF and the NSB. The necessity of the arrangement suggested by the Bush Report was realized later on.

Despite some distasteful events, federal domination did not occur. Academic freedom was preserved even during the presidency of Richard Nixon, who had abolished his Office of Science and Technology.

The issue of geographical distribution was the most important long-term issue. Along with the issue of presidential control, it was the most relevant to the conflict between the political and scientific fields. It was an issue that slowly gained momentum during the postwar era but was to become central in the post-Sputnik era, in which the enlarged budgets of the NSF would allow it support university development programs with minimal sacrifice from its best-science policy. The National Defense Education Act of 1958 (NDEA) by the Eisenhower Administration was a step in this direction, albeit a qualified one. The paradigmatic shift would later occur with the National Education Improvement Act of 1963 (NEIA) by the Kennedy Administration, which was to be followed by amendments and extensions by President Johnson to its apogee.

The issue of social science, contrary to others, was passively integrated and enlarged in time. This result was primarily due to the contrast between the parties involved in the debates after each world war. After WWI, the natural scientists had the authority conferred by their traditional leading role in the NAS, whereas the social scientists commanded respect due to their proximity to the administration (in the context of the New Deal). There, natural scientists allied themselves with some Congresspersons against the social scientists who allied themselves with the Administration. After WWII, however, the disparity of symbolic power between the two groups had become so wide that social scientists ceased to be major actors in the debates. Here, for the most part, the natural scientists allied themselves with Congresspersons against the Administration and the Congresspersons they supported. An important point this makes is the distinction of some disciplines from other disciplines conceived as 'science,' or at least 'scientific,' in the forms of capital re-legitimated in the field of power. This is fundamentally due to a difference of social

ontology, something which I believe to be inimical to Bourdieu's understanding of social reality, yet in complete harmony with historical relationalism.

The issue of patents was directly related to the issue of the scope of federal patronage and had ironically become obsolescent for basic research rather soon. Despite the controversy caused by Congressional deliberations, patents never attained the level of attention geographical distribution received. Even though it underwent some regulation during the Kennedy and Johnson administrations, agencies dealt with on an ad hoc basis as it did not pose a serious problem. It took a significant shift of policy in the NSF for patents to become an important issue once the NSF was oriented to incorporate applied research after the late 1960s.

Basic research ideology in the post-Sputnik era. Regardless, the postwar era did not go far in supporting basic research taken together in comparison with the years to come. The Korean War, budget stringency, and McCarthyism made it difficult for supporting science. It was, of course, certainly a momentous victory for basic science and made possible what was to come. Once Sputnik I was launched, the NSF was prepared to seize the moment. The launch exemplified the insufficiency of US investment in basic research—rocket and satellite systems had not been supported due to being categorized as basic research, since direct military applications were not foreseen.⁸ The Eisenhower administration took a sharp U-turn from the budget cuts to R&D and particularly piled up funds for basic research, which was to be increased to even greater heights under the Kennedy administration. Pressure toward a greater geographical distribution was particularly exerted by the following Johnson administration and the NSF picked up some development programs particularly aimed less well-to-do universities. Basic research's place in the federal R&D budget soared to its proportionate peak in 1968, during and after which an increasing tendency toward applied research followed debates regarding the transfer of basic research

⁸ Geiger, *Research and Relevant Knowledge*, 161-162.

into technology. Accordingly, it can be said that basic research ideology had reigned during the years 1945-1968, the final year marking the end of the post-Sputnik era.

As aforementioned, the NSF tried to shake off its evaluative policy role, which brought the President's Science Advisory Committee (PSAC) to the fore. Its chair, the Special Assistant for Science and Technology to the President, assumed the role of the presidential science advisory officially for the first time after the launch of Sputnik. Drawn from the higher echelons of the scientific community, the PSAC shared their belief in basic research ideology. However, even though effectively assuming the coordinative role in science policy, they too would refrain from exerting active involvement in the deliberations of federal agencies, arguing that scientific discovery could not be planned ahead, just like the NSF. In 1959, attended by leading figures from the government, industry, and higher education, basic research ideology was 'consecrated' at the Symposium on Basic Research.⁹ Soon, the Federal Council for Science and Technology was established to have an official body where federal agencies could coordinate with each other at the leadership of the presidential science advisor. Even though the NSF's statutory responsibility legally continued, it was unofficially relieved from it.¹⁰

A theme prominent in the post-Sputnik era was the rising involvement of Congress in science policy. Once science legislation was completed by 1950, Congress had been only involved in security related issues, and as science deliberation revolved around the administration, Congress became more critical as R&D budgets rose. Even though the administration-centered science deliberations were exaggerated by Congresspersons (the administration was involved in budgetary deliberations through the Bureau of the Budget but federal agencies had significant leeway in internal policy deliberations), Congress had

⁹ "Strengthening Basic Research," *Science* 129, no. 3358 (May 8, 1959): 1266-1267; "Eisenhower Speech Highlights Basic Research Symposium," *Science* 129, no. 3361 (May 29, 1959): 1475-1476; Geiger, *Research and Relevant Knowledge*, 169.

¹⁰ "Science Advisory Committee's Recommendation for Science Council Being Implemented by Executive Order," *Science* 129, no. 3350 (March 13, 1959): 708-709; George T. Mazuzan, *The National Science Foundation: A Brief History* (Washington, D.C.: National Science Foundation, 1988), 17-18.

been really left unaware of the government's role in R&D. Accordingly, Congress reorganized its committees, acquired a professional staff for science, and used hearings to scrutinize federal agencies support of research. Administration response to such requests was affirmative, however, and the PSAC was transformed into the Office of Science and Technology (OST) in order to give Congress access to it. The NAS also joined the fold by going beyond its historically (and legislatively) passive role to get involved in policy deliberations, which also used Congress's increased interest to gain access by lending its hand.¹¹

The social sciences also slowly made way to obtain a larger role in the NSF, ironically for which Harry Alpert (a sociologist at the Foundation) utilized Bush's arguments.¹² More important, however, was the problem of "scientific manpower," which elevated a part of basic research ideology to an unforeseen level of priority. Accordingly, fellowships and scholarships, as well as development programs, came to occupy a more important place. First highlighted by Sputnik I, it was exacerbated by the Cold War. A somewhat contradictory picture of the USSR was influential in this regard. Due to centralized management (though not compulsion), the output of scientists and engineers in the USSR was significantly larger, even though its enrollment rates were smaller than that of the US (also due to a lack of liberal arts education). Numbers was arguably the most important possible way of comparing two such large aggregations, which left other dimensions wanting. The centralization of Soviet science, particularly through the Academy, caused strains and difficulties in scientific inquiry. The USSR also did not enjoy the same level of means for scientific disseminations. Accordingly, they continued to lag behind the US in terms of

¹¹ "Congress Dissatisfied with the Executive's Role in Science," *Science* 129, no. 3365 (June 26, 1959): 1726-1727; Daniel S. Greenberg, "New Office of Science and Technology Proposed by Kennedy To Strengthen," *Science* 136, no. 3510 (April 6, 1962): 32-34; Mazuzan, *The National Science Foundation*, 13-14; John Walsh, "New Overseers for Federal Science," *Science* 142, no. 3589 (October 11, 1963): 210; Daniel S. Greenberg, "Academy and Congress: NAS Panel Completes Its First Assignment in New Relationship with Congress," *Science* 148, no. 3670 (April 30, 1965): 608-609.

¹² England, *A Patron for Pure Science*, 266-267, 269. Also see: "Science Foundation Establishes Social Sciences Division," *Science* 132, no. 3442 (December 16, 1960): 1824-1825.

basic research and achieve some level of parity in some areas, particularly in the applied domain. Regardless, the inherent difficulties in calculating a country's need for scientific and technical labor and international power anxieties may have caused the US to "over-produce" this pool of labor.¹³

The international power implications of basic research.

The relationship between basic research and national interests in basic research are somewhat paradoxical. Ironically, instances of promoting basic research ideology do not seem to try to conceal this, suggesting the possibility that promoters were not aware of it or at least disregarded it. The lack of awareness is further suggested by the lack of public discussion of the matter in the postwar era. Such issues came to receive more attention toward the end of the postwar era. This is of particular interest to international relations scholars. Basic research, if presupposed to operate under a scientific rationality (i.e. an autonomous field), consists of the free dissemination of knowledge and (to a lesser extent) the exchange of researchers. Since scientific knowledge is sought to become common knowledge, basic research is an international activity. However, at the same time, proponents of basic research ideology also argue that a nation must be self-sufficient when it comes to the production of basic science. In a scientific *modus operandi*, however, the replenishment of the pool of scientific knowledge for one country means the replenishment of the pool for other countries as well. Accordingly, what we have here is a free rider problem similar to what the NRC experienced with the National Research Fund but on an international scale.

¹³ Luther J. Carter, "Manpower: Output of Scientists and Engineers May Exceed Goals Set by White House," *Science* 151, no. 3711 (February 11, 1966): 666-668; "U.S. Technical Education," *Science* 123, no. 3204 (May 25, 1956): 928; "U.S. Specialists Describe Soviet Commitments to Education," *Science* 130, no. 3380 (October 9, 1959): 905-908; Daniel S. Greenberg, "Soviet Education: Newly Published Study Raises Some Disturbing Questions for the West," *Science* 135, no. 3499 (January 19, 1962): 204-205; Daniel S. Greenberg, "Science, Engineering Manpower: Uncertainties Cloud Nation's Future Needs," *Science* 135, no. 3500 (January 26, 1962): 301-303; "Wiesner Confirmed To Head New Science Office," *Science* 137, no. 3526 (July 27, 1962): 270-271.

That this was not realized as a problem does not mean that it is free of free riders. Considering that (1) decision-makers had many points of disagreement that called for more immediacy, that (2) literally everyone involved encountered actual science legislation (in peacetime) for the first time, that (3) the State Department was not involved in the debates, that (4) science and technology studies were not yet developed, and that (5) the most obvious scientific rivals to the US were underwent immense infrastructural damage, it is no surprise that such issues were not scrutinized. There is no reason to assume that government's will behave rationally. Rationality becomes even more elusive when it is in regard to specifics in scientific knowledge, since decision-makers lack scientific insight.

The free rider problem is not a pure one, however. Just as monopolies or oligopolies aggregately had an interest in supporting the National Research Fund, the US stood to benefit more from its shared knowledge for a few reasons. First, linguistic competence is a strong criterion of access for foreign scientists. However, this does not exclusively apply to different countries that share varieties of the same language. For example, Anglophone countries (and non-Anglophone countries that have a high proportion of English speakers) stand to benefit from American science more than others. Second, research infrastructure (including researchers) is a strong determinant in how much countries can make use of the research of others. Access to confidential nuclear research would be virtually useless until infrastructure is developed. Accordingly, there would be very few countries who stand to benefit from American research as much as Americans. Theoretically, then, free riding is a bigger problem for scientifically less developed countries. Practically, however, considering that scientific literatures are often near-monopolized by particular institutions and languages and their native speaking researchers, research conducted by researchers in scientifically less developed countries often go unnoticed for the most part anyway. The difference of scientific development was also pointed out by Nobel laureate Glenn T. Seaborg in a direct response to political concerns.¹⁴ Third, a country with a higher number of

¹⁴ "Views on Secrecy Given by American Nobel Prize Winners," *Science* 130, no. 3367 (July 10, 1959): 86.

publishers of original scientific work stand to benefit more, since these researchers presumably know their own work better than anyone else. Accordingly, even though researchers may utilize the scientific results of others, original authors are best suited to help realize the potential of their research, be it in industrial applications or raising students in that particular subject. How can one explain, then, the importation of the American model of supporting scientific research in other countries? Institutional isomorphism stands as the best explanation¹⁵—not only for countries scientifically less developed, but even those better suited to benefit from original work. As my thesis demonstrates, national interests in academic science is a *reasonable belief*. American success in scientific research, which rose on more than a successful system of federal patronage, was very important in regard to the later cross-nation isomorphism of supporting science.

Another variable (also involved in the free rider problem) is the concentration of original works in particular languages. Even though I accounted for linguistic competence, linguistic domination is another matter altogether. For example, even though linguistic competence suggests that a country benefits much more from its own research, other countries might heavily invest in a foreign language that might become a *lingua franca*. This is the case for English (and to a lesser extent for other languages), meaning that the gap of benefit from original work in English has decreased between English speaking and non-English speaking countries. Regardless, the reduction in the gap confirms the gap and the balance is presumably still in the favor of the originator of the work. This is suggested by President de Gaulle's calls for challenging English as the scientific *lingua franca*.¹⁶

However, power relations surrounding scientific knowledge are even more complicated than this. Just as scientific cooperation can enrich science, it can also enrich political opportunities. A government may seek both forms of benefits. For example, the AEC sent

¹⁵ DiMaggio and Powell, "The Iron Cage Revisited."

¹⁶ John Walsh, "De Gaulle: President of France Calls for a Harder Line in Behalf of French in International Science," *Science* 148, no. 3668 (April 16, 1965): 350.

non-classified data on atomic research 23 to countries in 1955 as part of President Eisenhower's Atoms for Peace program.¹⁷ Later, President Eisenhower received protests from Senators due to his intention to share with the world information gathered from prospective satellite launches.¹⁸ Despite Ike's expressed concern for science per se (which he has demonstrated on other occasions as well), sharing information also fits with political self-interest. One reason is to exert control over shared science and technology. As put by Daniel S. Greenberg, part of US support for non-nuclear countries' in developing nuclear energy was to ensure that it would not be used for nuclear weapons through having them agree to inspections.¹⁹ However, the Atoms for Peace program does not stand for a politically exemplary initiative, as political arrangements lagged behind technological advancements. China was a perceived risk in this case. The US lacked the immediate resources to halt the development of nuclear weapons even in other countries such as India, which had accumulated sufficient amount of nuclear capacity to build upon even if the US had cut its support.²⁰

Further research questions

There are other issues mostly uninspected and related to the study at hand that can be further researched, and may or may not add to a better understanding of the material here. One directly related research agenda would be past utterances and formulations reminiscent of basic research ideology. Most prominent is Abraham Flexner's famous essay, *The Usefulness of Useless Knowledge*.²¹ Originally published in 1939, Flexner discusses the indirect and unseen uses of seemingly useless knowledge (among other matters). Here,

¹⁷ "AEC Technical Libraries," *Science* 122, no. 3163 (August 12, 1955): 278.

¹⁸ "Secrecy in the Sky," *Science* 122, no. 3164 (August 19, 1955): 322.

¹⁹ Daniel S. Greenberg, "Atoms for Peace: Concern Growing That Program Is Spreading Means for More Nations to Build Weapons," *Science* 147, no. 3660 (February 19, 1965): 843.

²⁰ Ibid.

²¹ Abraham Flexner, *The Usefulness of Useless Knowledge* (Princeton & Oxford: Princeton University Press, 2017).

however, one finds a technically more limited discussion (than that of the natural scientists of WWII), and one that clearly goes beyond an economic theory as it incorporates music, art, and other human endeavors for their inherent worth.²² Most relevant is the absence of an invitation for the federal government. This applied to the scientific leadership of the interwar era as well, until convinced otherwise by Karl T. Compton. Going back further, I have located an acknowledgement of the economic returns of basic research by a statesperson before WWI—Secretary of the Treasury Karl Helfferich.²³ In the limited excerpts available, Helfferich makes no mention of government involvement. A survey of German sources might reveal interesting facts. It is possible that a federal system of research support did not emerge due to the lack of institutional organization in the German Empire, particularly in consideration of Prussia. Otherwise, it is known that an institutional environment of universities was already established. It is possible that such a model was too “innovative” at the time; yet it must be borne in mind that it was also the German Empire that created the first modern welfare state.

Further research in alternative conceptions, or *ideologies*, of higher education/campuses/science can be delineated to highlight differences and similarities. Locating networks of proponent individuals and institutions and the clashes of different conceptions through these might allow us to understand how different *modi operandi* came to dominate higher education.²⁴ After preliminary examinations of early US higher education history, I have noticed the following variables that can differentiate between ideologies: *scope of education* (exclusive vs. inclusive), *scope of curriculum* (holism vs. specialization), *academic focus* (teaching vs. research vs. service), *institutional experience* (curricular vs. extracurricular), *type of knowledge* (pure vs. applied), *aim of education* (cultivation vs. production), and *focus of subsidization* (private vs. governmental). Values of variables do not

²² Ibid, 76.

²³ Willis R. Whitney, “Research as a National Duty,” *Science* 43, no. 1114 (May 5, 1916): 631-632. It is possible that Helfferich became Secretary after he had said this, when he was the director of the Deutsche Bank.

²⁴ Such comparisons would be a contribution to institutional isomorphism studies as well.

have to be categorical, but often they do have a particular leaning. Historical comparisons also suggest that different conceptions can be aggregated into two groups: the *collegiate ideal* and the *universitarian ideal*, reflecting the historical tension between the two institutional modes. Possible examples of the collegiate ideal include the ideology of ‘liberal culture,’ as promoted by Woodrow Wilson, and the ideology of ‘muscular Christianity,’ seen in campuses after 1900.²⁵ Basic research ideology exemplifies the universitarian ideal, as well as the conception of ‘useful knowledge,’ which became the bedrock of American emphasis on the practical arts.²⁶

Another significant issue is the “importation” of the US model of federal patronage. As previously mentioned, there had been calls to other governments to support research in a similar manner, and the practice today is widespread. The influence of the American system on subsequent developments in other countries might be obvious, but its nature can be studied in relation to basic research. Today, both the US and much of the world supports both applied and basic research, but by the time the US had become the unquestionable leader in scientific research, applied research was still shunned as a matter of policy (excluding specialized agencies). How did this impact countries purportedly following the American example? Is it possible to see reflections of basic research ideology in these countries? These are worthwhile questions.

As previously mentioned, the US national science policy was one of basic research, even though not officially stated. This is based on the comparative priority given to basic, particularly in the case of the NSF, over applied science in research conducted by civilians for non-military purposes (once the DoD enters the picture, the statistics become disproportionately skewed to not only applied research but also to development). What made basic research as a policy unique was that the notion of economic return had

²⁵ Geiger, *The History of American Higher Education*, 377-378, 411-415.

²⁶ *Ibid*, 91-99.

been disrupted as understood by traditional economic theory. WWII had made the development of science policy likely and the basic research consensus possible, of which its achievement in the United States was arguably the most difficult among all industrialized countries.



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