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ABSTRACT

Improving Mankind:

Philanthropic Foundations and the Development of American University Research Between the World Wars

by

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This dissertation examines efforts by the largest American philanthropic foundations, particularly those established by Andrew Carnegie and John D. Rockefeller, to improve mankind by funding research in the fields of human behavior and biology. In this study I argue that during the period between the world wars foundation policies and practices revolved around three main themes: the formation of an "interlocking directorate" of foundation officers, scientific entrepreneurs, and university administrators; the promotion of the ideal of transcending disciplinary boundaries through "cooperation in research;" and the launching of a human engineering effort that was based on the premise that human problems could be investigated and attacked through scientific research. Throughout the interwar period, university research programs that were coordinated by well-connected scientific entrepreneurs, that pledged to cultivate interdisciplinary cooperation, and that fulfilled the goals of the human engineering effort received millions of foundation dollars. The case studies that form the centerpiece of this dissertation both exemplify the most successful grant applications of the interwar period and illustrate how the human engineering project unfolded over time.
The early phases of the human engineering project were based on the idea that humans could be improved through the investigation and control of behavior and sexual reproduction. Exemplary case studies for the earlier phases of human engineering include a multi-million dollar grant package for Yale University behavioral sciences, initiatives related to the eugenics movement, and support for the National Research Council Committee for Research in Problems of Sex. Gradually, foundation-sponsored human engineering was transformed into an effort to investigate and control living beings on a structural, chemical, and molecular level. Case studies that epitomize this later phase include grants for biological science research at Stanford University and University of Chicago, and especially the cooperative bio-organic chemistry and molecular biology projects that foundations helped to launch at the California Institute of Technology. My analysis of these case studies, viewed through the lens of the interlocking directorate, the cooperation in research ideal and the human engineering effort, elucidates intersecting social, intellectual, political and economic factors that shaped knowledge production in the United States.
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Preface

What accounts for the meteoric rise of academic research in American universities during the first half of the twentieth century? Since the reorganization of universities to emphasize research was largely a privately funded effort, the answer to this question requires an analysis of the intersecting histories of education, philanthropy, and science. To explain the development of American university research, then, it is necessary to investigate the philanthropic foundation managers, university administrators, and scholars who cooperated to launch a revolution in the sources, scale and rationale of research funding in American universities. As Robert Kohler argues, "It is not just the people who work in laboratories who do science, but everyone who takes part in sponsoring, producing, justifying, or making use of scientific knowledge." Without the philanthropic foundations, the story of the American university is incomplete.

The conspicuous role of philanthropic foundations in the transformation of American university research has begun to receive more attention in recent years. Still, the study of philanthropic foundations and their relationship to the production and organization of knowledge in the United States is relatively new. One of the leading scholars of foundation philanthropy, Ellen Condliffe Lagemann, has identified obstacles that limited research into foundations prior to the 1980s. Most importantly, foundations restricted access to their archives. As a result, the vast majority of foundation histories were favorable accounts written by officers or trustees rather than scholars. The new availability of archives, especially from the outstanding Rockefeller Archive Center,

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coupled with what Lagemann has dubbed “widespread disenchantment with approaches to intellectual history that presumed knowledge was a fabric of free-standing ideas that was largely self-generating and detached from surrounding social context,” has changed the circumstances for historians.\textsuperscript{3} It has become increasingly clear that an investigation of the production of knowledge in the United States requires an investigation into funding mechanisms, which in turn requires an investigation into the policies and practices of the largest philanthropic foundations.

My dissertation, which focuses on the effort by philanthropic foundations to improve mankind by funding research in human behavior and biology, builds on the excellent work done in recent years by a small group of historians who have combined the histories of education, philanthropy, and science, namely Lagemann, Kohler, Roger Geiger, Judith Sealeander, and the research team of Barry Karl and Stanley Katz. The work of these scholars has prompted questions that became central to my research: How did experts communicate their needs to the non-experts that controlled resources? To what extent did the concerns of the non-experts infiltrate the heavily guarded halls of academe?

In \textit{To Advance Knowledge: The Growth the American Research University, 1900–1940} (1986), Roger Geiger dedicates a chapter to the contributions that large foundations made to the transformation of American university research. Geiger contends that the allocation of foundation money was a principal determinant of the academic pecking order. Without foundation support, universities found it almost impossible to establish successful academic research programs because routine sources of academic income were inadequate. The businessmen who ran the foundations saw research as an investment,

\textsuperscript{3} Ibid., x.
and they sought low-risk, high-yield opportunities by supporting universities that already
had substantial resources at their disposal. The selected universities received ample
provisions: the foundations supplied endowment funds, built laboratories and libraries,
and supplemented professors' salaries. Foundations expanded their efforts on behalf of
research in these select universities after World War I and throughout the Great
Depression, and thus enabled schools including Harvard University, Columbia
University, the University of Chicago, and Yale University to create excellent research
programs despite harrowing economic conditions. Between the world wars, Geiger
maintains, foundation managers, the presidents of elite research universities, and
scientific researchers became interdependent.

I find Geiger’s analysis of the interlocking directorate that ran foundations and
universities to be intriguing, and one of my principal goals in this dissertation is to probe
more deeply into the exchange of ideas among the individuals who applied for or
allocated foundation grants. To what degree did foundation officers rely on scientists and
administrators at particular universities for advice and guidance, and how did their
relationships shape funding decisions? To what extent did personal and professional ties
between foundation managers and their favorite university leaders contribute to the
elevation of some research programs over others? Robert Kohler addresses these
questions in *Partners in Science: Foundations and Natural Sciences, 1900-1945* (1991)
by investigating the influence of foundations on a wide variety of scientific disciplines in
the United States and throughout the world. He offers valuable information about and
insights into the principal actors and institutions that shaped foundation philanthropy in
the first half of the twentieth century. Kohler’s chief contribution is to elucidate how
relationships between patrons and grant recipients formed over time, as well as how these relationships shaped funding for scientific research. Because he covers a vast amount of material from a wide-ranging set of projects, institutions, and countries, Kohler does not delve deeply into specific projects or policies. I endeavor to investigate several of the projects and policies that Kohler mentions briefly in more detail, which leads to my discovery of patterns in funding decisions and research rationales that relate to human engineering.

Aside from Geiger and Kohler, most historians of philanthropy have emphasized the impact of foundations on public policy or have criticized foundations for exercising undue influence over grant recipients. A number of projects that I analyze in this dissertation, including the biology program at the University of Chicago, have been neglected altogether by historians of education, philanthropy, and science. Yale behavioral sciences and the research programs at California Institute of Technology have received some attention, which I will discuss in Chapters Three and Five, respectively. I do not claim to fill in all of the gaps in the historiography, but I believe that my research will contribute to a rapidly expanding field of study. It is my intent to provoke additional research (including my own) into the ways that pervasive ideas about progress and the hope for improving mankind shaped the practices of foundation managers and university scientists, and thus authorized particular approaches to knowledge.
The growth of learning in nineteenth-century America was motivated and directed, in the first instance, by scientists, scholars, and institutional leaders who saw new organizations to promote knowledge as the fundamental instruments of material and cultural progress.—Alexandra Oleson and John Voss, *The Organization of Knowledge in Modern America*  

Chapter One  
The Development of the American Research University and the Advent of “Scientific” Philanthropy

Numerous historians agree that over the course of the nineteenth century American institutions of higher education underwent a profound transformation that culminated in the development of research universities. During this dramatic transformation, American university scientists, once considered hopelessly backward by their European counterparts, began to build research programs that could compete with the finest in the world. The pace and scale of change was remarkable, especially considering that American institutions of higher education had to rely almost exclusively on private sources of funding to support research. Although there were many factors that contributed to the rise of the American research university, reforms would have been impossible to implement without a massive influx of donations from wealthy industrialists turned philanthropists. At each stage of the reform process, from building

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the first science departments in the mid nineteenth century to expanding graduate programs and inaugurating new specialty fields in the late nineteenth century to establishing world-class research programs in the early twentieth century, philanthropic funding acted as a catalyst, fueling each development.

I contend that an exceptional faith in the ability of scientific research to improve mankind motivated wealthy industrialists turned philanthropists and, later, philanthropic foundation officers to funnel millions of dollars into American university research. The goal of improving mankind through science eventually led foundation officers to the fields of human behavior and biology, which I will explore in great detail in subsequent chapters. In this chapter, I will show how faith in the awesome power of scientific research to improve mankind began to take shape in the late nineteenth century, as scientists established professional authority and universities became centers for innovation rather than storehouses for “tradition.” The development of the research university wove philanthropists and scientists together and led directly to what became known as “scientific” philanthropy, an attempt to “cure evils at their source” through the investigation and attack of the roots of social problems.

The rise of the American research university and the advent of “scientific philanthropy” were the products of a growing trust in the authority of scientific expertise coupled with an increasing reliance on institutions run according to a corporate model. Scientists were increasingly relied upon to investigate and decipher everything from human behavior to technology. The trust placed in scientists rested on faith in the scientific method of inquiry, which was defined and protected by “communities of the competent” that scientists built in the late nineteenth century through professional
organizations and university departments. Professional scientists, as Charles Rosenberg contends, "provided the motivation and specific scientific knowledge necessary to crystallize in institutional form the amorphous enthusiasm of Americans for science and the progress it seemed to imply."³ The research university was the institution where the "crystallization" of science was fully realized.

Elite university presidents at the turn of the twentieth century believed that they were guiding the United States into a new era of higher education. In 1903, University of Chicago president William Rainey Harper noted, "We are celebrating in these days not only the twenty-fifth anniversary of the Johns Hopkins University, the completion of a quarter of a century of magnificent work by a great university; but we are celebrating likewise the close of the first period of university education in the United States."⁴ As the founding president of the University of Chicago, Harper had participated in the development of a major research institution that was supported entirely by philanthropic donations. He was confident that the priority placed on science combined with unprecedented levels of funding dedicated to higher education would help transform universities into centers of innovation. Harper celebrated "the many millions of dollars given directly for research and higher education," which would be supplemented by funds from "the new foundations which have recently come into being." As a result of the investment in research, previously unknown university scientists were becoming "famous for the work they have accomplished." "Surely there is reason to believe," Harper concluded, "that in the East, in the West, and in the Far West we are preparing to

³ Charles Rosenberg, No Other Gods: On Science and American Social Thought (Baltimore: Johns Hopkins University Press, 1997), 16.
enter upon a new period in the development of university education."5 Harper spoke of his hope for the future of the university, a hope that was explicitly linked to financial support, development of research, and expansion "of the highest educational ideals" far beyond their original home on the Eastern seaboard. He articulated the views of a generation of university presidents, who saw themselves as the pioneers in a march toward new educational frontiers. They had tremendous faith in the promise of universities, a faith that was shared by wealthy patrons, who would supply the necessary funds to educate a generation devoted to human progress through science.

The development of universities and the related devotion to science were made possible by a massive transformation in American society that took place over the course of the nineteenth century. Breakthroughs in applied science, engineering, commercial agriculture, and medicine demonstrated the value of specialized, technical knowledge to businessmen who reaped tremendous profits from revolutions in the transportation and communication industries. Professional scientists established intellectual authority firmly enough to convince Americans, and wealthy industrialists in particular, that they could find the keys to restoring social order in the turbulent wake of industrialization, urbanization, and increased immigration. With sufficient resources, well-structured institutions staffed by experts, and carefully directed scientific research programs, Americans of the Progressive Era came to believe that a vast array of social ills could be identified, investigated and attacked. These beliefs captured the imaginations of the two wealthiest Americans of the late nineteenth century, Andrew Carnegie and John D.

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5 Ibid., 153.
Rockefeller, and motivated them to donate their fortunes to higher education and research.\textsuperscript{6}

Today, we are comfortable with the idea that American universities are among the best in the world and that they derive their reputations from outstanding research programs and facilities supported by a mixture of public and private funds. Yet, the high priority assigned to American university research is a surprisingly recent development, dependent to a remarkable degree on reforms launched in the late nineteenth century with direct aid from private philanthropy. Prior to the mid nineteenth century, American "science" lacked a clear definition, an institutional home, and a consistent source of financial support. There were no professional scientists devoted to specialized fields of study. Rather, amateur naturalists, botanists and geologists funded their own work, which typically consisted of collecting and cataloging specimens. These naturalists were usually educated in small, denominational colleges that were designed primarily to prepare young upper middle class men to serve their local communities as ministers, lawyers, or gentlemen intellectuals.\textsuperscript{7} The traditional college curriculum reflected loyalty to Christian principles, classical humanism, and "mental discipline:" students learned Greek, Latin, rhetoric, logic, and history before taking a culminating moral philosophy course from the college president. While many colleges were revamping their curriculum throughout the course of the nineteenth century by gradually adding scientific and

\textsuperscript{6} Barry D. Karl and Stanley N. Katz, "The American Private Philanthropic Foundation and the Public Sphere 1890–1930," \textit{Minerva} 19, no. 2 (Summer 1981), 244.

\textsuperscript{7} Laurence Veysey, \textit{The Emergence of the American University}, 1–15.
practical courses, in the most prestigious colleges on the eastern seaboard, technical subjects were excluded and science-related fields like botany were given short shrift.\textsuperscript{8}

The early nineteenth century American college was hardly hospitable to the kinds of education that industrialists and businessmen prized. College courses in natural philosophy or natural history were strictly theoretical due to a perceived conflict between "science," which was considered an intellectual pursuit for educated elites, and technical training, which was denigrated as vulgar and unrefined. As Alan Trachtenberg argues, "During the earlier stages of industrialization, science and technology had seemed wholly separate and often antagonistic fields, theoretical scientists (often gentlemen amateurs) holding themselves aloof from either direct mechanical application or entrepreneurship."\textsuperscript{9} "Science" was the province of cultured gentlemen, while technology was the domain of inventors and tinkerers. The only similarity between the two groups was that both scientists and technicians were lone practitioners, operating outside any formal institution. There were few connections between the college curriculum and the pursuits of either "scientists" or technicians.

By the mid-nineteenth century, the classical college curriculum was falling into disfavor. In his seminal analysis, \textit{The Emergence of the American University}, historian of education Laurence Veysey contends that traditional colleges were overthrown by emerging universities due to "what might be termed Europhilic discontent, available wealth, and immediate alarm over declining college influence."\textsuperscript{10} A number of recent historians have contested Veysey's account of the sterility and weakness of the early


\textsuperscript{9} Alan Trachtenberg, \textit{The Incorporation of America: Culture and Society in the Gilded Age} (New York: Hill and Wang, 1982), 63.

\textsuperscript{10} Laurence Veysey, \textit{The Emergence of the American University}, 2.
nineteenth century college, and they have shown that the widespread perception of the traditional college as "a static institution" was greatly overstated.\textsuperscript{11} Still, in the mid-to-late nineteenth century, a broad cross-section of American society vociferously criticized the traditional college.

American scholars were painfully aware that their academic accomplishments lagged far behind those of Europeans. For a nation that was expanding economically at an astonishing rate, the disparity in intellectual capital between the United States and Europe became a source of frustration and embarrassment. The frustration was heightened by frequent, and legitimate, criticisms of colleges that labeled them little more than finishing schools for young men. The rising captains of industry were especially chagrined by what they saw as "useless" institutions that failed to prepare graduates for the world of business. As Andrew Carnegie argued,

While the college student has been learning a little about the barbarous and petty squabbles of a far-distant past, or trying to master languages which are dead, such knowledge as seems adapted for life upon another planet than this as far as business affairs are concerned, the future captain of industry is hotly engaged in the school of experience, obtaining the very knowledge required for his future triumphs. . . . College education as it exists is fatal in that domain.\textsuperscript{12}

Carnegie later joined the ranks of railroad and telegraph magnates who, infused with national pride and flush with newly acquired wealth, were willing and able to help

\textsuperscript{11}Helen Lefkowitz Horowitz, "In the Wake of Laurence Veysey: Re-examining the Liberal Arts College," History of Education Quarterly 45, no. 3 (Fall 2005): 420; See Roger Geiger, ed., The American College in the Nineteenth Century for a careful analysis of the debate.

\textsuperscript{12} As quoted in Laurence Veysey, The Emergence of the American University, 13–14.
American colleges compete internationally and become more relevant to a modern society.

Beginning with the Lawrence School of Science at Harvard (established 1847) and the Sheffield Science School at Yale (established 1860), wealthy industrialists turned philanthropists contributed large sums to institutions of higher education specifically for scientific and technical training. The Lawrence School at Harvard was constructed at the behest of Abbott Lawrence, a textile manufacturer turned railroad magnate, who donated the necessary funds because he wanted to encourage education in applied science, especially in the fields of metallurgy, chemistry, and engineering.\textsuperscript{13} Railroad mogul Joseph Sheffield, who supplied the funds for the Yale Sheffield School of Science, followed Lawrence’s example. Like Lawrence, Sheffield viewed his donation as an investment in practical sciences that would have a direct impact on the country’s future success. As historians Merle Curti and Roderick Nash have observed, Sheffield exemplified the industrialist turned philanthropist who earned his wealth by investing in transportation technology and, based on his business experience, “realized that traditional higher education was totally unsuited for training young men to meet the new demands in the world of affairs they would encounter outside the campus.”\textsuperscript{14} Sheffield was certainly not alone in his beliefs or his methods. He and other industrialists chose to invest in institutions of higher education for a number of reasons: to solidify their status as American gentry by endowing schools that bore their names, to train young men in the technical and administrative skills that would prove valuable for the urban-industrial age,


and to invest in the nation’s future. Funding institutions of higher education provided excellent opportunities for industrialists to achieve a combination of personal and professional goals, while investment by industrialists gave college trustees and administrators added impetus to address curricular inadequacies.

The investment by Lawrence and Sheffield in science schools contributed to a reform process that dramatically altered the nature of higher education in the United States. According to Laurence Veysey, the transformation of American higher education that the influx of funds helped to facilitate was necessary to overhaul an outmoded curriculum that was mocked by industrial leaders as frivolous, and by farmers and labor organizers as elitist and aristocratic.\footnote{Laurence Veysey, \textit{The Emergence of the American University}, 13–14.} While Veysey’s analysis has been challenged, his main point remains salient: the detractors of the traditional college both funded major changes and inspired the leaders of institutions of American higher education to re-examine their curricula.\footnote{Julie Reuben, “Writing When Everything Has Been Said,” 416; Roger Geiger, ed., \textit{The American College in the Nineteenth Century}.} As prevailing public opinion turned against the traditional college curriculum, the federal government passed the Morrill Act (1862), which provided funds to establish institutions for training in mechanics and agriculture. Businessmen subsidized new universities while states took advantage of Morrill Act funds to build agricultural and technical schools. Students flocked to these recently established universities, and enrollments in traditional liberal arts colleges like Columbia, Yale and Princeton faltered.\footnote{Laurence Veysey, \textit{The Emergence of the American University}, 4.} In Veysey’s analysis, traditional colleges were revivified by enterprising presidents, who made their schools competitive again by incorporating scientific research, graduate study, and electives into the liberal arts curriculum. Again,
although Veysey’s ideal type of the traditional college was incomplete (and, in some ways, inaccurate), few historians have convincingly questioned that American higher education underwent a “revolution” in the late nineteenth century, largely as a result of reforms launched by these presidents.  

The most significant leaders in the transformation of American higher education in the late nineteenth century were Andrew D. White, the founding president of Cornell, Charles W. Eliot, president of Harvard from 1869 until 1909, and Daniel Coit Gilman, the founding president of the Johns Hopkins University. For these reformer presidents, the concerns of industrialists who supplied funding for the universities certainly had some influence, but their greatest source of inspiration was the German university. White, Eliot, and Gilman had all earned advanced degrees in German universities, and they appreciated the German emphasis on “disinterested research.” Compared to state-funded German universities, where research was a high priority for professors as well as graduate students, American universities offered few opportunities for original investigation. The reformer presidents viewed the distinction between the two countries as an unfortunate imbalance, and they endeavored to replicate Germany’s model while combining it with the pioneer spirit that was pervasive in mid to late nineteenth century America. They became committed to education in the service of innovation. Cornell’s White, Harvard’s Eliot, and Johns Hopkins’s Gilman wanted to be leaders who broke

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new ground and helped to establish American universities as institutions that ranked among the best in the world, largely by incorporating science and original research.

The establishment of Cornell University in 1868, which was financed by a combination of Morrill Act funds and a donation from telegraph mogul Ezra Cornell, created an ideal environment for higher education reforms, including the promotion of research, technical training, and other curricular expansions.²¹ Cornell became a prototype for the modern American university, an institution that provided students with opportunities to learn the latest techniques in science, engineering, and commercial agriculture without sacrificing a liberal arts curricular base.²² Andrew D. White, Cornell’s first president, played an integral role in defining the university and committed himself to bringing the “scientific” and the “aesthetic” together with the “practical” in an open-minded, non-sectarian educational environment.²³ White created opportunities for students at all levels to pursue studies in fields as varied as animal breeding and Greek philosophy at a newly established and amply funded university. White’s tremendous success at Cornell alerted other college presidents to both possibilities and needs for reform.

Charles W. Eliot, a chemist who had participated in mid-nineteenth century efforts to establish science in higher education and had taught in Harvard’s Lawrence Science School, began to inaugurate major reforms immediately after becoming president

²²Laurence Veysey, The Rise of the American University, 82.
of Harvard in 1869.\textsuperscript{24} Eliot adopted White's approach by embracing a wide diversity of courses, though he incorporated fewer "practical" studies and more theoretical science classes. In his inaugural address, Eliot referred to the priority that science would have during his administration at Harvard:

The University recognizes the natural and physical sciences as indispensable branches of education . . . but it would have science taught in a rational way, objects and instruments in hand—not from books merely, not through the memory chiefly, but by the seeing eye and the informing fingers.\textsuperscript{25}

Eliot's reference to the natural and physical sciences as "indispensable" marked a radical departure from earlier conceptions of college curricula that viewed science as peripheral to the liberal arts. Moreover, Eliot embraced experimental, hands-on university science, signifying that he privileged innovation over tradition.

Throughout his tenure as president, Eliot focused increasingly on developing scientific research and graduate education at Harvard, and the idea that the university should be a center for original investigation became his overriding preoccupation by the turn of the century. As he wrote, "The university seeks new truth . . . Hence universities are places of research, of diligent inquiry for new or forgotten truth."\textsuperscript{26} Eliot's emphasis on "new truth" signified an authoritative shift away from previous views of education that had privileged tradition, discipline and piety above science or the advancement of knowledge. The reforming institutions of the late nineteenth century, led by Cornell and


\textsuperscript{25} Charles W. Eliot, "Educational Reform" (Harvard University, October 19, 1869), in \textit{Builders of American Universities: Inaugural Addresses, Privately Controlled Institutions}, vol. 1, David Andrew Weaver, ed. (Alton, Ill: Shurtleff College Press, 1950), 249.

Harvard, were breaking the earlier mold and recasting the role of higher education. It was not easy. Advocates of tradition were wary of the dangers of "science" because, to quote Laurence Veysey, "science, paraded nakedly, seemed vulgar; it appeared to denigrate the position of men in the universe."\(^{27}\) Eliot was accused by his detractors, principally Noah Porter of Yale University, of abandoning reverence and wonder to embrace the arrogant assertion that the natural world was comprehensible. Yet, science did overcome its position of "humble subserviency" to religious doctrine and moral philosophy within the university in the late nineteenth century, largely due to reformers like Charles W. Eliot and Andrew D. White. Science was to receive an even greater champion in Daniel Coit Gilman, the first president of the Johns Hopkins University.

The Johns Hopkins University, established in 1876 with donated funds from the railroad giant Johns Hopkins, was designed around faith in science. Again, a pioneering institution was enabled by a massive donation from a wealthy industrialist and led by a creative, young president with a German graduate degree. Andrew D. White and Charles W. Eliot pushed the trustees of the new university to select Gilman as president because they knew he would follow their example and act as a promoter of university science. They were not disappointed. When he assumed the presidency in 1876, Gilman listed twelve principles to guide Johns Hopkins. Several of the principles defended science and research as valuable educational enterprises: "All sciences are worthy of promotion . . . Religion has nothing to fear from science . . . The best teachers are usually those who are free, competent and willing to make original researches in the library and laboratory."\(^{28}\)

\(^{27}\) Laurence Veysey, _The Emergence of the American University_, 41.
\(^{28}\) Daniel Coit Gilman, "Inaugural Address" (Johns Hopkins University, February 22, 1876), in _Builders of American Universities: Inaugural Addresses, Privately Controlled Institutions_, vol. 1, David Andrew
With his statement of principles, Gilman directly challenged the assumptions of
traditionalist educators, particularly the idea that science undermined religious belief.

Gilman immediately set Johns Hopkins apart from other universities by firmly
coupling research with graduate training. 29 A few other universities had awarded a Ph.D.
or two, but none had developed organized graduate programs. 30 At Johns Hopkins,
which initially (albeit briefly) had no undergraduates, graduate study was the stated focus
of the university. Gilman attracted high quality graduate students by providing
fellowships and offering a program of study comparable to top European universities. 31
Johns Hopkins University defined the American doctorate, and the definition was entirely
based on the idea that graduate students would conduct scientific research. 32 The idea
that “science was not primarily a body of knowledge, but a tool for the production of new
knowledge” was formalized through the Johns Hopkins graduate degree. 33 By the time
Johns Hopkins fully instituted its graduate programs, the outlines of trends in the
development of science in the United States were becoming clearer. As Roger Geiger
observes, “the natural sciences gained a foothold in American higher education around
the 1850s,” “utilitarian objectives became institutionalized in the 1860s,” and “research-
based graduate training became a permanent component in the 1870s.” 34 Each trend built
on earlier trends, leading to an overall push toward greater and greater acceptance of science as a critical component of the university.

Like the reformer presidents who encouraged "pure" research, most of the aspiring scientists who became professors in American universities in the late nineteenth century had been trained in Germany. These aspiring scientists embraced the curricular and institutional changes launched by Eliot and Gilman in particular, because they maintained a characteristic mistrust of technical, practical studies and wanted to conduct research in the service of theoretical innovation. According to the German model that these scientists endorsed, research was an objective search for truth through factual investigation, with little or no attention paid to potential practical applications.\(^{35}\) Pure research needed no justification because science would lead humans inexorably toward a better understanding of themselves, their society, and even the cosmos. Technology was a collection of tools, but science was a practice, a way of thinking, a form of inquiry that would harvest the achievements of the past and ensure mastery of the future.

Charles W. Eliot explained the hopes and ideals attached to scientific inquiry in a speech to the National Education Association:

All thinkers agree that the horizon of the human intellect has widened wonderfully during the past hundred years, and that the scientific method of inquiry, which was known to but very few when the nineteenth century began, has been the means of that widening. ... Now, the passion for pure knowledge is only to be gratified through the scientific method of inquiry.\(^{36}\)

\(^{35}\) Metzger, *Academic Freedom in the Age of the University*, 107.

I interpret the "scientific method of inquiry" to be a mode of knowledge production that was articulated, refined, and strengthened over the course of the late nineteenth century, with universities playing a central role. The "scientific method of inquiry" entailed both institutional and intellectual developments. The first development was the promotion of the idea that truth could be discovered through investigation by trained practitioners. This first development was wholly dependent on the second development, the professionalization of the scientist, with the associated building of "communities of the competent" that legitimized knowledge claims. The third development was the institutional manifestation of the first two: the rise of the university department based on the specialized discipline. The university department was where professional scientists distilled the fundamentals of their esoteric fields of study and trained the next generation of specialists.

The institutional and intellectual components of the scientific method of inquiry helped scientists develop rationales for their fields of interest, construct compelling philosophical justifications for pure research, and campaign for financial support for their experimental laboratories. The scientific method would create a space for university scientists in American society. Within their specialized domains, scientists would be the arbiters of truth claims, the disinterested pursuers of facts, and the seekers of pure knowledge. Universities would be their homes, but American society would be the ultimate beneficiary.

David Hollinger describes the idealized vision of the late nineteenth century scientist as "a humble and modest man of steady habits, laboring patiently, diligently,
selflessly, and without prejudice in the interests of truth."37 This description is not a
caricature. According to Hollinger, scholars and, to an increasing extent, society as a
whole began to believe in the "moral efficacy of science" in the late nineteenth century.38
The moral efficacy of science was linked to the idea that the scientific method was the
best way to guarantee that problems were addressed with integrity and consistency.39
Philosopher John Dewey, who wrote prolifically on the social and intellectual benefits of
science in the late nineteenth century, maintained that the scientific method of inquiry
was the most reliable source of truth given that practitioners had the freedom to pursue
solutions to problems without outsiders interfering with their work.40 For Dewey, as well
as other prominent philosophers like Charles Peirce, the belief that professional scientists
were dedicated to a disinterested pursuit of truth was an article of faith.41 Peirce believed
that, in the words of historian R. Jackson Wilson, "the ethical decision involved in a
conversion to science was nothing short of a complete facing about from the normal
human concern with the self to a love for the world at large."42 Wilson's use of the term
"conversion" is telling. During an era of rapid change and uncertainty, scholars found a
source of faith in science comparable to religious devotion.43

37 David Hollinger, "Inquiry and Uplift: Late Nineteenth-Century American Academics And the Moral
Haskell, ed. (Bloomington, IN: Indiana University, 1984), 142.
38 Hollinger, "Inquiry and Uplift," 147.
39 Thomas Haskell, "Professionalism versus Capitalism: R. H. Tawney, Emile Durkheim, and C. S. Peirce
on the Disinterestedness of Professional Communities," in Objectivity is Not Neutrality: Explanatory
Schemes in History (Baltimore: The Johns Hopkins University Press, 1998), 102; Thomas Bender, "The
Erosion of Public Culture: Cities, Discourses, and Professional Disciplines" in The Authority of Experts,
88–89; Thomas Haskell, The Emergence of Professional Social Science: The American Social Science
Association and the Nineteenth-Century Crisis of Authority (Baltimore: The Johns Hopkins University
Press, 2000), 89.
41 Thomas Haskell, "Professionalism versus Capitalism," 80.
42 R. Jackson Wilson, In Quest of Community: Social Philosophy in the United States, 1860–1920 (New
43 Charles Rosenberg, No Other Gods, 12.
The growing acceptance of the moral efficacy of science was intertwined with a growing respect for the authority of the scientifically trained professional who earned his credentials by taking a specialized course of study at a university. These university trained professional scholars carved out a niche in society by claiming their uniquely valuable contribution to progress through the advancement of knowledge. Scientists were especially effective at building "communities of the competent," which took their institutional form in professional associations and university departments, because they elevated the object of inquiry above all other considerations. For "communities of the competent" to maintain valid claims to intellectual authority, members had to be above reproach. The criteria for membership had to appear, in the words of Thomas Haskell, "impersonal, objective, value-free—not mere opinion, but 'truth.'" It was membership in a professional community of inquiry that lent credence to the "moral efficacy of science" and convinced laymen to trust the validity of scientific investigation.

Scientists established their own norms and standards, and they policed the boundaries of their disciplines and departments. Only those who had the requisite training, preferably graduate level degrees from research universities, were fit to judge the quality of fellow specialists. The university performed a vital function as the institutional fulcrum for this meritocracy, by "screening students upon entrance, formalizing courses of study, publishing textbooks, standardizing examinations, and awarding degrees." Universities thus provided the scaffolding that held up the structure of the community of the competent.

44 Thomas Haskell, The Emergence of Professional Social Science, 89.
45 Burton Bledstein, The Culture of Professionalism, 124.
Scholars within the community of the competent relied on a “theory of knowledge” that, as Robert Post defines it, not only presupposed that “knowledge exists and can be articulated by scholars,” but also “that it is advanced by the free application of highly disciplined forms of inquiry, which corresponds roughly to what Charles Peirce once called ‘the method of inquiry’ as distinct from ‘the method of authority.’”\(^{46}\) To protect the method of inquiry, the advancement of knowledge as practiced by skilled practitioners operating within communities of the competent, scientists organized numerous professional associations. Scientists built on the framework inaugurated by the American Association for the Advancement of Science (AAAS) and the National Academy of Sciences (NAS), both of which had been established in the mid nineteenth century, to launch more than twenty specialist organizations devoted to particular disciplines in the 1880s and 1890s.\(^ {47}\) University departments, professional associations, and journals were designed around the emerging disciplines, and disciplinary affiliation became increasingly important, as members of specialized communities of inquiry worked to establish a common language and to define their areas of expertise.\(^ {48}\) As Roger Geiger argues,

> The growth and specialization of knowledge created bodies of increasingly esoteric doctrines over which academic disciplines claimed sovereignty; the social organization of the membership through associations, meetings, and journals

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formed community structures of authority and legitimacy; and the development of the modern university provided the locus where disciplines could be practiced.\textsuperscript{49} Disciplines were thus the central organizing elements for an “institutional matrix,” to use John Higham’s phrase, that served to support specialized study.\textsuperscript{50}

The increase in specialization and the rising importance of disciplinary affiliation coincided with a remarkable expansion in the size of universities and, accordingly, in the resources available for professionalizing scientists. As enrollments grew, job opportunities for professional scholars multiplied. Growth in one sector stimulated growth in others and professionalization trends accelerated in the 1890s: more associations, more journals, more advanced degree programs.\textsuperscript{51} Scientific journals launched by professional disciplinary associations, as well as academic presses, were housed at the emerging American research universities.\textsuperscript{52} Again, Johns Hopkins University was the forerunner and the home for the leading journals in mathematics, chemistry, biology, physiology, and psychology.\textsuperscript{53} The establishment of journals and academic presses within the research universities provided the final component in the effort to create a network of communication and exchange among professional scientists.

Thus, by the late nineteenth century, scientists had developed a set of compelling arguments on behalf of research, had created institutions to promote their interests and had established professional media outlets to share their specialized knowledge with

\textsuperscript{49} Roger Geiger, \textit{To Advance Knowledge}, 20.
\textsuperscript{51} Roger Geiger, \textit{To Advance Knowledge}, 22–23.
\textsuperscript{53} Rudolph, \textit{The American College and University}, 405.
fellow experts. They had the support of reformer presidents who believed in the promise of scientific research and in the efficacy of the scientific method. Further, industrialist-philanthropists were expressing great interest in universities and were investing large sums to promote technical and practical scientific education. Scientists had created an environment where they could pursue their ideals of truth, objectivity, and progress while securing their status in the national marketplace, and promoting their specialized fields of research. Yet, funding remained problematic. In 1890, retiring AAAS president T.C. Mendenhall delivered an address in which he praised the work of the Association for strengthening the profession and providing “a more systematic direction to scientific research in our country,” but qualified his remarks by lamenting that one important goal remained unmet, “namely ‘to procure for the labors of scientific men increased facilities and a wider usefulness.’”

University expansion had provided more job opportunities for professional scientists, but their employment did not include guaranteed research support. There were two main obstacles to university scientists receiving direct support for their research. The first obstacle was the tension between professors and university trustees regarding the proper balance of teaching and research. The authority that scientists exercised as professional scholars working within their disciplines was subverted by the relative lack of influence they had over the financial and administrative affairs of the university. University administrators and trustees viewed institutions of higher education as business enterprises and the professors as regular employees. While university scientists were

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54 T. C. Mendenhall, “The Relations of Men of Science to the General Public,” *Science* 16, no. 403 (October 24, 1890), 228.
55 Roger Geiger, *To Advance Knowledge*, 16.
trying to define their professional roles in terms of service to the advancement of knowledge through specialized training and research, trustees and administrators were calculating how to stretch endowment and tuition funds to cover a variety of expenses. To trustees and administrators, research was a distraction from the primary responsibilities that professors had to their employers; namely, to teach tuition-paying undergraduates.

University scientists spent decades working to overcome this obstacle, largely by championing the cause of academic freedom. In fact, as Robert Post has argued, the "first and foremost task of those who sought to protect academic freedom was to alter [the] idea that a university professor was an employee serving at the mere sufferance of his employer."\textsuperscript{57} In the late nineteenth century, though, the campaign to protect academic freedom had not yet attracted enough support or interest to make an impact on the relationship between trustees and scholars. Therefore, although research was gaining a foothold in the university in the late nineteenth century, the role of the scientist as professor remained ambiguous. Was the primary function of the professor to train undergraduates to prepare them for future careers or was his main priority to conduct pathbreaking research by running a laboratory? The question of whether to emphasize teaching or research as the main priority was only one of many conflicts that complicated the issue of promoting research within the university. As historian Daniel Kevles asserts, the late nineteenth century American university was

\begin{quote}
  a battleground between ideals of purpose—between the diffusion as against the advancement of knowledge, between the education of widely literate citizens as against the training of professionals, between the preservation of a general culture
\end{quote}

\textsuperscript{57} Ibid.
as against the encouragement of specialization to a degree that would make
general culture impossible.\textsuperscript{58}

These conflicts would continue to challenge university scientists in the ensuing decades.\textsuperscript{59}

Of course, the issues relevant to professional scientists were not isolated to the
university. There was a constant interaction between the scientist as a member of the
community of inquiry and the scientist as an interpreter of society and technology for the
lay public. This interaction had direct implications for the development of the institutions
that supported research, as Charles Rosenberg explains:

The needs and attitudes of society at large have had a significant influence upon
the development of scientific institutions in America, first preparing a social and
economic climate in which science could ultimately flourish, then dictating the
patterns of research and support, and—at times—even influencing the scientist in
his attempts to evaluate and explain research findings.\textsuperscript{60}

The late nineteenth century was the time when “a social and economic climate in which
science could ultimately flourish” came into being. This did not translate into funding for
“pure” university research, but it did set the stage for philanthropists to be receptive to the
value of science and laid the groundwork for future support.

The rise of the machine was in integral part of the social climate that created a
hospitable environment for science. Scientists recognized that urban elites and
businessmen were awed by technology and machinery, and reverence for the machine
helped to encourage reverence for the power of science. At fairs like the 1876
Philadelphia Exposition, attendees learned that “modern technology was mankind’s

\textsuperscript{58} Daniel Kevles, “The Physics, Mathematics, and Chemistry Communities,” 145.
\textsuperscript{59} Charles Rosenberg, \textit{No Other Gods}, 18.
\textsuperscript{60} Ibid., 17.
‘civilizing force,’ driving out superstition, poverty, ignorance."61 Industrialists in particular saw the machine as an engine of social progress. In an essay devoted to the relationship between the owner and the laborer, Andrew Carnegie argued that the progress in society from savage to civilized, facilitated by the machine, had led to “the upward march of labour [sic]” from a “slave, serf who did manual labour” to an independent “freeman.”62 Carnegie was championing a popular theory that the machine was “a great emancipator from the bondage of labor,” as well as a source of general social progress.63

The machine was seen as an apt metaphor for a society that had, in Herbert Spencer’s influential formulation, evolved from simple to complex, from independent to interdependent, and from primitive to civilized.64 In the complex, interdependent society, the machine helped people regain a sense of control and mastery over their environment (not to mention their workers). Science performed a similar function. Americans who were overwhelmed by the extent of changes wrought by the formation of a national urban-industrial marketplace in the wake of the transportation and communication revolutions came to see trained professional scientists as uniquely capable of unraveling the complicated network of causes and effects that shaped modern lives.

The typical educated citizen of the late nineteenth century was perplexed by an inchoate awareness of what Thomas Haskell defines as “interdependence,” “that tendency of social integration and consolidation whereby action in one part of society is transmitted in the form of direct or indirect consequences to other parts of society with

61 Alan Trachtenberg, The Age of Incorporation, 42.
62 Andrew Carnegie, The Problems of To-day (London: George Allen and Sons, 1908), 59.
63 Alan Trachtenberg, The Age of Incorporation, 42.
64 Ibid., 45.
accelerating rapidity, widening scope, and increasing intensity." In the interdependent society, according to Haskell, the illusion of autonomy disintegrated because individuals were unable to identify the causes and effects of their life experiences. Professional scientists, with their unique claims to authority over matters abstract and incomprehensible, coupled with their method of inquiry as supported by communities of the competent, had a specific function in the interdependent society: they could "track down" causation. The cause that was to be "tracked down" became the object of scientific inquiry. Haskell refers specifically to social scientists in his analysis, but the same could be said of natural scientists who were called upon to explain the causes of physical ailments, mental disease, or various other forms of "dysfunction." By performing the valuable function of tracking down causation, professional scientists helped to recapture a sense of control for Americans who had felt unmoored by their perceived loss of autonomy in an interdependent society. Professional scientists provided the reassurance that society could be investigated, analyzed, and, ultimately, improved.

Wealthy industrialists were among the Americans who regained a sense of control and mastery over their social environment by placing their trust in scientific experts. They were already optimistic about the possibilities for change and progress, even if they needed guidance from professionals to determine how to create a better, more orderly society. The industrialists were, after all, the direct beneficiaries of the transportation and communication revolutions. They had achieved more in their lifetimes than they had ever dreamed possible, and they felt empowered to use their wealth to improve society. Their sense of empowerment was heightened by a belief, based on a popular

65 Thomas Haskell, The Emergence of Professional Social Science, 28–29.
66 Ibid., 40.
67 Ibid., 44.
understanding of evolutionary theory, that society was progressing. Throughout the 1880s and 1890s, prominent thinkers including Thomas Huxley, Herbert Spencer, Edward Youmans, John Fiske, Chauncey Wright, John W. Draper, Robert Ingersoll, and Andrew D. White digested evolutionary theory for the American public. The most popular interpreter of evolutionary theory was Herbert Spencer, whose books were exchanged among members of the urban elite and who came on a speaking tour of the United States in the early 1880s at the invitation of Andrew Carnegie.

Spencer served as an inspiration to wealthy elites on two fronts. First, he presented the analogy between evolution and social progress, with the corollary that the findings of natural scientists could provide direct guidelines for the construction of a better world. Second, he offered terms and phrases, like “survival of the fittest” and “struggle for existence,” that wealthy elites adopted to describe the social interactions that they witnessed in cities and boardrooms. The use of these terms was associated with a trend in thought that has become known as “Social Darwinism.” Historian Richard Hofstadter, whose analysis of Social Darwinism helped to define the concept, argued that “the Spencerian system serves students of the American mind as a fossil specimen from which the intellectual body of the period may be reconstructed.” Hofstadter’s assertion is definitely an exaggeration, but it rings true in some respects. For businessmen as well as social reformers, Social Darwinism seemed to explain the nature of the human struggle

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69 Ibid., 324.
to survive in the urban jungle and to offer the hope that progress was possible, even inevitable, if the strongest, “fittest” members of society took control.

Faith in science, trust in experts, a belief in institutions run according to the corporate model that had worked so effectively in business administration, and the idea, gleaned from Social Darwinism, that the wealthy were the “fittest” to lead society convinced industrialists that they could improve mankind through the rational distribution of wealth. They had embraced the possibility that science could provide the tools to re-engineer society as a whole and had accepted responsibility for aiding the effort. Throughout the 1880s and 1890s, wealthy elites began to sponsor a number of social movements that involved “scientific” human engineering, including the eugenics movement, the social hygiene movement, and the institutionalization movement. All of these movements promised to improve mankind through the selective application of “scientific” principles.

There were clear connections between the perceived moral efficacy of science and the social movements that progressive elites launched in the late nineteenth century, as Charles Rosenberg explains:

Though perhaps phrased in measured terms of empirical analysis, proposals for reform in late nineteenth-century American were often suffused by a vision of transcendent moral benefit. Eugenical sterilization, for example, in the mind of one enthusiast was not simply a means of reducing criminality and mental retardation. It would rid man of all “the ills which flesh is heir to.”

Science, as the progressive elites who were directing these movements understood it, could save souls while it ameliorated society. By translating social dysfunction into a

73 Charles Rosenberg, No Other Gods, 12.
"series of solvable problems," scientists could identify and attack a number of sins. The belief that science could perform this valuable function rested on the intellectual authority created by the communities of the competent, because reform measures were "presented as the product of disinterested inquiry" that were "untainted with narrow and sordid partisanship, untainted by the treacherous ideology of the political extremist." Still, Rosenberg adds the caveat that "the appeal of science as an absolute was limited largely to the educated, to the elite and articulate," which accounted both for the pervasiveness of "scientific faith" among the leaders of the eugenics, social hygiene, and institutionalization movements and for its enduring attractiveness to the industrialists who launched "scientific" philanthropy.75

The term "eugenics" was coined by Francis Galton, Charles Darwin's cousin, in 1883. Galton defined eugenics as a "science of improving human stock by giving the more suitable races or strains of blood a better chance of prevailing speedily over the less suitable."76 The "scientific" basis for eugenics was simple to grasp: like begets like, and each successive generation replicates the positive and negative traits of their ancestors. Additionally, based on Lamarckian theory, late nineteenth century Americans, physicians and laymen alike, believed that acquired characteristics were inherited, that "heredity was a dynamic process beginning with conception and extending through weaning," and that "the inheritance of character, disease, and temperament was a protean affair of tendency

74 Ibid.
75 Ibid., 13.
76 Daniel Kevles, In the Name of Eugenics: Genetics and the Uses of Human Heredity (New York: Alfred A. Knopf, 1985), ix.
and disposition.” Lamarkian hereditarian theory gave humans a remarkable amount of control over the destinies of future generations.

The eugenics movement presented skittish elites with a place to turn when they were anxious about lawless immigrants and urban poverty. They could use scientific principles to wrest control back from the teeming masses. All that was required was regulation of reproduction and sexuality. The human stock could be improved as long as mothers carefully monitored their behavior throughout the conception, birth, and weaning of their babies. As Charles Rosenberg contends, “the mother’s ability to shape the child she bore provided—with careful attention paid to the avoidance of mating with the infirm and immoral—a means of achieving social perfection.” Who would deny that “social perfection” was a worthy goal? Michel Foucault has identified this period in the nineteenth century as a time when optimism about improving humans and control over sexuality were interwoven so completely that it seemed imperative for powerful members of society to devote considerable energy to eradicating sexual dysfunction. “The analysis of heredity was placing sex (sexual relations, venereal diseases, matrimonial alliances, perversions) in a position of ‘biological responsibility’ with regard to the species,” and elites would have felt remiss if they had not ensured that sex and fertility were properly “administered.”

The eugenics movement, in its most hopeful and “scientific” guise, spawned a number of other fields and movements. Eugenics was the basis for anthropometry, or the measurement of the brain and body for the purposes of comparative analysis. According

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77 Charles Rosenberg, No Other Gods, 26-27.
78 Ibid., 37.
to the logic of anthropometry, which was developed by the founder of eugenics, Francis Galton, scientists could influence the health and success of future generations by objectively analyzing vital statistics about human attributes. The anthropometrist would use an assessment of physical traits to determine whether an individual would contribute positively or negatively to the race. The goal was to identify talents and weaknesses in order to prevent “dysgenic” couplings between flawed individuals and to encourage “eugenic” couplings between genetically superior individuals. The prevention of “dysgenic” coupling was known as negative eugenics, and tactics included sterilization, immigration restriction, and forced isolation in institutions. Negative eugenics is more widely known today due to the infamous practices of Nazi doctors, and it is justifiably much maligned. The encouragement of “eugenic” coupling, or positive eugenics, had much less nefarious implications and usually involved little more than matchmaking proposals. Each component of the eugenics movement—anthropometry, “negative” strictures, and “positive” matings—were heavily supported by elites who felt that they were employing science to improve mankind.

The eugenics movement was part of a nascent, informal human engineering effort that began to unfold in the late nineteenth century. Other closely related movements also contributed to the formation of the idea that human engineering was possible and worthwhile as a long-term goal. The social hygiene movement, which will be discussed in detail in subsequent chapters, was meant to reduce the incidence of venereal disease and to “improve the human stock” by controlling sexual behavior. Social hygiene adherents were inspired by sentiments like those expressed by Herbert Spencer: “we see

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that progress toward higher social types is joined with progress toward higher types of sexual relations.\textsuperscript{81} The effort to monitor and regulate sexual behavior would become a major preoccupation for philanthropists during the first half of the twentieth century. Another movement that was connected to human engineering involved a late nineteenth century transition from individualized moral suasion to institutionalized social reform in the attempt to cure dysfunction. This was the era of the penitentiary and the mental hospital, a time when social reformers began a quest to, as Michel Foucault argues, confine “unreason” in an age when rationality and “science” were revered.\textsuperscript{82} Michael Willrich explains the common assumptions that linked eugenics with the institutionalization movement:

In the 1880s and 1890s, the widespread acceptance of Lamarck’s theory of acquired characteristics gave a hopeful premise to hereditarianism and to the burgeoning public institutions for the insane and feeble-minded: Under the disciplinary regime of the asylum, deviants could be reformed into sober, law-abiding citizens—characteristics their children would inherit.\textsuperscript{83}

Progressive elites were convinced that they could normalize society through the eugenics, social hygiene and institutionalization movements.

These progressive elites were responsible for the advent of “scientific” philanthropy, a structured approach to giving that was intimately related to the rise of the university, the professionalization of scientific expertise, and the institutionalization

\textsuperscript{83} Michael Willrich, City of Courts: Socializing Justice in Progressive Era Chicago (Cambridge, UK: Cambridge University Press, 2003), 249.
movement. Multiple historians have traced a transition that took place of the course of the nineteenth century, in which charity, personal acts of compassion conducted on an individual basis, was eclipsed by “scientific” philanthropy, a rational attack on the roots of social problems conducted through institutions. The hope was that a “scientific” approach to philanthropy, which basically meant an appeal to reason rather than emotion in social reform efforts, would yield long-term solutions to fundamental “evils” like criminality, pauperism, and alcoholism. Scientific philanthropy was based on three key principles. First, there was the idea of “curing evils at their source,” either by investigating problems scientifically or by facilitating opportunities for individual self-improvement. The principle of “curing evils at their source” was tied both to the institutionalization movement and to “an ideological commitment to professionalization, scientific rationalization, and administrative governance.” The first principle of scientific philanthropy thus led to the second principle, a reliance on institutions as mediating bodies between donors and recipients of philanthropy. The third principle, which built on the first and second, was that giving should be directed toward elites and experts to administer “scientifically.” According to the third principle, well-run institutions staffed by experts would give the deserving poor opportunities to lift themselves out of destitution, while the truly unfit would be left to fend for themselves. Institutions funded by scientific philanthropy would also obliterate the need for charity by attacking the causes of poverty and other social ills, often by sponsoring scientific

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85 Michael Willrich, City of Courts, xxxviii.
investigation into these causes. "Scientific" philanthropy was reliant on the pivotal and pervasive role that scientific inquiry played, and was accordingly dependent upon the development of the research university.

The most powerful philanthropists, including Andrew Carnegie and John D. Rockefeller, chose to invest in universities because they believed that well-trained scientists were the best equipped to achieve the goal of "curing evils at their source." As historian Peter Dobkin Hall argues,

The creation of [a] cadre of highly trained, university-based professional specialists was the cornerstone in the foundation of "scientific" philanthropy. When Social Darwinist ideology, industrial wealth, and academic expertise came together in the 1890s, modern American philanthropy was born. 86

Hall's statement is an over-simplification, but he knits together some key points. The "scientific" philanthropists, led by Carnegie and Rockefeller, were inspired by a faith in science that was based on trust in the community of the competent that university scientists had spent years constructing. This faith in science was coupled with a Social Darwinist conceit that the wealthy were the best equipped to distribute resources to the most deserving recipients. The most deserving recipients were often the scientists housed in the universities. "Scientific" philanthropists of the late nineteenth and early twentieth centuries assumed that universities would be training grounds for the engineers of humanity and society. Philanthropists would support universities, and universities would train the experts to staff institutions and direct social movements.

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Despite the growing faith in science and universities, considerable confusion about the best approaches for funding science remained. Tensions between practical, applied science and "pure" research, between popular movements and specialization, and between teaching and research within the university presented challenges for philanthropists who wanted to find the best methods for attacking social problems rationally. Universities were to become the ideal institutional setting for achieving the goals established by "scientific" philanthropy, but it took decades to refine the policies to achieve these goals. In the interim, Andrew Carnegie and John D. Rockefeller, experimented with their versions of "scientific" philanthropy. For good or ill, they would become the most enthusiastic participants in the advent and spread of "scientific" philanthropy, especially through investment in higher education. They were drawn to the idea of scientifically re-engineering society for all of the reasons enumerated in this chapter: faith in scientific expertise, trust in institutions, and a belief that their wealth could be used to re-engineer society. Moreover, both men had additional impetus based on their personal and professional experiences. The heightened sense of agency and social responsibility that motivated Carnegie and Rockefeller was a product not only of their trust in expert authority, but also of their individual biographies.

Both Carnegie and Rockefeller grew up in rural areas and moved to larger cities to pursue careers, and they both built unparalleled fortunes by investing in railroads and telegraphs. They were products of the urban-industrial transformation in every way. Carnegie and Rockefeller experienced such remarkable success so fast—they were the richest men in the country by the time they reached age thirty—that anything seemed possible with hard work, good decisions, and a focus on long-term gain. Their
experiences clearly demonstrated the value of applied science and administrative organization, a combination they endeavored to replicate as philanthropists. They also learned the value of investing in the future, which they translated into philanthropic investments in cultural and educational institutions. Combined with their embrace of the dominant ideals of the time, particularly a faith in scientific expertise, it was unsurprising that Carnegie and Rockefeller joined the private educational philanthropy movement begun by Ezra Cornell and Johns Hopkins by endowing research universities. Once they donated their first millions to higher education, their odyssey as the world’s most significant “scientific” philanthropists began in earnest.

Carnegie and Rockefeller were almost exact contemporaries, born in the same decade (1830s) as other future industrial magnates including Jay Gould and J. P. Morgan. They both came from modest backgrounds, surpassed their fathers in earning power while still in their early teens, borrowed money to become investors, scrambled to the top of multi-million dollar business conglomerates by the age of thirty, spent their years as business owners swallowing competitors and crushing unions while maintaining the stance that they were “friends to labor,” contributed substantially to developing institutions of higher education in the late nineteenth century, and launched large philanthropic foundations in the early twentieth century. Yet, their differences were as remarkable as their similarities. Carnegie was a rambunctious, rotund Scotsman who was known for his enjoyment of rich food and drink. Rockefeller was a pencil thin teetotaler and devout Baptist who rarely cracked a smile in public. They had frequent personal and professional disagreements. As Rockefeller's most astute (and most recent) biographer, Ron Chernow, explains,
In their approach to business, [Carnegie and Rockefeller] had often mirrored each other, stressing attention to detail, ruthlessly slashing costs, and keeping dividends low. Both had struggled with their own unacknowledged avarice, pioneered in philanthropy, and prided themselves on being friends of the working man. Yet they never seemed to get along.\textsuperscript{87}

Ultimately, the personal differences that separated Carnegie and Rockefeller had a greater impact on their respective approaches to philanthropy than on their business dealings.

Andrew Carnegie was born in Dunfermline, Scotland on November 25, 1835 to William and Margaret, a working class couple with radical political beliefs.\textsuperscript{88} William Carnegie was a weaver who was displaced by textile factories in the 1840s. The family suffered through several impoverished years and harsh winters before deciding to emigrate from Scotland to Pittsburgh in 1848, where young Andrew was forced to start working immediately. Beginning with his first job as a textile mill worker at the age of thirteen, Andrew succeeded at every business endeavor that he attempted. He became a bookkeeper, then a telegraph operator, and, eventually, an investor in telegraph and railroad industries. Carnegie’s dedication to hard work and to long-term investing shaped his philosophy of life, one that he would later apply to his philanthropic endeavors.\textsuperscript{89}

Carnegie became the main breadwinner for his family in 1855, when his father died. Rather than depending on his meager salary, Carnegie convinced his mother to take out a second mortgage on the family home to enable him to invest in the Adams Express telegraph company. The investment quickly turned into a source of greater riches than

\textsuperscript{88} Andrew Carnegie, \textit{Autobiography of Andrew Carnegie}, John C. Van Dyke, ed. (Garden City, New York: Doubleday, Doran, 1933), 1–6.
the Carnegie family had ever known. Carnegie used his income from the Adams Express stock to invest in numerous other transportation and communication industries. At the same time, he rose through the ranks of the Pennsylvania Railroad, where he had started as a telegraph operator. By the outbreak of Civil War hostilities, Carnegie was a key player in the burgeoning transportation and communication industries.\(^{90}\)

During the Civil War, Carnegie was responsible for organizing Union Army railroad transportation and telegraph communication in Virginia. The experience of mobilizing the railroads for war opened a path to future business success for Carnegie, who saw opportunities for profit in every aspect of the transportation sector. Like other industrialists who profited from war-related businesses, Carnegie became convinced of the value of bureaucratic organization and technical proficiency, which piqued his interest in higher education and applied science. Beginning in 1861, Carnegie invested in companies specializing in oil extraction and refining, iron extraction and refining, sleeping car manufacture and distribution, telegraph communications, and bridge building. His myriad investments and war work gave him an inside track on railroad development and all the ways that he could "exact tribute" from the components of the industry.\(^ {91}\) Carnegie's wealth continued to expand rapidly throughout the 1860s and 1870s, as he consolidated his holdings into his massive conglomerate, the Carnegie Steel Company.\(^ {92}\) By 1893, the Carnegie Steel Company (later U.S. Steel) was "the largest and most profitable steel business in the world."\(^ {93}\) Carnegie finally retired in 1901 by selling

\(^{90}\) Ibid., 116–144.
\(^{91}\) Ibid., 309.
\(^{92}\) Ibid., 360.
his shares in U.S. Steel to J. P. Morgan for $408,000,000 (equivalent to over $9,000,000,000 in 2005). At that point, he was the richest man in the United States.94

John Davison Rockefeller accumulated his massive wealth at a pace similar to Andrew Carnegie, and also profited tremendously from Civil War mobilization. John Davison was born to William Avery “Big Bill” and Eliza Rockefeller in Richland, New York in 1839. Big Bill was a con artist who probably married Eliza for her dowry. Immediately after Big Bill married Eliza, he moved his long-time mistress in as a housekeeper and proceeded to father several children with both women. Due to his father’s philandering and gambling, John spent his youngest years in Richland living in abject poverty, with his mother often reduced to begging for food from the neighbors. The situation changed considerably when Eliza moved her children to live closer to her parents, where the family was able to enjoy the plenitude of the Davison farm in Moravia, New York. While in Moravia, Rockefeller became a dedicated Baptist. In church, he learned that accumulating wealth through hard work was admirable but that ostentatious display was abhorrent, values along with beliefs in self-improvement and redemption that he carried throughout his life and applied directly to his future philanthropic pursuits.95

In September 1855, Rockefeller left New York for Cleveland, where he was hired as an assistant bookkeeper. His bookkeeping job not only trained him in accounting practices, where he was meticulous, but also introduced him to the growing importance of telegraphs and railroads for the American economy. Even at this early stage in his career, Rockefeller used a bookkeeping entry system to catalogue every activity and

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94 Ibid., 784.
expense, including a 6% tithe for charity. He remained a devout Baptist, and wrote in his diary that he believed God would grant him increasing wealth because he would be a good steward.\footnote{96}{Ibid., 45–50.} As Rockefeller biographer Chernow asserts, the teenager embodied “the Protestant work ethic in its purest form, leading a life so consistent with Weber’s classic essay that it reads like his spiritual biography.”\footnote{97}{Ibid., 55.}

At the age of eighteen, Rockefeller left his job and used all of his savings to start a produce business, which was successful enough to allow him to pay for a substitute in the Civil War.\footnote{98}{Ibid., 60.} Due to Cleveland’s central location and the dramatic rise in commodities prices during the war, profits soared. Rockefeller remembered the lessons of his days as a bookkeeper, and decided to invest the war profits in industries affiliated with railroad transportation. Like Carnegie, Rockefeller fervently believed in the idea of investing in the future, a belief that colored his philanthropic ideals. He made his first foray into the oil industry in 1863, when he and his partners invested $4000 (equivalent to $74,000 in 2005) in a refining operation.\footnote{99}{Ibid., 69–77.} As the profits again rolled in, Rockefeller “lovingly tended his refinery” and became entranced by oil.\footnote{100}{Ibid., 79.}

Throughout the 1860s, Rockefeller used his clout and business acumen to negotiate some of the most infamous deals in American history and to secure his position as the most powerful oilman in the world. One such deal, which became known as the “Cleveland Massacre,” was a secret agreement with railroad magnate Jay Gould that gave Rockefeller and his partners access to pipelines and railroad lines at a huge discount in exchange for oil supplies. According to Ron Chernow, the deal “marked a turning point
for Rockefeller, the oil industry, and the entire American economy,” and Cleveland soon surpassed Pittsburgh as the leading refining center in the United States. Rockefeller’s success in Cleveland enabled him to consolidate his disparate investments into a joint-stock company, Standard Oil, in 1870. Over the next decades, Standard Oil proceeded to absorb competitors and effectively gain control over the American oil industry.

Through his one quarter share in Standard Oil, which had a “near monopoly in the refining, distribution, and marketing of petroleum products,” Rockefeller’s wealth accumulation accelerated in the 1880s and 1890s. During this period, the Standard Oil trust “gushed forth a stream of wealth as no other closely held corporation in the world’s history has ever paid.” In addition to his $3,000,000 annual dividend from Standard Oil (equivalent to an average of approximately $65,000,000 in 2005), Rockefeller earned income from investments in nine real estate firms, six steel companies, six steamship companies, and nine banks. Like Carnegie, Rockefeller made a decisive exit by selling his shares of Standard Oil to J. P. Morgan in 1901. While Rockefeller received a total of $88,500,000 from Morgan (about one-fifth of what Carnegie received; equivalent to almost $2,000,000,000 in 2005), he retained enough interest in other investments to continue to accumulate wealth at a rapid pace. By 1913, his fortune was approximately $900,000,000 (equivalent to $17,000,000,000 in 2005), allowing him to surpass Carnegie as the wealthiest American.

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101 Ibid., 87–115.
104 Ron Chernow, Titan, 336.
105 Ibid., 383–393.
106 Allan Nevins, Study in Power, 300.
Carnegie and Rockefeller accumulated so much wealth in the late nineteenth century that they were unsure what to do with the money. Both men had simple desires and small families. They could buy estates and luxuries galore, and still their dividend incomes would bring in more than they could spend. Rather than squander their wealth on frivolity, both men chose to begin second careers as philanthropists well before they retired from business. Like several of their fellow wealthy industrialists, Carnegie and Rockefeller began to donate large sums to support or establish cultural and educational institutions. They were among the most outspoken proponents of funding institutions of higher education in particular. Why did they devote so much effort to philanthropic endeavors and to supporting institutions of higher education? Historians have contended that Carnegie was motivated primarily by self-aggrandizement, Rockefeller by his devotion to his Baptist faith, and both by a guilty need to compensate for ruthless business practices.\textsuperscript{107} Some historians have made the enormous investment in American higher education by Carnegie and Rockefeller seem almost inevitable:

The diverse purposes of the universities matched the diverse impulses of the new industrial magnates. The stewardship of wealth, conceived as a religious obligation and a more secular social responsibility; the economic benefits promised by the new science and technology; public reputation as a defender of culture, acquired by patronizing rather than pursuing knowledge; penance for amassing great fortunes with ruthless tactics; and immortality for the family

name—all could be expressed and realized by endowing a new or existing university.\textsuperscript{108}

These motivations were doubtlessly important and they played a significant role in the way that Carnegie and Rockefeller saw philanthropy and the support of institutions of higher education. Yet, there was more to the story for these two philanthropic pioneers. A closer look at the way Carnegie and Rockefeller structured their arguments in support of philanthropy reveals that they were each inspired by a set of ideals and beliefs that crystallized in response to their personal experiences and their exposure to the rise of scientific expertise in the late nineteenth century.

As noted earlier, Carnegie and Rockefeller experienced, both personally and professionally, an astonishing amount of change in a short period of time. After spending their formative years in rural poverty, both men participated in and benefited from almost every aspect of the urban-industrial transformation in America. The ground was constantly shifting beneath their feet, and the shifts were seismic in proportion. It seemed that the possibilities to transform the world for the better were endless. Both men were grateful for their wealth and the opportunities to use it for the benefit of society. Nonetheless, neither Carnegie nor Rockefeller credited chance or providence for their good fortune. They were happy to take credit for their own perseverance, foresight, and talent. It was only logical that they sought out others with similar character traits, all of which were cultivated and rewarded in universities. By donating to institutions of higher education, they would provide others with the opportunity to compete in the marketplace, where the next generation of leaders would distinguish themselves through hard work and dedication.

\textsuperscript{108} Alexandra Oleson and John Voss, "Introduction," xi.
Contributions to universities were also contributions to science, to the advancement of knowledge, and to progress. Although neither Carnegie nor Rockefeller had a formal education, they had learned to trust the expertise of highly trained scientists.\textsuperscript{109} For example, Herman Frasch had essentially saved Standard Oil from experiencing a major fuel shortage by investing a process for transforming poor quality crude into high octane oil, while Carnegie could credit the Bessemer process for enabling his ability to corner the steel market. Rockefeller and Carnegie learned to trust scientific expertise in other matters as well. Scientists were turning the issues that were of greatest concern for philanthropists and social reformers—public health, criminality, poverty, mental and sexual hygiene—into fields of specialized study. With the right tools and training, both of which were provided by universities, scientists could fulfill the goal of "scientific" philanthropy and cure problems at their roots. This constellation of beliefs and assumptions became powerful motivations for Carnegie and Rockefeller to invest in universities.

Carnegie also had personal reasons for his interest in universities. He was enthralled by culture and science, and he desperately wanted to belong to the educated, cosmopolitan elite who gathered in salons and supported cultural institutions. In the 1880s, Andrew Carnegie joined the Nineteenth Century Club, a New York salon that included a mixture of intellectuals and socialites.\textsuperscript{110} Inspired by discussions about Herbert Spencer and Matthew Arnold, Carnegie began to argue that the best way to contribute to the progress of society was to provide access to sources of "sweetness and light" (an expression that Arnold used to described the effect of art and culture on the


human spirit).\textsuperscript{111} Carnegie marveled at the wonders presented at the popular scientific and cultural expositions that proliferated in the late nineteenth century, which "paraded the extraordinary progress apparently being made in every branch of human achievement."\textsuperscript{112} His earliest forays into philanthropic giving were part of the public celebration of art, literature, and science represented by expositions, adult education courses and seminars, and newly established museums, art collections, and libraries.

Soon after he entered the world of philanthropy, Carnegie presented his philosophy of giving to the American public. In June 1889, Carnegie published an article titled "Wealth" in the \textit{North American Review} in which he expressed his deeply felt beliefs about the obligations of the wealthy to society. The article, which became known as the "Gospel of Wealth" by readers who associated Carnegie's arguments with the Social Gospel movement, was laced with expressions drawn from Social Darwinism. Carnegie extolled his wealth as a necessary and beneficial consequence of social evolution: "The contrast between the palace of the millionaire and the cottage of the laborer with us to-day . . . is not to be deplored, but welcomed as highly beneficial. It is well, nay, essential, for the progress of the race."\textsuperscript{113} This statement was a tribute to his idol, Herbert Spencer, and his interpretation of natural selection in economic terms.\textsuperscript{114} Carnegie read and quoted Spencer selectively, searching for passages that supported

\textsuperscript{114} J. D. Y. Peel, \textit{Herbert Spencer}, 2.
immense wealth and industrial growth like excerpts from *Social Statics* that promoted a
free market social system based on the "survival of the fittest."\(^{115}\)

According to Carnegie, the wealthy must act as stewards of their massive
financial resources to serve social progress. Instead of leaving inheritances for family
members, which would invite sloth and vanity, the wealthy should attempt to give away
the bulk of their fortunes during their lifetimes. To encourage such behavior, the
government should tax estates heavily.\(^{116}\) Through this approach, the wealthy man would
become "the mere trustee and agent for his poorer brethren, bringing to their service his
superior wisdom, experience, and ability to administer, doing for them better than they
could or would do for themselves."\(^{117}\)

Carnegie’s essay was a carefully reasoned endorsement of the shift from
ameliorative charity to "scientific" philanthropy, where contributions would be directed
toward attacking the roots of social problems. His vision was, actually, the very
definition of "scientific" philanthropy. The wealthy should give to institutions that were
staffed by experts and dedicated to progress through the scientific investigation of
dysfunction. To ensure the efficacy of "scientific" philanthropic measures, Carnegie
advocated for the separation of the "irreclaimably destitute, shiftless, and worthless" from
the "well-doing and industrious poor, who are liable to be demoralized by contact with
these unfortunates."\(^{118}\) With this statement, Carnegie showed his support for the
institutionalization movement, which was designed to shield the virtuous members of
society from the irredeemable.

\(^{117}\) Ibid., 25.
\(^{118}\) Ibid., 31.
As expressed in “Wealth,” Carnegie’s philosophy included a celebration of unabridged competition, adapted from Spencer, coupled with strict labor discipline and the careful regulation of “defectives.” This belief system, which he attempted to put into practice through philanthropy, corresponded to the “three great schemata” that philosopher Michel Foucault outlines in his definition of the “carceral apparatus” that developed in the nineteenth century: “the politico-moral schema of individual isolation and hierarchy; the economic model of force applied to compulsory work; the technico-medical model of cure and normalization.”\(^{119}\) As Foucault asserts, the relationship among these schemata was not coincidental. The underlying idea that tied these practices together suffused philanthropy after Carnegie penned “Wealth:” society would improve if individuals could be taught to behave responsibly within the existing economic order. The defectives should be identified and removed, either through selective eugenic breeding or institutionalization, and the remaining members of society should be given ample opportunity to improve themselves through education and exposure to healthy cultural activities.

Carnegie was unaware that his prescription for benevolence would strike many of his readers as elitist and coercive. He simply wanted to cultivate self-improvement in the lower classes and root out the causes of social ills. To that end, Carnegie suggested that philanthropists donate funds to universities, libraries, hospitals, parks, concert halls, swimming pools, and churches (in that order).\(^{120}\) This suggestion managed to irk a broad spectrum of Americans, from labor unions to social workers to clergy. As His Eminence Henry Cardinal Manning, Archbishop of Westminster, wrote in the magazine *Nineteenth*


\(^{120}\) Andrew Carnegie, “Wealth,” 32–46.
Century in December 1890, many churchgoers saw Carnegie’s embrace of wealth and dismissal of traditional charity as “irresponsible,” self-serving, and short-sighted. Manning characterized Carnegie as “an anti-Christian phenomenon, a social monstrosity, and a grave political peril.”121 Additionally, labor unions continued to object that Carnegie’s wealth should be remitted to workers in the form of back-wages rather than parcelled out to recreational and cultural activities.

Undeterred, Carnegie followed his own advice in his late nineteenth century philanthropic endeavors. To expose the average working class citizen in Scotland and the United States to “sweetness and light,” Carnegie donated thousands of church organs and provided the funds to build and equip over two thousand public libraries, all of which bore his name.122 His first substantial endowment gift was to donate $2,000,000 (equivalent to $44,000,000 in 2005) to establish the Carnegie Institute of Pittsburgh in 1896, which began as a civic gallery and art museum.123 He followed these early efforts with a multitude of endowments and foundations, always maintaining his pattern of placing his name front and center: he established the Carnegie Institute of Technology (a technical school attached to the Carnegie Institute of Pittsburgh) in 1900, the Carnegie Trust for the Universities of Scotland in 1901, the Carnegie Institution of Washington in 1902, the Carnegie Hero Fund Commission in 1904, the Carnegie Foundation for the Advancement of Teaching in 1905, the Carnegie Endowment for International Peace in 1910, and, in his final effort to rid himself of his excess wealth, the gigantic Carnegie

Corporation of New York in 1911.\textsuperscript{124} By the time of his death in 1919, Carnegie had
donated over two-thirds of his fortune, $350,000,000, to his permanent philanthropic
endowment funds.\textsuperscript{125} In his various efforts to encourage self-improvement, Carnegie
continued to adhere to the principles of "scientific" philanthropy by demanding that his
wealth be directed at the roots of social problems and that funds be distributed based on
institutional policies rather than personal interests.

John D. Rockefeller also eventually promoted a version of "scientific"
philanthropy, but his initial efforts to give away his fortune were deeply personal and
based on his religious faith. From the time of his first job as a bookkeeper at age sixteen,
Rockefeller had devoted part of his salary to charitable causes. Rockefeller was hardly
methodical in his giving; he simply donated what he could to Baptist charities. This
charitable practice worked well for a time, but as his wealth grew, Rockefeller was
flooded with letters requesting charitable assistance and he quickly felt overwhelmed. He
wanted to help others, but he was unsure about how to approach philanthropy. Unlike
Carnegie, Rockefeller never attended expositions or salons. He was an intensely private
man who avoided events that required small talk with strangers. Most of his free time
was spent with family or with associates from his Baptist church.

Since Rockefeller's dedication to Baptist organizations was of paramount
importance to him, he looked to church figures for advice. As he floundered under the
weight of seemingly endless requests for assistance, he came across writings by Frederick
T. Gates of the Baptist Education Society. Rockefeller was especially intrigued by an

(Cambridge, UK: Cambridge University Press, 2003), 224.
article Gates wrote titled "The Need for a Baptist University in Chicago," which the Baptist Education Society published in 1888.126 The two men met to discuss the best ways to promote Baptist education soon after the article was published, and Gates began to guide Rockefeller in his philanthropic endeavors. It was Gates who first espoused "scientific" philanthropy. In the words of sociologist Barbara Howe, "Rockefeller and Gates, as well as their biographers, agree that it was Gates who conceptualized, extended, and implemented the principle of scientific benevolence for which Rockefeller became so famous."127 Rockefeller even paid Gates a salary to serve as a philanthropic advisor.

With Gates's urging, Rockefeller donated $600,000 (equivalent to $12,000,000 in 2005) in 1889 to establish the University of Chicago, with the condition that the Baptist Education Society raise an additional $400,000 in matching funds. Gates convinced Rockefeller that William Rainey Harper, a devout Baptist with a Ph.D. in Hebrew from Yale University who was teaching Semitic languages at his alma mater, was the best candidate for president of the new university.128 Harper made it clear that he would only accept the presidency of the University of Chicago if he could have access to enough funds to inaugurate an institution that compared with the leading universities in the country. Rockefeller was reluctant about making any promises, but Harper was insistent. He wrote Rockefeller in August 1890:

The denomination, indeed, the whole country, are [sic] expecting the University of Chicago to be from the very beginning an institution of the highest rank and

126 Allan Nevins, Study in Power, 168.
character. Already it is talked of in connection with Yale, Harvard, Princeton, Johns Hopkins, the University of Michigan, and Cornell. No one expects that it will be in any respect lower in grade and equipment that the average of the institutions to which I have referred, and yet, with the money pledged, I cannot understand how the expectations can be fulfilled. Naturally we ought to be willing to begin small and grow, but in these days when things are done so rapidly, and with the example of Johns Hopkins before our eyes, it seems a great pity to wait for growth when we might be born full-fledged.129

In this statement, Harper referred to the innovations in graduate education and research that Johns Hopkins had pioneered. Gates and Rockefeller were well acquainted with the revolution in higher education that Harper was hoping to join. Harper was offering an opportunity to be a part of the vanguard of scientific progress. With an adequate supply of funds from Rockefeller, Harper argued, the University of Chicago would immediately join the small group of elite American research universities. Harper convinced Rockefeller and Gates that the development of a world-class research university with graduate programs and top-notch professors was the best possible philanthropic choice. It was the beginning of a long and fruitful collaboration.

To lure Harper, Rockefeller pledged $1,000,000 in 1890. After the University of Chicago opened in 1892, Rockefeller continued to respond to Harper’s endless requests for additional funds. From 1892 to 1910, when he contributed a final endowment gift of $10,000,000, Rockefeller personally donated almost $35,000,000 (equivalent to

approximately $720,000,000 in 2005) to the University of Chicago.\textsuperscript{130} Rockefeller continued to supplement his initial donations to the University of Chicago because the university appeared to live up to Harper’s promises. Within a decade of its founding, the University of Chicago became one of the most esteemed universities in the United States, with highly regarded departments in the natural sciences, social sciences, and humanities. The University of Chicago presented the Rockefeller family with firsthand experience in working with university administrators and, less directly, with professional scientists. By the early twentieth century, Rockefeller and Gates had both adopted the faith in science and in university scientists, a faith that became as relevant to their philanthropic decisions as their devotion to the Baptist church.

With the University of Chicago established as a successful ongoing interest, Gates dedicated himself to learning more about ways to promote social progress through philanthropy. Gates also mentored Rockefeller’s only son, John D. Rockefeller, Jr. (known as “Junior” to his father’s “Senior”), and schooled the young man in the art of philanthropic giving.\textsuperscript{131} Immediately after graduating from Brown University in 1897, Junior began working with Gates on a daily basis to help devise a long-term plan for Rockefeller philanthropy. By 1901, Gates declared that Junior had earned “a postgraduate degree in business and benevolence.”\textsuperscript{132} Together, Gates and Junior became the architects of Rockefeller philanthropy, with Senior primarily acting as the final arbiter

\textsuperscript{131} Waldemar Nielson, \textit{The Big Foundations}, 49. Friends, associates and family members referred to the elder Rockefeller as “Senior” and the younger as “Junior.” Henceforth, I will do the same when comparing both men, but I will also use only the surname Rockefeller when it is obvious that I am referring to one man or the other.
\textsuperscript{132} Albert F. Schenkel, \textit{The Rich Man and the Kingdom: John D. Rockefeller, Jr., and the Protestant Establishment} (Minneapolis: Fortress Press, 1995), 38.
and source of funds. Senior felt he could trust his son and his advisor to act on his behalf because they shared his philanthropic "conscience," which was informed by deeply held Baptist beliefs coupled with a faith in modern science. Historian Albert Schenkel characterizes the Rockefeller belief system as "Protestant modernism," in which father and son "promoted modern science, supported the modern university, and responded to the findings of modern research not because . . . religion should yield the task of cultural leadership to science, but because [there was] no conflict between the two." Both Rockefellers, "Senior" and "Junior," believed that civilization would progress toward a greater approximation of "God's kingdom" if society was governed according to principles that were both rational and Christian.

Gates and Junior had different ideas about the best ways to promote progress, but both believed that education was crucial. For Gates, medical education and research held the greatest promise, whereas Junior focused initially on the deficiencies of public education in the South. The first two major Rockefeller philanthropies were devoted to supporting these two areas: the Rockefeller Institute for Medical Research, established in 1901, and the General Education Board, established in 1903. As he established these philanthropic foundations in the early twentieth century, keeping pace with Carnegie, Senior relied increasingly on close advisors to help him with his fundamental goal, "to encourage research into and diffusion of ideas that would potentially improve society and increase the well-being of mankind." Senior was the public face and name behind Rockefeller philanthropy, but Gates and Junior were the masterminds. When Senior

134 Albert Schenkel, The Rich Man and the Kingdom, 68.
137 Judith Sealander, "Curing Evils at the Source," 224.
delivered public pronouncements about his philosophy of philanthropy, he echoed the beliefs that Gates and Junior had put into practice.

Almost twenty years after Carnegie penned “Wealth,” Rockefeller articulated his beliefs about philanthropy in an article that was widely distributed in 1908 under the title “The Difficult Art of Giving.” Like Carnegie, Rockefeller celebrated his accumulation of a great fortune and warned that the wealthy must act as responsible stewards in their philanthropy. He also paid homage to “scientific philanthropy:”

The giver of money, if his contribution is to be valuable, must add service in the way of study, and he must help to attack and improve underlying conditions. . . . All over the world the need of dealing with philanthropy with something beyond the emotions is evident, and everywhere help is being given to those heroic men and women who are devoting themselves to the practical and essentially scientific tasks. . . . Every new fact discovered, every widening of the boundaries of human knowledge by research becomes universally known to all institutions of learning, and becomes a benefaction at once to the whole race.¹³⁸

Rockefeller’s statement reveals that university scientists had succeeded in their long-term campaign to establish themselves as expert interpreters of society and the physical world. When Rockefeller referred specifically to “service in the way of study,” he was endorsing rationales for scientific research that had been carefully constructed by professional scholars. Scientists had convinced Rockefeller and his compatriots that “professional inquiry within a matrix of norms defined and enforced” by the community of the competent was the best, even the only, way “to attack and improve underlying

conditions.” As Rockefeller celebrated the “heroic men and women” who were pursuing scientific investigations, he lent his powerful name and the weight of his fortune to the movement to establish American university scientists as morally efficacious experts. In the minds and through the policies of “scientific” philanthropists like Rockefeller, the advancement of knowledge and the improvement of mankind were becoming more densely interwoven.

The Rockefellers, guided by Gates and William Rainey Harper as well as the example of Carnegie, had arrived at the conclusion that research and higher education were the dual paths toward social progress. They had clearly shown their dedication to Harper’s vision for the University of Chicago, and it was a vision that was expansive and far-reaching. The hope for the future of higher education that Harper had expressed in his 1903 speech, quoted at the beginning of this chapter, was bound to be realized in fairly short order. The combination of foundation investment, professional science, and university reform held tremendous promise for the future of research. The only element that remained unclear was whether university scientists would receive direct support in the form of project grants.

By the turn of the twentieth century, the elements necessary to develop American university research had begun to coalesce: professional scientists had established their authority as experts, universities had been restructured to emphasize research as well as teaching, and “scientific” philanthropists, primarily Carnegie and the Rockefellers, had shown their willingness to invest in the advancement of knowledge. At the same time, “scientific” philanthropy was undergoing major renovations. The “close of the first period of university education in the United States” that William Rainey Harper

celebrated in 1903 also marked the close of the first era of modern American scientific philanthropy, an era characterized by ad hoc endowment of particular educational institutions. With the establishment of philanthropic foundations in the early twentieth century, the second era of “scientific” philanthropy began. The transition from the first era to the second involved continuities in terms of personnel and stated objectives, but substantive changes in terms of policies and institutional structure. The goals that Rockefeller and Carnegie had promoted in the late nineteenth century, namely the advancement and diffusion of knowledge and the improvement of mankind, remained central, but the new foundations marked a departure from earlier philanthropic efforts because they were autonomous, chartered institutions with boards of trustees and officers who pledged to carry out stated objectives. Instead of allowing university administrators and trustees to determine whether scientists received research funds, Rockefeller and Carnegie established independent research institutes that competed with university science departments. Rather than parceling out endowment funds to a few select institutions of higher education, Rockefeller and Carnegie launched educational foundations that initiated long-term reform projects designed to improve American education at all levels.

The second era of “scientific” philanthropy and the launching of the first major philanthropic foundations will be discussed in Chapter Two. The decision by Carnegie and Rockefeller to make separate provisions for universities, which would be supported by educational foundations, and research, which would be funded through institutes that operated apart from universities, was a defining aspect of the second era of scientific philanthropy. Although this second era was more policy driven than the first era, many
funding decisions were still made on a trial-and-error basis. The frustration persisted for university scientists, who continued to struggle to find direct support for their research. In fact, the separation of research institutes from universities in the new foundations exacerbated the existing conflicts between teaching and research within the universities. Resolving the tensions between teaching and research, between supporting universities through endowments and funding scientists directly, and between bolstering existing programs and establishing new institutes became the main preoccupations for philanthropists as they experimented with foundation policies in the early twentieth century. In the process of experimentation, Carnegie and Rockefeller learned valuable lessons in the "difficult art of giving" that had a direct and appreciable impact on American university research.

By the end of World War I, philanthropic foundations and scientists at elite universities were working together toward a common goal: the improvement of mankind through research. As I examine the efforts of philanthropic foundation officers to improve mankind by funding scientific research, I concentrate on three main themes that directed foundation policies and practices in the 1920s and 1930s, and that tie together the projects that I discuss in this dissertation: the "interlocking directorate," "cooperation in research," and human engineering. Foundation policies were based on relationships as much as ideals, particularly relations among members of an "interlocking directorate" comprised of foundation officers, scientific entrepreneurs and university administrators. I introduce and explain the formation of the interlocking directorate in Chapter Two, and then continue to demonstrate the efficacy of the interlocking directorate in Chapters Three through Five.
Throughout the interwar period, as foundation officers defined and implemented policies, they lauded the ideal of “cooperation in research.” “Cooperation in research” meant transcending disciplinary boundaries to reduce barriers that isolated scientists from one another. Specialization was viewed as a dangerous trend that prevented scientists from addressing the largest and most significant social issues. As I will show in Chapters Three through Five, “cooperation in research” was a deceptively simple objective because, as I have already argued in Chapter One, disciplines and departments were crucial for scientists in terms of establishing standards, training graduate students, and shoring up claims to intellectual authority.

The goal of improving mankind also led foundation officers to launch what I conceptualize as a human engineering effort. My understanding of human engineering is based on Donna Haraway’s definition: “the project of imagining and producing the ideal human being in the ideal society,” which is “clearly associated in concrete detail and in metaphoric extension with the scientific management of material and human factors of production in industrial, familial, and general social organization in America in the twentieth century.”140 Chapters Three through Five are devoted to examining the phases of the human engineering effort, especially as it was defined by the officers of the Rockefeller Foundation, as it unfolded between the early twentieth century and World War II. The three phases were continuous and overlapping, and thus only roughly correspond to strict chronology. The first phase, which occurred mainly between the late nineteenth century and the mid-1920s, was devoted more to “movements” than to the promotion of particular university science research projects. The institutionalization,

eugenics, social hygiene and mental hygiene movements figured prominently in this phase, which was a loosely defined social reform effort. The second phase began with eugenics research and then expanded to include sex research, psychology, "psychobiology," and primatology. This phase of the human engineering project was premised on assumptions gleaned from evolutionary theory and behaviorism, namely that humans could be improved as a species through the investigation and control of behavior and sexual reproduction. The third phase, which began in the mid-1930s, represented a major shift from research into humans as a species to research into the component parts of human life: chemical processes, genetic material, and organic molecules. This phase was devoted to the investigation and control of the structure and function of human bodies on a molecular level.

Each phase of the human engineering project, as I conceive it, was guided by similar goals and assumptions, even though the tactics for achieving the goals changed dramatically over time. The primary goal was to improve mankind. Foundation officers never felt compelled to explicate their understanding of this goal, just like they never worked through the implicit causal assumptions behind their policy initiatives. They made many assumptions that were apparently contradictory, especially that humans were malleable enough to be transformed through behavioral control but were also biologically determined to the extent that the entire race could be improved through eugenics. The important point is that these assumptions had the weight of millions of dollars behind them, and they were incorporated into the rationales that successful grant applicants used to appeal to foundation officers.
I have already introduced aspects of the first phase of the human engineering project that is the centerpiece of this study, which begins with the institutionalization, eugenics, social hygiene, and mental hygiene movements. The first phase will also be discussed in some detail in Chapter Two. In Chapter Three and at the beginning of Chapter Four, I examine the second phase of the human engineering effort by exploring foundation policy initiatives that were built on the eugenics and social hygiene movements. In Chapter Three, I use the case study of Yale University behavioral sciences to examine the human engineering effort as it was understood by the officers of the Laura Spelman Rockefeller Memorial and the architects of the Division of Studies “human biology” program at the Rockefeller Foundation. In Chapter Four, which is devoted to an analysis of foundation intervention in the biological sciences, I compare the Carnegie Institution of Washington and Rockefeller Foundation Division of Studies policies of the 1920s, both of which were devoted primarily to eugenics and social hygiene, to the policy reforms that Warren Weaver initiated for the Rockefeller Foundation in the early 1930s.

As I demonstrate in Chapter Four, the human engineering project was reinvigorated and redefined by Warren Weaver, a Ph.D. in mathematics who became the Rockefeller Foundation Director of the Division of Natural Sciences in 1932. Weaver had a mechanistic understanding of human development, informed by both his mathematics background and his fascination with the latest developments in genetics, embryology, endocrinology, molecular biology, and biochemistry. He presented a “new science of man” agenda that was based on the idea that humans were products of intersecting physico-chemical processes and genetic predispositions. His reconceived
human engineering effort reached its fullest realization at the California Institute of Technology (Caltech). At Caltech, Weaver's intervention helped facilitate the development of biochemistry, genetics, and molecular biology programs, in which researchers led by Linus Pauling made major breakthroughs.

My analysis of the interlocking directorate, cooperation in research, and the human engineering effort that foundations launched in the interwar period is not meant to be a comprehensive history of the impact of philanthropic foundations on the development of American university research. I focus on research in the fields of human behavior and biology that are especially illustrative of the three themes that I have outlined, and thus neglect very important contributions that foundations made in a number of other fields. I also concentrate on the "winners" in the funding game—the scientists and universities that received the largest and, in my opinion, most influential grants. Still, I submit that an analysis of the interlocking directorate, cooperation in research ideal, and human engineering effort will elucidate the intersecting social, intellectual, political, and economic factors that shaped knowledge production in the United States during the interwar period. Ultimately, I tell a story that centers on the formation and tenacious grip of a profound yet simple idea—that scientific research can improve mankind—and the work that the idea performed for American university scientists.
The great benefactions of Andrew Carnegie and John D. Rockefeller brought to prominence a new conception of giving. They believed that the foundations should expand their funds in a creative search from new solutions to human problems. They sought not just to cure but to prevent, not just to ease human misery but to get at the causes of that misery. And in doing so, they initiated what many believe to have been the most exciting half-century in the long history of philanthropy.—John Gardner, "Private Initiative for the Public Good"\textsuperscript{141}

Chapter Two

Philanthropic Foundations, Improving Mankind, and the Quest to Re-engineer Society through Scientific Research

The prerequisites for the development of American university research were in place by the turn of the twentieth century: scientists had created professional organizations to promote their research activities, industrial leaders had begun to contribute substantial resources toward institutions of higher education, and enterprising university presidents including Charles W. Eliot of Harvard, William Rainey Harper of the University of Chicago, Andrew D. White of Cornell, and Daniel Coit Gilman of Johns Hopkins had inaugurated research programs modeled on German institutions. American universities were well on their way to developing strong research programs, but scientists were still unable to secure direct financial support from philanthropists for laboratories, research assistant stipends, and graduate student fellowships.\textsuperscript{142} According to historian Roger Geiger, "The resources available for direct support of university research prior to World War I were irregular and uncertain. A larger difficulty, however, was to be found in the disjunction between what universities were paid to do—teach—and what they aspired to do—research."\textsuperscript{143} The perceived conflict between teaching and


research was the main obstacle for university scientists who were trying to solicit grant funds in the early twentieth century because philanthropic foundation officers believed that it was impossible for professors to manage teaching responsibilities and laboratories at the same time. This apparent conflict between teaching and research was one of the many issues that foundation officers confronted as they wrestled with the problem of how best to allocate funds.

The Rockefellers and Carnegie elected to support research by establishing institutes with in-house laboratories, where scientists could "advance" knowledge without the distraction of teaching. At the same time, they launched educational foundations that provided endowment funds, grants for operating costs, and salary supplements and pension plans for professors. Neither the research institutes nor the educational foundations offered financial support directly to university scientists. Even when Carnegie and Rockefeller established the largest foundations in the world, the Carnegie Corporation of New York (CCNY) and the Rockefeller Foundation, with combined endowments that far exceeded the federal budgets for science and education, university scientists received minimal direct benefits. CCNY became an extension of Carnegie's private giving, which mostly went to cultural and educational institutions in the form of endowment grants, while the Rockefeller Foundation officers concentrated almost exclusively on public health initiatives. The lack of support for university research remained until scientists representing the National Research Council (NRC) during World War I forced philanthropic foundation officers and trustees to re-examine funding policies.

Foundation officers worked with the NRC during the war and witnessed resourceful, innovative American scientists who were building excellent university research programs in spite of financial obstacles. The experience led foundation officers to increase their emphasis on and contributions to university research after the war. In the period during and immediately after World War I, foundation officers looked to university scientists for assistance in achieving the goal of improving mankind, which was a stated objective for Carnegie and Rockefeller philanthropies. Ill-defined social hygiene, mental hygiene and eugenics movements were transformed into policy initiatives for funding university research programs that pledged to investigate and control human behavior. The hopes and ideals of foundation officers were channeled into universities, where scientists were expected to find the cures to social dysfunction through specialized research. Foundations grew increasingly dependent on university researchers as advisors and began to hire academic scientists as officers, while university scientists at elite institutions came to rely on the grants that foundations provided to build world-class programs.

By the late 1920s, foundations and universities were in a symbiotic relationship, sharing personnel, resources, administrative organization, and guiding principles. It was this symbiosis that prompted Frederick Keppel, the president of the Carnegie Corporation of New York for over twenty years, to assert, “The foundations may be convenient, sometimes very convenient indeed to the university, but the latter could get along without it . . . the foundation, on the other hand, is absolutely dependent on the university, or, to be more accurate, on university men and women.” Keppel was referring to his reliance

145 Frederick Keppel, “Opportunities and Dangers of Educational Foundations,” in *Journal of the Proceedings and Addresses of the Twenty-Seventh Annual Conference of the Association of Graduate*
on university professors, whom he had come to see as eminently capable of determining how philanthropic funds should be spent. Foundations officers had finally arrived at the conclusion that, to achieve the goal of improving mankind, they should design policies around the needs and interests of university researchers.

In this chapter, I trace the early history of foundation philanthropy to demonstrate the continuities and ruptures between the periods before and after World War I. I show how before World War I, the development of research institutes and educational foundations helped foundation managers (officers and trustees) determine how to administer grants most effectively and established a hierarchy of recipient institutions that remained after the war. Then, I examine the relationship between foundation officers and university scientists during and immediately after World War I to unravel the complex relationships among the figures who determined postwar funding policies. The examination of World War I includes an explication of the human engineering effort as it was transformed into a major source of inspiration for the funding of research in the fields of human behavior and biology. It will become clear how foundation officers’ beliefs about the ability of science to improve mankind and further human progress were translated into major university research grants during the interwar period.

The vast majority of historians of philanthropy accept the definition of a philanthropic foundation offered by F. Emerson Andrews:

A nongovernmental, nonprofit organization having a principal fund of its own, managed by its own trustees or directors, and established to maintain or aid social,
educational, charitable, religious, or other activities serving the common welfare.\textsuperscript{146}

Prior to the turn of the twentieth century, there were only six organizations that fit this definition. The largest foundations of the late nineteenth century were the Peabody Education Fund (established 1867) and the John F. Slater Fund for the Education of Freedmen (established 1882).\textsuperscript{147} With endowments of $2 million and $1 million, respectively, the Peabody and Slater Funds paled in comparison to the foundations launched by Carnegie and Rockefeller in the early twentieth century.\textsuperscript{148} Beginning in 1901, Carnegie and Rockefeller completely dominated foundation philanthropy.

The first major foundation of the twentieth century was the Rockefeller Institute for Medical Research (RIMR), established in 1901. Frederick Gates, Rockefeller’s personal philanthropy advisor, suggested the idea of the RIMR after spending several years in the 1890s investigating the most promising avenues for “scientific” philanthropy. As he traveled and read widely on various subjects related to public health and welfare, Gates began to note the lack of consistent standards and practices in American medicine.\textsuperscript{149} He turned to Europe for inspiration, where Louis Pasteur had established a research institute in Paris in 1888 and Robert Koch had followed with an institute in

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Berlin in 1891.\textsuperscript{150} To Gates, these research institutes had the potential to have a tremendous positive impact on society because scientists working in the laboratories could use the latest instruments, including microscopes and x-rays, to attack the root causes of devastating problems like infectious disease. In 1897, Gates read William Osler's \textit{Principles and Practices of Medicine} and wrote in his journal, "When I laid down this book, I had begun to realize how woefully neglected . . . had been the scientific study of medicine."\textsuperscript{151} According to RIMR historian George Corner,

This was no ordinary textbook. Though written for physicians and students, its readable style and humane outlook fascinated many laymen, for whom it provided the best possible introduction to the medical knowledge of the time. It dealt with the whole field of internal disease in the light of the newest information and with a critical scientific outlook. Gates read the thousand page volume from cover to cover. His skepticism about the contemporary state of medical knowledge was fully confirmed, but at the same time he began to see the hope that lay in scientific investigation of the unsolved mysteries of disease.\textsuperscript{152}

After he read Osler's text, Gates decided to channel his faith in the ability of science to reduce suffering and improve mankind into support for medical research.

In late 1897, Gates wrote to the Rockefellers (both Senior and Junior) to suggest opening a medical institute modeled on the Pasteur Institute in Paris. Junior was excited about the idea and sent family lawyer Starr J. Murphy to visit institutes throughout Europe. Senior was less enthusiastic about the plan because he failed to see the

\textsuperscript{151} George Corner, \textit{A History of the Rockefeller Institute}, 23.
\textsuperscript{152} Ibid., 23.
immediate value of medical research for society. He quickly changed his mind in 1901, though, when his eldest grandson died of scarlet fever at the age of three.\textsuperscript{153} Suddenly, the need for medical innovation became very personal. Soon after his grandson died, Senior contacted Gates and pledged an annual sum of $20,000 for ten years (a total equivalent to approximately $4,500,000 in 2005) to establish the Rockefeller Institute for Medical Research (RIMR).\textsuperscript{154}

Although he had the model of the Pasteur Institute in mind, Gates was uncertain where to begin as he organized the RIMR. He turned to representatives from the few prominent American medical schools for advice about how to support research that would reduce suffering and improve “the well-being of mankind.” Gates and his medical advisory board addressed questions that would arise again and again as philanthropic foundations struggled to find a sense of direction: Which deserved more emphasis, pure or applied research? Should the balance of the funds go toward internal laboratories or to grants-in-aid for university scientists? Who was best equipped to make the important decisions, a panel of experts selected from universities or a single director with a broad knowledge of the relevant fields of inquiry?

As he wrestled with these questions, Gates realized that he needed to turn control of the RIMR over to an expert. In 1902, Gates and the Rockefellers asked William H. Welch to take the lead in designing the institute.\textsuperscript{155} Welch, the dean of the Johns Hopkins medical school, was a renowned physician who had distinguished himself by committing

\textsuperscript{153} Ibid., 23–31.  
\textsuperscript{155} George Corner, \textit{A History of the Rockefeller Institute}, 33.
wholeheartedly to research and laboratory science. The first step that Welch took was to recruit his colleague and protégé, Simon Flexner, to serve as director of the RIMR. Although he had never completed high school, Flexner had shown enough talent to obtain degrees in pharmacy and medicine at Johns Hopkins in 1889. In the 1890s, Flexner had become a highly respected professor of pathology and bacteriology at the University of Pennsylvania. By the time he took the director post at the RIMR, Flexner had joined Welch in the “innermost circle of the medical elite.”

Welch and Flexner began their administration at the RIMR by creating a grants-in-aid program that distributed small allotments to medical researchers. To acquire grant funds, scientists had to apply directly to Welch or Flexner on a yearly basis, at which point one of the RIMR officers would determine the amount of the allotment. As more applications arrived, this practice became untenable and the grants-in-aid program quickly fell into disfavor. Trustees complained that the application process was demeaning and wasteful because scientists had to fill out forms and present their work year after year to receive relatively small sums with no guarantee of future support. When the RIMR moved into its permanent Manhattan buildings and laboratories in April 1906, the grants-in-aid program was discontinued and replaced by in-house medical research laboratories run by full-time appointees.

The early years of the RIMR affected the policies and personnel of the Rockefeller philanthropies. The RIMR was the starting point for careers in foundation

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158 Gerald Jonas, Circuit Riders, 37.
philanthropy. Of the founding trustees of the RIMR, 67% became trustees of Rockefeller’s General Education Board (GEB) and 83% became founding trustees of the Rockefeller Foundation.\textsuperscript{160} Several of the RIMR administrators also became key players in the Carnegie philanthropies. For example, Simon Flexner was the director of the RIMR, a trustee for the Carnegie Institution of Washington (CIW), and a founding trustee for the Rockefeller Foundation, while William H. Welch served as both president of the RIMR and as a trustee for the CIW. The RIMR thus created the first links in an interlocking directorate of officers and trustees that controlled foundations for years to come.

In terms of policy, the failed attempt on the part of RIMR officers to create a grants-in-aid program made future leaders of Rockefeller foundations reluctant to provide small grants in any field. The rejection of the grants program in favor of internal RIMR laboratories frustrated university scientists, who hoped that they would receive foundation support for their existing research and graduate programs.\textsuperscript{161} In the wake of the RIMR officers’ decision to operate independent laboratories rather than administer grants to university researchers, “scientists’ eager anticipation of their new patrons soon turned to disappointment and conflict.”\textsuperscript{162} University scientists would have to find research funds elsewhere.

Andrew Carnegie gave university scientists renewed hope that they would receive philanthropic support for their research programs. Carnegie had spent the 1890s casting


\textsuperscript{161} Roger Geiger, \textit{To Advance Knowledge}, 62.

about for more ways to fulfill his ambition to be the consummate "scientific"
philanthropist. Unlike Rockefeller, Carnegie did not have an advisor like Frederick Gates
or a devoted son to point him toward a particular area of concentration, so he asked
experienced university leaders for advice. Carnegie found no shortage of enthusiastic
consultants, as his biographer Joseph Frazier Wall explains:

    Grover Cleveland, a newly appointed trustee of Princeton University, Woodrow
Wilson, president of Princeton, Charles Eliot of Harvard, Daniel Coit Gilman of
Johns Hopkins, Elihu Root, loyal alumnus of Hamilton College, and the
indefatigable Andrew White, president emeritus of Cornell University, all knew
precisely how Carnegie could best spend his money, and they were eager to tell
him.163

This elite circle of Carnegie's associates proposed two alternatives, a national university
or a research institute, with all agreeing that Washington, DC was the optimal location.
The impasse between proponents of a national university, led by Andrew White, and
advocates for a research institute centered on the perceived conflict between teaching and
research within the university. Carnegie sided with those who argued that researchers
needed space where they could escape teaching responsibilities. In 1902, Carnegie
donated $10,000,000 (equivalent to $220,000,000 in 2005) to establish the Carnegie
Institution of Washington (CIW) as an independent research institute with an affiliated
grants-in-aid program for university scientists.164

164 Daniel Kevles, "Foundations, Universities, and Trends in Support of the Physical and Biological
Sciences, 1900–1992," Daedalus 121, no. 4 (fall 1992): 195; All inflation adjustments calculated on
The charter aim of the Carnegie Institution of Washington (CIW) was "to encourage, in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the improvement of mankind." The charter statement epitomized the assumptions behind "scientific" philanthropy that guided Carnegie and Rockefeller as they built major foundations in the early twentieth century. The problem was implementation. The Articles of Incorporation included an elaborate and wide-ranging set of priorities that did little to clarify how the quest to "improve mankind" through research was to be achieved:

The particular business and objects of the Institution are the promotion of study and research, with power . . . to conduct, endow, and assist investigation in any department of science, literature, or art, and to this end cooperate with governments, universities, colleges, technical schools, learned societies, and individuals; to appoint committees of experts to direct special lines of research; to publish and distribute documents; to conduct lectures; to hold meetings; to acquire and maintain a library; and, in general, to do and perform all things necessary to promote the objects of said Institution. This was an ambitious assortment of tasks to fulfill, but Carnegie believed that bold plans were the best plans.

When university scientists learned of the sizeable Carnegie Institution of Washington (CIW) endowment and its broadly defined objectives for encouraging

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research, they were understandably excited. After all, the charter had included the promise that the CIW would discover the exceptional man in every department of study whenever and wherever found, inside or outside schools, and enable him to make the work for which he seems specially designed his life work.\textsuperscript{168}

In addition to finding and financing the “exceptional man,” the CIW guidelines included provisions for aiding university facilities and professors. According to Nathan Reingold, professors believed “that the bulk of the Carnegie gift was destined for universities, their faculty members, and their students. Indeed many drew that conclusion—to their bitter disappointment as the Institution’s program unfolded.”\textsuperscript{169} Like the RIMR, the CIW moved further and further away from university scientists in its early years.

The lack of emphasis on university scientists was particularly surprising considering that the first president of the Carnegie Institution of Washington (CIW) was Daniel Coit Gilman, the inaugural president of Johns Hopkins University. Gilman had served on the advisory board for the CIW, and he had assumed that university scientists would be a high priority. When he tried to initiate programs to support university science as the CIW president, however, Gilman was overruled by the trustees, who were convinced that university teaching responsibilities interfered with research.\textsuperscript{170} Gilman, who was over seventy and ready to retire, chose not to fight the trustees. Before he left in late 1903, he made a last ditch effort to carve out a niche for university scientists at the CIW by setting up a grants-in-aid program that was modeled on the RIMR.

\textsuperscript{169} Nathan Reingold, “National Science Policy in a Private Foundation,” 335.
\textsuperscript{170} Ibid., 317.
Gilman’s successor Robert Woodward, who took over as president in 1904, initially expanded the grants-in-aid program, but he soon began to struggle with the same problems that had doomed the RIMR grants-in-aid program. The sheer number of grant applications created an administrative nightmare, because each document went directly to Woodward. Not only was Woodward personally responsible for determining who would receive funds and the size of each grant, but he was also charged with the task of notifying both grant recipients and rejected applicants. Since the grants required applications for renewal on an annual basis, Woodward was perpetually swamped with correspondence from hopeful researchers.

The experience of coordinating the grants-in-aid program left Woodward with a negative impression of university scientists and administrators. He was dismayed when university administrators used grant money to replace rather than supplement existing sources of research funds. Woodward began to rail against the inadequacy of university research departments, arguing that professors were distracted by teaching responsibilities and that inexperienced assistants, rather than the “exceptional man,” received the bulk of grant money.¹⁷¹ Over the course of his presidency, which lasted from 1904 until 1920, Woodward diminished the grants-in-aid program and focused instead on developing a handful of major projects that were run by the CIW. The two most significant projects were the Mt. Wilson Observatory in Pasadena, which was coordinated and directed by astronomer and master scientific entrepreneur George E. Hale, and the Station for Experimental Evolution at Cold Spring Harbor in Long Island, which was managed by the eugenicist Charles E. Davenport. In 1911, Carnegie donated an additional

$11,000,000 (equivalent to $217,000,000 in 2005) to the CIW specifically for these projects.\(^{172}\)

The CIW, like the RIMR, provided trustees and officers with opportunities to begin careers in philanthropic foundation management. Of the founding trustees of the CIW, over 45% became officers or trustees of other foundations.\(^{173}\) There was a notable difference between the roles of the CIW and RIMR within the networks of Carnegie and Rockefeller foundations, though. From its inception, the CIW was the center of operations for all activities related to scientific research for the Carnegie philanthropic foundations, even after the inauguration of the Carnegie Corporation of New York (CCNY) in 1911. Any applications for scientific research were automatically forwarded to the CIW for assessment. Consequently, Carnegie intervention in scientific research was limited to the fields represented by CIW laboratories and to the preoccupations of CIW laboratory directors.

The lack of viable collaboration with universities greatly circumscribed the impact that the CIW and RIMR had on scientific research in the United States in the early twentieth century. University scientists had worked long and hard to build graduate programs, disciplinary associations, and departments, and they were unwilling to abandon their academic posts for independent research institutes.\(^{174}\) The grants programs had provided a brief glimmer of hope for university scientists. When the RIMR and CIW grants programs began to crumble, university scientists saw their only potential avenue


\(^{174}\) Roger Geiger, To Advance Knowledge, 66.
for philanthropic funding disappear. As Daniel Kevles asserts, “in the decade before World War I . . . the Carnegie and Rockefeller gifts to research raised the hopes of university science but did little to fill its treasury.”

Kevles’s statement could be slightly amended to describe the educational foundations that Rockefeller and Carnegie established in the first decade of the twentieth century: gifts to universities did much to raise the hopes of researchers, but did little in terms of direct support. There was never any doubt that Rockefeller and Carnegie were committed to supporting educational institutions; they had contributed millions in endowment funds to universities. Their emphasis on education was not an emphasis on research, however. Carnegie and the Rockefellers believed strongly that high quality instruction was the key to an excellent education, and their educational foundations focused almost exclusively on teaching.

The Rockefeller foray into educational foundations began with the General Education Board (GEB), which eventually became a major source of funding and reform initiatives for American institutions of higher education. The GEB was initially designed by John D. Rockefeller, Jr. ("Junior") with the help of his trusted mentor, Frederick Gates. In 1901, Junior participated in a conference of the Southern Education Board, a gathering of southern education reformers and philanthropists. At the meeting, he learned that the Peabody and Slater foundations were struggling to maintain their reform efforts due to limited funds. Junior decided that his family could solve the problem by pooling resources and personnel with existing southern education reform philanthropies to create a new foundation. Within a few months of the conference, Junior and Gates

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gathered with directors of the American Baptist Education Society, the Peabody and Slater Funds, and the Southern Education Board to coordinate what became the General Education Board (GEB). With eleven trustees, including J. L. M. Curry, the executive director of the Peabody Fund, and Wallace Buttrick, the executive director of the Slater Fund, the GEB was launched in 1902 for “the promotion of education within the United States, without distinction of race, sex, or creed.” Buttrick, a Baptist minister and educator who had spent almost twenty years working to reform southern education, was selected as “Secretary and Chief Executive Officer.”

The initial group of GEB trustees was dominated by a combination of southern education reformers, Rockefeller family advisors and employees, and RIMR trustees and officers. In the first few years, GEB trustees mostly expanded programs that southern education reformers had promoted in the late nineteenth century, including hosting farm demonstrations and building elementary and secondary schools in rural areas. Gates felt that the GEB was too limited and that institutions of higher education needed more attention. He appealed directly to the Rockefellers and insisted on making the reform of higher education a priority. When Senior contributed a $10,000,000 endowment gift to the GEB in 1905 (equivalent to $205,000,000 in 2005), Gates ensured that the donation letter included a statement mandating that the income from the endowment be used “to

178 “Wallace Buttrick,” Rockefeller Archive Center Biography Files, Rockefeller Archive Center, Tarrytown, New York (henceforth RAC).
promote a comprehensive system of higher education in the United States." \(^{180}\) Gates held that "the institutions of higher education in the United States should form a system 'territorially comprehensive, harmoniously related, individually complete, and so solidly founded'," and that the GEB should work toward cultivating such a system. \(^{181}\) The other trustees yielded to Gates, whose access to the Rockefellers was unmatched, and elected him Chairman of the Board in 1907.

Gates began his effort to reform and coordinate higher education by conducting a survey of the 700 private colleges and universities in the United States. The survey excluded state schools because Gates had learned an important lesson from Barnas Sears of the Peabody Fund: an emphasis on smaller, private schools was less complicated than attempting to cooperate with state legislatures on behalf of public schools. \(^{182}\) In the 1880s, Sears had tried to establish normal schools in Tennessee in a joint effort with the state legislature, but the Peabody Fund had become solely responsible for financing the project. After learning of Sears's problems with the Tennessee state legislature, Gates decided to draw clear distinctions between private and public schools, with the former receiving a much larger share of the financial resources. This preference for dealing with private institutions was a critical aspect of GEB policy because it influenced which universities received endowment funds and, later, the degree to which the foundations contributed to scientific research at select schools. \(^{183}\)

In his report on private institutions, Gates contended that American higher education was in a chaotic state and that most colleges were barely viable. He identified


\(^{183}\) Ibid., 35.
only twenty-five colleges or universities with incomes that exceeded $500,000 and noted that most of the schools in the survey were barely providing a secondary level education.\textsuperscript{184} Based on this survey, Gates and Buttrick determined that the General Education Board (GEB) should use its money and influence to promote only the most promising institutions. The GEB would fund private colleges and universities with the ability to raise matching funds (often requiring dedicated alumni and a location near an urban area) and the allotments would be exclusively for endowment purposes.\textsuperscript{185} In practice, the institutions that fit these requirements were few; twenty colleges and universities received 75\% of the GEB funds in the first two decades of the foundation’s existence.\textsuperscript{186} Both Buttrick and Gates noted that it was easy to find worthy recipients in the Northeast, but the South and Midwest presented formidable challenges. Unsurprisingly, Harvard, Columbia, Yale, Princeton, and the University of Chicago received the most substantial GEB allotments.\textsuperscript{187}

The years that Buttrick and Gates spent disseminating endowment funds had a significant impact on the overall development of higher education in the United States. Between 1902 and 1925, the GEB contributed $60,000,000 in endowment grants for colleges and universities, and the recipient institutions raised $140,000,000 in matching funds.\textsuperscript{188} Thus, the GEB facilitated the contribution of $200,000,000 (equivalent to approximately $3,000,000,000 in 2005) to the permanent funds of American institutions.

\textsuperscript{184} Raymond Fosdick, \textit{Adventure in Giving}, 128.
\textsuperscript{188} Raymond Fosdick, \textit{Adventure in Giving}, 135; Ernest Hollis, \textit{Philanthropic Foundations and Higher Education}, 201.
of higher education. In addition, Senior donated $50,000,000 (equivalent to
$615,000,000 in 2005) to the GEB in December 1919 explicitly for professors' salaries at
institutions already receiving support from the foundation to assist the colleges and
universities as they recovered from World War I.¹⁸⁹ The distribution of these
supplemental funds for professors' salaries followed the same guidelines as those
established for general endowment grants. By using the substantial funds at their
disposal to encourage the institutions that they deemed worthy, Buttrick and Gates helped
to create an unofficial set of standards for American colleges and universities.¹⁹⁰

Under the auspices of the GEB, philanthropic foundation officers became key
players in the reform of American higher education in the early twentieth century. By
conducting educational surveys, determining policies for fund distribution, and meting
out endowment grants, GEB officers and trustees learned both how to run an effective
philanthropic foundation and how to communicate with university administrators. The
GEB managers imparted these lessons to fellow trustees and officers that they
encountered in other Rockefeller philanthropic foundations. Since over 60% of early
GEB trustees and officers went on to serve in the Rockefeller Foundation, they were able
to ensure that their top priority—supporting well-established, elite institutions of higher
education—guided the policies of Rockefeller philanthropy for years to come.¹⁹¹

The Carnegie complement to the GEB was the Carnegie Foundation for the
Advancement of Teaching (CFAT), an organization founded in 1905 to provide pensions
and salary supplements for professors. The germ of the idea for the CFAT began when

¹⁸⁹ Raymond Fosdick, *Adventure in Giving*, 144.
¹⁹¹ Raymond B. Fosdick, *The Story of the Rockefeller Foundation*, 309–312; General Education Board,
Carnegie was a trustee of Cornell University in the 1890s, where he was surprised to see how professors struggled to support their families due to low salaries. Carnegie had the salary issue on his mind when he met Henry S. Pritchett, the president of MIT, at a reorganization meeting for the CIW in 1904. Pritchett presented a solution: Carnegie should reward professors for many years of service to institutions of higher education by providing them with pensions, which colleges and universities did not offer at the time. Pritchett proposed a pension plan based on an annual endowment income of $500,000. To receive a pension, professors would have to teach for at least thirty years and be over the age of 65. This was the essence of the 1905 charter for the Carnegie Foundation for the Advancement of Teaching (CFAT) that was approved by Carnegie and trustees who represented “25 leaders in higher education,” including the presidents of Princeton, Yale, Stanford, Harvard, Columbia, and the University of Chicago (all future foundation favorites). Pritchett was selected as president and his close friend Charles W. Eliot, the president of Harvard who had played a central role in the reform of the American university, was appointed as the chairman of the board of trustees.

In his capacity as president of the CFAT, Pritchett announced that the “board could and would engage in standard setting as a means for reforming and distinguishing among the motley array of schools and colleges that had grown up all around the United States.” Only the institutions of higher education that proved their worth according to

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Pritchett’s standards were eligible to receive pension funds. The standards were clear and rigid, as Pritchett explained in a 1906 *Outlook* article titled “Mr. Carnegie’s Gift to the Teachers”:

In a word, this Foundation is intended, not so much to benefit a particular individual or class of individuals, as to advance and dignify the profession of the teacher and the cause of higher education which the teacher serves. . . . An institution to be ranked as a college must have at least six professors giving their entire time to college and university work, a course of four full years in liberal arts and sciences, and should require for admission not less than the usual four years of academic or high school preparation. . . . To accept less than this is to throw away any real standard of work both for the college and for the high school. 197

The four years of high school preparation guidelines included a definition of a credit hour that became known as the “Carnegie unit,” a standard that was soon widely accepted by colleges and universities as part of the admissions process.

The guidelines also excluded a large number of institutions, including those with an endowment of less than $200,000, state universities, and colleges affiliated with a particular religious denomination. According to official policy, pension funds would only be distributed to:

Universities, colleges and technical schools of requisite academic grade, not owned or controlled by a religious organization, and whose acts of incorporation or charter specifically provide that no denominational or sectarian test shall be

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applied in the choice of trustees, officers or teachers, nor in the admission of students. 198

Like the GEB officers, Pritchett wanted to ensure that the institutions that received support could sustain themselves, which accounted for the stipulation mandating a minimum endowment fund. The exclusion of state institutions was similar to the GEB as well, because Pritchett was reluctant to deal with legislatures.

The rules for pensions became highly controversial as soon as Pritchett announced them. As Theron Schlabach contends, “There was something distasteful about Pritchett’s entire conception of the Foundation’s purpose, to wield its influence in such a way as to set the educational standards for the entire country.” 199 State institutions and denominational colleges found the provisions especially offensive. Pritchett justified his decisions by arguing that state institutions already benefited from the wealth of industrialists like Carnegie through taxation and that there were simply too many denominational colleges to maintain any kind of educational quality. The exclusion of state institutions did not last long, though, because Carnegie felt compelled to address the criticism by contributing an additional $5,000,000 (equivalent to $99,000,000 in 2005) in 1908 to provide pensions for public university professors. 200 The rules barring denominational colleges, however, remained stringent. The restriction held because Pritchett fervently believed that Christian denominations were building colleges to satisfy small constituencies without paying adequate attention to academic standards, faculty qualifications, or the educational backgrounds of admitted students. In addition, reliance

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198 Carnegie Foundation for the Advancement of Teaching, A Third of a Century of Teachers Retirement: Records of Measures Adopted to Carry out the Purposes of the Founder (New York: Carnegie Foundation for the Advancement of Teaching, 1940), 75.
199 Theron F. Schlabach, Pensions for Professors (Wisconsin: University of Wisconsin, 1963), 49.
200 Carnegie Foundation for the Advancement of Teaching, A Third of a Century of Teachers Retirement, 3.
on one denomination limited the capacity of these schools to draw from the larger community for financial support. To Pritchett, these colleges were hopelessly “provincial” and doomed to fail, and he had no interest in rescuing struggling colleges.\textsuperscript{201} As Pritchett wrote in the \textit{Atlantic Monthly} in 1908, “the problem before us now is, not the building of more colleges, but the strengthening of those which exist, and the bringing of some measure of educational unity into our whole system of education.”\textsuperscript{202} Despite the controversy, which centered “on the degree of control the Foundation was justified in exercising,” the standards that Pritchett and the CFAT set for colleges and universities greatly influenced the system of national accreditation for American institutions of higher education.\textsuperscript{203} Pritchett achieved his fundamental goal of reducing the chaos in higher education and establishing standards for secondary schools to prepare students for colleges and universities. The Association of American Universities (AAU) officially adopted the CFAT standards in 1913, when the guidelines were incorporated into the Association’s definition of American institutions whose bachelor’s degrees should be accepted by foreign universities. As Walton John notes in his Department of the Interior study of American graduate education,

The acceptance by the AAU and regional accrediting associations of the technique used by the foundation in evaluating colleges and universities was an event of great importance in that this procedure has come to be the defining force in standardizing our higher education institutions.\textsuperscript{204}

\textsuperscript{201} Henry S. Pritchett, “Education and the Nation,” \textit{Atlantic Monthly} 109, no. 4 (April 1912): 543.
\textsuperscript{202} Henry S. Pritchett, “The Organization of Higher Education,” \textit{Atlantic Monthly} 102, no. 6 (December 1908): 784.
\textsuperscript{203} Ernest Holli, \textit{Philanthropic Foundations and Higher Education}, 57.
The AAU executive committee freely admitted that “in studying the development of university standards of this association that in the beginning, qualitative standards were determined largely by the personal acquaintance of a few leaders with sister institutions rather than by a set of formal quantitative standards.”\textsuperscript{205} The CFAT guidelines had provided a basis on which to build a normative accreditation system and Pritchett had used his authority over pension funds to make a lasting impact on the organization of American higher education.

The pension program was incorporated into the national education system through the establishment of the Teachers Insurance and Annuity Association (TIAA) in 1918 with Pritchett as president.\textsuperscript{206} College-level instructors were able to contribute to TIAA accounts, which were supplemented by CFAT contributions. Gradually, the CFAT ceded control of the pension funds to the TIAA and ceased acting as an operating foundation. The TIAA grew to become a contributory pension system for teachers at all levels of the education system in the United States.

The Carnegie Foundation for the Advancement of Teaching (CFAT) influenced higher education through surveys and reports as well. A series of bulletins analyzing professional programs included one of the most significant reports issued by a philanthropic foundation, Abraham Flexner’s \textit{Bulletin No. 4: Medical Education in the United States}, which the CFAT distributed to medical schools throughout the country in 1913. That year, the overwhelmingly positive response to the bulletin inspired Carnegie to add an endowment gift of $1,250,000 (equivalent to $24,000,000 in 2005) for a Division of Educational Enquiry \textit{[sic]}. Abraham Flexner began to serve as an

\textsuperscript{205} Ibid., 32.
\textsuperscript{206} Ellen Condliffe Lagemann, \textit{Private Power for Public Good}, 54.
intermediary between Carnegie and Rockefeller philanthropies. He already had a family connection to Rockefeller philanthropy: Abraham was the older brother of Simon, the director of the RIMR. More notably, Flexner’s bulletins (he followed with an analysis of medical education in Europe) facilitated cooperation between the CFAT and the GEB, as Harold Savage explains:

The wide public attention that Flexner’s Bulletins gained established a reputation for the Carnegie Foundation. . . . Even if the two reports “simply expressed and disseminated what enlightened medical educators have been working to bring about,” their vigor, completeness, and timeliness became fundamentals to great changes. The medical curriculum was remade. . . . Guided by Flexner’s concepts the Rockefeller endowments, which Flexner joined in 1913, and the Carnegie Corporation invested more than $130 million in medical education, a sum which attracted as much again from other sources. . . . The Flexner studies, plus Rockefeller and Carnegie money and other benefactions they spurred, wrought what was probably the greatest improvement that ever took place during four decades in professional education.207

Abraham Flexner thus contributed to “the Carnegie Foundation’s professionalizing campaign—its effort to define and institutionalize nationally uniform, science based, training paradigms that would serve as prerequisites for entrance into the professions.”208

The bulletins exemplified CFAT efforts to encourage the transformation of American institutions of higher education into facilities for professional development.

207 Harold Savage, Fruit of an impulse, 108.
208 Ellen Condliffe Lagemann, Private Power for the Public Good, 59.
The GEB and the CFAT became de facto accrediting agencies for institutions of higher education in the early twentieth century. In Frederick Rudolph's assessment, "The foundations discovered what others already knew: The definition of an American college was in disarray, and in fact, if a list of colleges and universities was to be drawn up, the foundations themselves would have to decide what a college was." Through their funding choices, as well as reports and surveys, the leaders of the GEB and CFAT used the financial resources at their disposal to establish a definition of quality American higher education that included high admission standards, attention to professional development, and financial criteria.

The decisions that the GEB and CFAT made about allotting funds were based on assumptions and goals that became part of the long-term philanthropic foundation approach to financing institutions of higher education. Both foundations selected colleges and universities that already had strong financial support, privileging institutions that were in or near major urban centers and well-established, elite schools in the Northeast. Thus, one of the most significant effects of GEB and CFAT contributions to higher education in the early twentieth century was to increase the separation a small group of elite, well-funded colleges and universities from the mass of institutions of higher education. Many of these universities, especially Yale, Harvard, Columbia, the University of Chicago, and the California Institute of Technology, were able to build on the initial support that they received from the GEB and CFAT when they constructed world-class research programs in the 1920s and 1930s.

209 Ibid., 221.
211 Frederick Rudolph, *Curriculum*, 226.
Establishing the research institutes (CIW and RIMR) and educational foundations (GEB and CFAT) had provided Carnegie and Rockefeller with opportunities to experiment with the implementation of "scientific" philanthropy, and the experiments yielded many fruitful results. Yet, the wealthiest men in the United States were determined to go far beyond their initial efforts in their quest to improve mankind through philanthropy. In the 1910s, Rockefeller and Carnegie established the largest and most ambitious foundations in the world: the Rockefeller Foundation and the Carnegie Corporation of New York (CCNY). For Rockefeller and Carnegie, these massive foundations were to become their crowning achievements in "scientific" philanthropy.

The Rockefeller Foundation was conceived in 1909, when Rockefeller, Sr. deeded 72,569 shares of Standard Oil, valued at over $50,000,000 (equivalent to $1,026,500,000 in 2005), to Rockefeller, Jr., Frederick Gates and Harold McCormick (his son-in-law) in a trust to promote the well-being and to advance the civilization of the peoples of the United States and its territories and possessions and of foreign lands in the acquisition and dissemination of knowledge, in the prevention and relief of suffering, and in the promotion of any and all the elements of human progress.212

The language that John D. Rockefeller, Sr. used in this deed encapsulated the hopes and values that defined "scientific" philanthropy. He explicitly tied "well-being" and the "advance of civilization" to "the acquisition and dissemination of knowledge," and thus linked "human progress" to the advancement of research and education. Senior went to the heart of the matter in his concluding statement: "The most significant contribution which any foundation can make, particularly a foundation with funds as large as ours, lies

in long-range objectives which attack the causes of human ills and maladjustment rather than their effects.\textsuperscript{213} By establishing the Foundation, Senior believed that he could tackle all of the objectives of “scientific” philanthropy at once. Senior outlined the principles that the Foundation should follow when distributing funds: no charity or relief, financial participation only (not an operating agency), no propaganda or political alliances, and flexibility to adapt to changing circumstances. These guidelines, along with the mandate to prevent as well as to relieve suffering by “curing evils at their source,” would later be cited to justify Foundation support for university scientific research.

To gain national recognition for the Rockefeller Foundation, Senior decided to submit an application for a Congressional charter. When Senior applied to Congress, he assumed that approval was a formality because the GEB had been granted a federal charter in 1903 without incident.\textsuperscript{214} He was mistaken. By 1910, when the Congress considered the Foundation charter application, senators had grown leery of concentrated wealth and the potential for industrial magnates like Rockefeller to abuse the power that came with control of a sizeable fortune. To satisfy the skeptical legislators, Rockefeller amended his application several times between 1910 and 1912. The new provisions were designed to reassure legislators that the Foundation would neither be a vehicle for increasing Rockefeller wealth and power nor a source of conflict with the government in terms of public policy: no more than $100,000,000 total assets, no reinvestment of income to supplement the principal endowment, Congress could order that all funds be

\textsuperscript{213} “Report of Committee on Appraisal and Plan,” December 11, 1934, pp. 35–39, folder 170, box 22, Series 900, Record Group 2.1, Rockefeller Foundation Archives, RAC.
distributed after 100 years, and all trustee nominations would be submitted to the
President of the United States, the Chief Justice of the Supreme Court, the President of
the Senate, the Speaker of the House and the presidents of Harvard, Yale, Columbia,
Johns Hopkins and the University of Chicago for approval. Still, Congress refused to
act. Defeated, Senior and Junior submitted an application to the New York legislature
that was promptly approved in 1913. The final charter had a simpler mandate than the
original: “To promote the well-being of mankind throughout the world.” Junior took
over as president and put together a board of trustees filled with representatives from
other Rockefeller philanthropies. Over 80% of the initial Rockefeller Foundation trustees
had served on other Rockefeller boards, mostly the GEB.

Meanwhile, Carnegie was perturbed that he had failed to achieve his goal of
giving away his wealth despite years of continual philanthropic endeavors. In 1911,
Carnegie decided to create a foundation with the bulk of his remaining fortune. The
endowment of $125,000,000 (equivalent to $2,475,000,000 in 2005) made the new
Carnegie Corporation of New York (CCNY) the largest philanthropic foundation in the
world. After witnessing the problems that Rockefeller had encountered when applying
for a federal charter, Carnegie went directly to the New York legislature for approval.
According to the official New York charter, the Carnegie Corporation of New York
(CCNY) was dedicated to promoting “the advancement and diffusion of knowledge and

216 John Lankford, Congress and the Foundations in the Twentieth Century (Wisconsin: Wisconsin State
217 Raymond B. Fosdick, The Story of the Rockefeller Foundation, 309–312; General Education Board,
219 John Lankford, Congress and the Foundations, 16.
understanding." The CCNY mandate echoed the early draft of the Rockefeller Foundation charter, which referred to "the acquisition and dissemination of knowledge." Again, the combination of research and teaching was at the forefront, at least in theory.

Carnegie took charge of the CCNY with his close friends and heads of his other philanthropies serving as trustees. Until Carnegie died in August 1919, CCNY was an extension of his personal philanthropy, with substantial sums devoted to libraries and hero funds. In terms of contributions to colleges and universities before World War I, Carnegie followed a similar practice to that of the GEB by donating large sums to the endowment funds of select institutions.

Despite their optimistic and progressive charters, the Rockefeller Foundation and Carnegie Corporation of New York (CCNY) were plagued by controversy in the years immediately prior to the onset of World War I due to public suspicions about the motives of "robber barons" who had conducted shady business deals and abused laborers to accumulate their fortunes. Almost immediately after the CCNY and Rockefeller Foundation were established, the House of Representatives convened a Commission on Industrial Relations. The Commission began

with a study of labor exploitation, [and] went on to investigate concentrations of economic power, interlocking directorates, and the role of the then relatively new large charitable foundations (especially Carnegie and Rockefeller) as instruments of power concentration.

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220 Carnegie Corporation of New York, Board of Trustee Minutes, November 10, 1911, Columbia University Archives, New York, NY (henceforth "Columbia").
222 Ernest Hollis, Philanthropic Foundations and Higher Education, 204.
The hearings were essentially a public rebuke of Carnegie and Rockefeller, with their foundations denounced as evidence of the spoils of corrupt business practices.

Carnegie and both Rockefellers (Senior and Junior) testified before the commissions to defend their foundations, which only served to inflame the spectators. Since Carnegie was in charge of CCNY, he was vulnerable to criticism, but his opponents were stymied by the fact that he had retired from business. The Rockefellers were in a slightly different position. Senior was neither actively involved in the foundations nor in business. Junior, on the other hand, was both the president of the Rockefeller Foundation and the majority shareholder for Colorado Fuel and Iron (CFI), meaning he was in the upper echelons of both philanthropy and business. Although his position as president of the Foundation was largely symbolic because Secretary Jerome D. Greene handled most of the official duties, Junior was still seen as the main representative for the foundation. To his critics, it appeared that Junior was uniquely situated to abuse power. Junior’s handling of CFI labor disputes certainly did nothing to disabuse his detractors of their beliefs.

Colorado Fuel and Iron (CFI) workers initiated a massive strike on September 26, 1913 that turned into one of the most devastating episodes of violent labor suppression in American history. The strike was organized by United Mine Workers to protest extraordinarily dangerous conditions in the Colorado mines, where 464 men had been killed or maimed in accidents in the eight months before the strike began. Rather than negotiate with the strikers, Junior told his managers to hold firm. Managers punished strikers by evicting them and their families from company-owned homes. The strike participants ended up living in a tent colony in Ludlow, where they hoarded weapons and
awaited what seemed to be an inevitable showdown. After two weeks of a tense standoff, a gunfight broke out between striking workers and a ragtag militia brought in by CFI managers. After the shooting subsided, the militia set fire to the workers’ tents. Two women and eleven children who had cowered in a ditch during the gunfight were trapped, and they suffocated to death from smoke inhalation. At the time of his testimony before the Commission on Industrial Relations, Junior was forced to defend the indefensible: the deaths of women and children at the hands of his CFI managers during what became known as the “Ludlow Massacre.”

Junior, who initially claimed that he was unaware of the escalation of tensions in Ludlow because he was not involved in the daily workings of the mines (he had not visited Colorado in over ten years), had a change of heart by the time he testified before the Commission on Industrial Relations in 1915. He formally apologized to the workers and their families, and then went to live with the miners for two weeks in September of that year. At the end of his stay, Junior promised new houses, schools and recreation facilities, as well as support for a “company union.”

Though these gestures were hardly enough to compensate for the wreckage that had preceded his visit, Junior’s efforts did much to quell the anti-Rockefeller and related anti-foundation fervor that was building in 1914 and 1915. Nonetheless, the Final Report of the Commission on Industrial Relations included a statement that accused philanthropic foundations of subverting democracy:

The domination by the men in whose hands the final control of a large part of American industry rests is not limited to their employees, but is being rapidly extended to control the education and “social service” of the Nation. This control

is being extended largely through the creation of privately managed funds for indefinite purposes, hereinafter designated "foundations." ... Two groups of the "foundations," namely, the Rockefeller and Carnegie foundations, together have funds amounting to at least $250,000,000, yielding an annual revenue of at least $13,500,000, which is at least twice as great as the appropriations of the Federal Government for similar purposes, namely, education and social service. 225

This statement revealed both how influential the foundations were in the field of education and the degree to which legislators felt threatened by this influence. Carnegie and the Rockefellers took different approaches to combating the antagonism that was directed at their foundations. Carnegie essentially ignored his critics, preferring to continue with the practices that he started when he founded CCNY. In contrast, the Rockefellers made a public effort to distance themselves from the direct control of the Rockefeller Foundation and encouraged the Foundation officers to concentrate on issues that were uncontroversial and patriotic, particularly war relief and public health.

By the time the Commission on Industrial Relations testimony drew to a close, the Rockefeller Foundation had become fully invested in war relief efforts. Between 1914 and the cessation of hostilities in November 1918, the Foundation contributed a total of $22,300,000 (equivalent to $322,000,000 in 2005) to war relief, the largest sum of any organization in the United States. 226 The foundation wartime activities began with European war relief and public health initiatives run by two organizations: the Sanitary Commission and the Bureau of Social Hygiene. The Sanitary Commission was dedicated

226 "Report of Committee on Appraisal and Plan," December 11, 1934, p. 12, folder 170, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
to eradicating infectious diseases including yellow fever, malaria, and hookworm in poor regions of the United States and the world. The Bureau of Social Hygiene, which was run by Junior and longtime Rockefeller advisors Jerome Greene and Starr J. Murphy, focused on informing soldiers about hazards related to alcohol and prostitution.227

Bureau of Social Hygiene officers followed the same logic that had guided the development of the eugenics and institutionalization movements in the late nineteenth century. In particular, Bureau officers were preoccupied with controlling sexual behavior. The chief inspiration for the Bureau was "sexologist" Havelock Ellis, who advocated for a "scientific" approach to sexual purity in his 1912 book, The Task of Social Hygiene. Junior, Jerome Greene, and Starr Murphy subscribed to Ellis's argument that "it is the control of the reproduction of the race which renders possible the new conception of Social Hygiene."228 Based on a mixture of Ellis's sexology, hereditarian assumptions and a popular understanding of Freudian psychoanalysis, Bureau of Social Hygiene officers concluded that a reduction in the incidence of venereal disease and lascivious sexual behavior would lead inexorably to improved mental and social fitness.

The Bureau of Social Hygiene became the inspiration for myriad Rockefeller Foundation activities during and after the war, including "mental hygiene" programs and long-term projects designed to investigate sexuality.229 The mental and social hygiene efforts became critical sources for rationales to support university research after the war, based on logic that Rockefeller Foundation officers explained in their reports:

227 Raymond Fosdick, The Story of the Rockefeller Foundation, 28–79.
229 "Report of the Committee on Appraisal and Plan, December 11, 1934, p. 10, folder 170, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC."
It was the intention of the Trustees that "the related fields of heredity, alcoholism and venereal disease" should be included with mental hygiene and "should be approached as one broad problem, the solution of which would require the effective coordination of the several lines of inquiry subject to adequate supervision."  

The list of "evils" that the Foundation intended to combat through mental and social hygiene projects ranged far and wide: "insanity, feeblemindedness, alcoholism, drug habit, prostitution, crime, venereal disease, infant mortality, child labor, illegitimacy, pauperism, divorce, bad milk and other foods, bad housing, tuberculosis, typhoid fever."  

Although this diffuse collection of "evils" hardly seemed capable of providing a salient set of policy objectives, the belief that such a wide range of problems could be addressed through research in the fields related to social and mental hygiene continued to guide Foundation officers as they made funding decisions. In fact, the "mental hygiene" program was the only project inaugurated before World War I that was expanded directly into a scientific research policy agenda in the 1920s and 1930s.

In addition to providing the impetus to research and control human behavior, especially sexual behavior, the Bureau served as an agency for recruiting personnel to the Rockefeller Foundation. One of the most important recruits was Raymond Fosdick (1883–1972). In 1913, Fosdick began working as an advisor to the Foundation when he accepted an assignment on behalf of the Bureau of Social Hygiene to investigate

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230 Ibid.
231 Ibid., p. 11.
prostitution in Europe. As soon as Fosdick met Junior, the two men became inseparable. As Fosdick recalled,

My work for Mr. Rockefeller proved to be of infinite variety and fascination. I represented him on the boards of some of the companies in which he was financially interested; I developed a plan by which he could keep intimately in touch with the problem of industrial relations in the business concerns largely controlled by the Rockefellers; I served as an adviser in relation to many of his personal philanthropic projects; and more important than anything else, at least to me, I became a trustee and a member of the executive committee of the various foundations established by the Senior Rockefeller. Fosdick became Junior's proxy, acting on the younger Rockefeller's behalf in business and foundation matters. To further distance himself from direct control over the Rockefeller Foundation, Junior pegged Fosdick to serve as acting president of the Foundation from 1916 to 1917, though Secretary Jerome Greene continued to make most of the funding decisions until George E. Vincent took over as Rockefeller Foundation President during World War I.

During World War I, including the years before the United States officially declared war, Fosdick, Junior and representative from the Rockefeller Foundation and Bureau of Social Hygiene part of a collective, cooperative effort to mobilize scientific resources for the benefit of the allies. According to historian Ellis Hawley, "From the wartime experience came an enhanced vision of enlightened private orders enlisted in the

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232 "Raymond Fosdick," Rockefeller Archive Center Biography Files, RAC.
national service and working with public agencies to advance the common good." Hawley's statement certainly rings true for the Rockefeller Foundation. The Foundation played a central role in facilitating the collaboration between public and private, between foundations and universities, and between voluntary Associations and government agencies.

The ranks of "enlightened private orders" included university administrators and professors. Although involvement in World War I meant decreased enrollments and financial hardship, universities enthusiastically participated in mobilization. As historian Carol Gruber argues, "When the United States entered the war, the institutions of higher learning donated their intellectual and physical resources almost without reservation." In terms of "physical resources," universities including Columbia, Harvard, and the University of Chicago turned over engineering and laboratory facilities, and offered campus spaces for training and research. More importantly, the government drew upon the "intellectual resources" that the universities offered: scientific researchers.

World War I presented a unique situation for university scientists. Their research was being valued as never before, but war work often required them to leave the university departments that were their centers for professional development and bastions of intellectual authority. In this environment, professional associations began to outweigh university departments as the primary organizing institutions for specialized research. Leaders emerged within the professional associations who were especially

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237 Ibid., 96–97.
adept at organizing fellow researchers, dealing with foundation managers and
government officials, and negotiating on behalf of their disciplines. They were scientific
entrepreneurs, as skilled in the business of promoting their professional interests as they
were in their research fields. These scientific entrepreneurs used the professional
organizations that researchers had been cultivating for fifty years as vehicles to attract
greater interest in their work, especially from philanthropic foundation officers who
controlled the financial resources necessary to support the future development of
university science, and their efforts were responsible for much of the success that
university scientists experienced after World War I.

The organization that built the strongest ties to the foundations, the National
Academy of Sciences (NAS)-National Research Council (NRC), received millions from
the CCNY and Rockefeller Foundation during and after the war. Other professional
organizations, particularly the American Association for the Advancement of Science
(AAAS), were unable to gain access to foundations despite earnest efforts. The different
paths that the AAAS and NAS-NRC traveled when soliciting foundation support during
the war shaped university research funding for decades. J. McKeen Cattell and Edward
C. Pickering of the AAAS competed against George E. Hale of the NAS-NRC for the
attention of foundation officers. Cattell and Pickering lost. Hale's victory on behalf of
the NAS-NRC led to a relationship with foundations that spawned a new era in
philanthropy in which university science became a top funding priority.

To understand why Hale was more successful than Cattell and Pickering, it is
helpful to take a brief look at the respective histories of the American Association for the
Advancement of Science (AAAS) and the National Academy of Sciences (NAS). The
AAAS, which was founded in 1848 by a cadre of scientists led by intrepid Harvard Lawrence School professor Louis Agassiz, was the first major professional organization designed to promote the needs and interests of American scientists. The purpose of the AAAS, according to the founding members, was to extend the influence of scientists through “periodical and migratory meetings,” “to promote intercourse between those who are cultivating science in different parts of the United States,” “to give a stronger and . . . a more systematic direction to science,” and “to procure for the labours [sic] [of scientists] increased facilities and a wider usefulness.”\(^{238}\) During the early years of the AAAS, the organization was dominated by a small group of scientists that came to be known as the “Lazzaroni.” Historians have come up with various definitions of the Lazzaroni, all of which speak to the elusive nature of the group.\(^{239}\) Nathan Reingold defines the Lazzaroni as “professional physical scientists” whose “interests and range of influence extended to all of the sciences and included much of the research performed in universities and government.”\(^{240}\) By most accounts, the members of the Lazzaroni aimed to establish themselves as the standard bearers of science, the self-selected professional elite. The group’s power was certainly formidable: Lazzaroni leaders Alexander Bache, Joseph Henry, Louis Agassiz, Benjamin Peirce, and James Dana had become successive presidents of the AAAS in the 1850s and 1860s. They had also drafted the original plan for the National Academy of Sciences (NAS).


On March 5, 1863, the National Academy of Sciences (NAS) was signed into law by Congress with the promise that it would “whenever called upon by any department of the Government, investigate, examine, experiment, and report upon any subject of science or art,” a promise that was ostensibly meant to aid the Union in the Civil War.\(^{241}\) Yet, the charter stipulated that “the Academy shall receive no compensation whatever for any services to the Government of the United States,” and NAS members apparently did little to support the war effort. Therefore, the NAS was mainly symbolic in the late nineteenth century, and served primarily to bolster the reputation of its members. The form of organization was particularly telling: the Lazzaroni had handpicked fifty of their closest associates to be the inaugural NAS members. These first fifty members were appointed for perpetuity and would select their successors. The NAS was thus remarkably insular, with members known more for their participation in an exclusive group than for their professional activities on behalf of the organization. As A. Hunter Dupree attests, “The Academy’s sole function was to exist. The population inside its boundaries—a population selected from the larger scientific community—was in itself the sole symbol.”\(^{242}\) Many of the AAAS members saw the NAS as an elitist club rather than a complementary professional association. As a result, although the two organizations had numerous members in common, there had been a long history of tension between the leaders of the NAS and the AAAS by the time of World War I.

The most ardent supporter of the AAAS during World War I was Edward C. Pickering (1846–1919), an astronomer who had spent most of his professional career


promoting scientific research. In 1877, after ten years as a highly respected physics professor at MIT, Pickering had moved to Harvard to serve as the director of the Harvard College Observatory. That same year, he was elected vice president of the AAAS. In his vice presidential speech, Pickering argued that the AAAS should pursue private sources of funding to create opportunities for scientific research. He had been inspired by Rockefeller and Carnegie, as Howard Miller explains,

Pickering watched pools and trusts proliferate, marveling especially at the organizational genius of men like John D. Rockefeller and Andrew Carnegie. He saw himself doing for science what they had done for industry. "The same skill in organization, combination of existing appliances, and methodical study of detail, which in recent years has revolutionized many commercial industries," he explained, "should produce as great an advance in the physical sciences."²⁴³

Pickering’s main suggestion was that the AAAS should coordinate an institute for research in physics, chemistry, and mathematics, but the idea had fallen on deaf ears.²⁴⁴ Throughout the 1880s and 1890s, Pickering agitated for scientific research funding from private sources. He even devised a plan for an AAAS grant program. Again and again, though, he was rebuked by fellow AAAS members and potential donors.

Pickering’s plan failed for several reasons. First, other leading scientists from the AAAS saw Pickering as rather dictatorial in terms of controlling endowment funds. Second, many of Pickering’s fellow scientists were wary of tying their fortunes too closely together. For these scientists, participation in a formal grants program with

shared obligations to donors was a threat to their autonomy as researchers. As Seth C. Chandler, the editor of the *Astronomical Journal* in the 1890s, explained the general sentiments among scientists about Pickering's aspirations: "Only Pickering himself is to blame, that his claims and desires that everyone should unselfishly cooperate with him & place him in charge of everything as the wise & good dispenser of all things—are met with natural distrust." Finally, Pickering was unable to communicate effectively about the importance of his work with potential donors. Wealthy Americans believed in science but had some difficulty understanding the needs of scientific researchers in the late nineteenth century, especially in fields as far removed from daily life as astronomy. As Howard Miller notes, "The pursuit of astrophysical truths might inspire a dedicated investigator, but it was too esoteric for the layman." Nonetheless, Pickering remained dedicated to surmounting these obstacles, and he continued to try new tactics to achieve his funding goals.

Pickering had some success as a promoter of his own Harvard Observatory during the late 1880s thanks to Catherine Wolfe Bruce, who financed a 24-inch telescope and several small grants for visiting researchers. In 1902, Pickering convinced Henry H. Rogers of Standard Oil to donate $20,000 to the Observatory. These successes rejuvenated Pickering's campaign to create a grants program for the AAAS. He put together a pamphlet entitled "Endowment of Research" in 1903, which he distributed to representatives from major professional science organizations. Since the Carnegie Institution of Washington (CIW) had opened within the past year, Pickering argued that the AAAS should create a partnership with the new institute, but CIW officers showed no

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246 Howard Miller, *Dollars for Research*, 114.  
interest. For the next ten years, Pickering repeatedly submitted applications to foundations on behalf of the AAAS grants program, all to no avail.

In December 1913, Pickering changed strategies. He contacted Columbia psychologist J. McKeen Cattell (1860–1944), and together they created the Committee of One Hundred to Promote Scientific Research, an arm of the AAAS devoted specifically to fundraising.\(^{248}\) Cattell was an avid promoter of the AAAS who disdained the National Academy of Sciences (NAS) as “an exclusive social club” despite the fact that he was a member.\(^{249}\) In the influential journals that he owned and edited, including *Scientific Monthly, Science*, and *School and Society*, Cattell had criticized elite control over academic life. In his writings, he had frequently lamented that “he who holds the purse strings holds the reins of power.”\(^{250}\) Yet, Cattell put his protestations against moneyed influence in academe aside to join Pickering in the pursuit of foundation support. Of course, he likely realized that his anti-elite screeds might hinder his and Pickering’s attempts to garner foundation interest in the AAAS. Initially, Cattell let Pickering do the communicating with foundation officers.

On May 15, 1915, Pickering wrote Jerome Greene, secretary and operating officer of the Rockefeller Foundation, to make the case for a grant to the Committee of One Hundred of the AAAS. In the letter, he asserted that the United States was far behind Europe in terms of research because funding priorities were out of step with the needs of university scientists:

> This seems to me a disgraceful condition for the richest country in the world, which in colleges spends a hundred millions a year in teaching, and but little in

\(^{248}\) Gerald Jonas, *Circuit Riders*, 86.
\(^{250}\) J. McKeen Cattell, *University Control* (New York: The Science Press, 1913), 36.
extending human knowledge. For the latter, we are dependent almost entirely upon Mr. Rockefeller and Mr. Carnegie. The latter pointed out that one should find the particular man and aid him. The Carnegie Institution has abandoned this policy. Unless this is done by Mr. Rockefeller's gifts, there remain, therefore, only a few small research funds.\textsuperscript{251}

Pickering touched on the issues that had been effective in capturing the attention of wealthy donors: competition with Europe, the advancement of knowledge, and the relative weakness of research compared to teaching within institutions of higher education. These arguments certainly appealed to Greene, but he explained that the Foundation was too busy with war relief work to get involved with the AAAS grants program. Greene also intimated that the grant-in-aid idea was unappealing, an opinion that was probably formed in response to the problems that the CIW and RIMR had experienced administering grants programs:

\begin{quote}
I am free to say that the existing foundations leave much to be desired in respect to the promotion of research, and yet I confess that I see grave objections to the plan of doling out money for research under any system of distribution that has hitherto been devised.\textsuperscript{252}
\end{quote}

To Pickering, Greene's reply left room for hope. Over the next year, Pickering wrote Green regularly, probing the Rockefeller Foundation secretary for information on how to make the AAAS applications more compelling. Nothing worked.

\textsuperscript{251} Edward Pickering to Jerome Greene, May 21, 1915, folder 1, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{252} Greene to Pickering, June 14, 1915, folder 1, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
On March 25, 1916, Cattell took over from Pickering and appealed to Greene yet again for a Committee of One Hundred grant. In his letter, Cattell reflected on the changing relationship between university scientists and philanthropic foundations. His argument deserves a lengthy quotation because it was such a clever exposition on the critical role that professional scientific researchers play in society and on the need for more involvement by philanthropists in supporting the work of scientists:

Scientific research is for the benefit of society as a whole and cannot be sold to individuals. It must be paid for by society . . . Until the recent establishment of the Carnegie Institution and the Rockefeller Institute, scientific work was done mainly in universities or by amates of independent means. In this country, we have no aristocratic class with inherited wealth, some of whom . . . might give not only their time but pay also for the needed assistance, apparatus, etc., and our universities have grown so rapidly that the time of the professor is mainly taken up with teaching . . . Science and its applications have made democracy possible . . . In return this country should take the lead in scientific research . . . The value of science to the world has been so immeasurable that it scarcely seems possible that money spent on scientific research can be ill-invested . . . The only question is how scientific research can be advanced most effectively. Research institutions, including universities where research is encouraged, are the most important means. But they require a relatively large expenditure . . . It seems probable that cooperation between scientific men and the trustees of endowed
institutions might be undertaken with advantage to both and in the interest of orderly social development.\textsuperscript{253}

Using broad strokes, Cattell captured the essence of the trends and issues that defined university scientific research in the first decade of the twentieth century: the promise that scientific research held for the future of social progress, the unfulfilled hope that scientists felt during the early years of the CIW and RIMR, the need to overcome the tension between teaching and research within the university, and the financial investment required for the United States to compete internationally in scientific research. His message encapsulated all of the ideas that justified further foundation intervention in the sciences, and the very arguments that he presented in the grant application letter were echoed throughout foundation policies after World War I. At the time, though, Cattell’s grant application was rejected.

Many of Cattell’s and Pickering’s contentions about the importance of supporting American scientific research clearly resonated with Rockefeller Foundation officers. Greene alluded to the Committee of One Hundred applications and reprised his discussions with Cattell and Pickering when he submitted a policy statement to his fellow Foundation officers in May 1916:

The useful services of the Foundation in the future . . . are likely to be divisible into two main divisions, the discovery of new knowledge bearing on human welfare, and the dissemination of this knowledge by various educational methods. . . . Scientific truth is not only worthy of search for its own sake, but it almost

\textsuperscript{253} J. McKeen Cattell to Greene, March 25, 1916, folder 1, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
certain to have sooner or later practical applications to the use and enjoyment of man.\textsuperscript{254}

This statement established that the Rockefeller Foundation was going to invest in scientific research at some point, particularly in areas “bearing on human welfare,” but Greene offered nothing specific.

When George Vincent took over as president of the Rockefeller Foundation in 1917, he immediately embraced the principles that Greene enunciated in the May 1916 statement. Vincent, who had a Ph.D. in sociology from the University of Chicago, had been lured away from his position as president of the University of Minnesota by Fosdick. Prior to serving as president of Minnesota, Vincent had been the dean of the Junior College (1900–1904), professor of sociology (1904–1907), and the dean of arts and sciences (1907-1911) at the University of Chicago.\textsuperscript{255} With an academic at the helm, the Rockefeller Foundation cooperated with universities even more. Still, Vincent was as unimpressed as his predecessor with the minor research grants program that Pickering and Cattell had outlined in their applications for the Committee of One Hundred. Ironically, the principles that Pickering and Cattell articulated were embraced by the Rockefeller Foundation even as the AAAS applications foundered. It finally became clear to the Committee of One Hundred members that philanthropic foundations were indifferent to the AAAS. Cattell and Pickering resigned from the AAAS in November 1917 and May 1918, respectively, and the Committee of One Hundred disintegrated.\textsuperscript{256}

\textsuperscript{254} Greene report to Officers’ Meeting re: Committee of 100 on Scientific Research of the AAAS, May 24, 1916, folder 6, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{255} Robert Kohler, \textit{Partners in Science}, 51.
Vincent and his fellow Rockefeller Foundation officers lost interest in the AAAS and its Committee of One Hundred partly because they were coming under the influence of George E. Hale (1868–1938), a singularly compelling advocate for scientific research and the National Academy of Sciences (NAS)-National Research Council (NRC) who was able to engage the interest of foundation officers much more effectively than Cattell or Pickering. Hale had begun trying to reform the NAS into an organization "favorable alike to the development of research and the public application of science" immediately after he was selected as a member in 1902.257 He was certainly the right man for the job. By the first decade of the twentieth century, Hale had become the most adept scientific entrepreneur in the country. In the 1880s, Hale had secured funding from his father to build a breakthrough invention, the spectroheliograph, an ingenious device for recording images of the sun. Soon after he had completed the spectroheliograph project, Hale had negotiated a deal with industrialist Charles Yerkes to build the Yerkes Observatory at the University of Chicago.258 In 1902, the same year that he had begun his campaign to overhaul the NAS, Hale had convinced the officers of the Carnegie Institution of Washington (CIW) to construct the Mt. Wilson Observatory in Pasadena, California.

By the 1910s, Hale had begun channeling his energy toward two institutions: the NAS and Throop Polytechnic Institute (later California Institute of Technology). His efforts yielded remarkable results.259 Unlike Pickering and Cattell, who both had personality conflicts with their fellow scientists, Hale was immensely popular among his colleagues. NAS members responded with enthusiasm to Hale’s reform proposals, which

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259 See Chapter Five of this dissertation for more details about Hale’s efforts on behalf of the California Institute of Technology.
were detailed and comprehensive. At a November 1913 meeting of the NAS, Hale argued that the moment was right for a full-fledged realization of the reform proposals that he had begun advocating a decade earlier. He presented an agenda that included establishing high standards of scholarship, providing scientific leadership for the government, stimulating innovative research projects, securing financial support for laboratories and for publications of research results, and facilitating interdisciplinary cooperation among members.\(^{260}\) In contrast to the rather limited grants-in-aid program that Cattell and Pickering had presented on behalf of the AAAS, Hale had a vision for organizing scientific research that went far beyond acquiring small allotments to subsidize individual projects. To finance this reform agenda, Hale had turned to Carnegie, who had previously supported the Mt. Wilson project for the CIW. His initial application to CCNY for $750,000 was rejected, but Hale bided his time, waiting for a chance to demonstrate the promise of his ambitious goals for the NAS. The outbreak of World War I provided a golden opportunity.

In April 1916, Hale and other NAS leaders met with President Woodrow Wilson to offer their assistance for war mobilization. When Wilson appeared interested, Hale joined with Simon Flexner (director of the RIMR), Robert A. Millikan (renowned professor of astronomy at the University of Chicago), and Arthur Amos Noyes (prominent physical chemist from MIT) to launch the Committee on the Organization of the Scientific Resources of the Country for National Service, which was to “bring into cooperation existing governmental, educational, industrial and other research organizations with the object of encouraging investigation of natural phenomena” in

addition to providing scientific research for national defense. This Committee formed the basis of the National Research Council (NRC), which was signed into law by President Wilson in July 1916. In September 1916, the NAS-NRC elected Hale as the chairman and formed an executive committee that included representatives from universities, research institutes, foundations, and industry. The initial members of this executive committee became some of the most influential intermediaries between university scientists and philanthropic foundations, forming a network that channeled foundation resources toward NRC interests for years to come. Committee members included Hale, Millikan, and Noyes, as well as:

William H. Welch, president of the National Academy of Sciences (NAS), RIMR trustee, CIW trustee and advisor, and dean of the medical school at Johns Hopkins;

Charles D. Walcott, Secretary of the Smithsonian Institution and CIW trustee;

John J. Carty, Chief Engineer for AT&T, CIW trustee, and Carnegie Corporation of New York (CCNY) trustee (beginning in 1923);

E. G. Conklin, Professor of Zoology at Princeton University; and

Raymond Pearl, Professor of Biology at Johns Hopkins University. Most of the committee members (75%) were university scientists, with Carty as the sole industry scientist and Walcott the lone government scientist. The university scientists on the committee were amply rewarded for their close association with foundation officers and trustees during the war: Hale, Millikan, Noyes, Conklin, and Pearl each received substantial foundation support for their research in the 1920s and 1930s.

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262 Ibid., 214.
Hale, Millikan and Noyes had always planned to use the National Research Council (NRC) as an organization to promote university research after the war, and they knew that the best way to secure future support was to prove their usefulness for the war effort. On February 4, 1917, the day after President Woodrow Wilson declared war, Hale contacted the Council of National Defense to offer the services of the NRC. With Wilson’s approval, the NRC moved into the Council of National Defense building, using $74,700 in donations from the Rockefeller Foundation and Carnegie Corporation of New York to support a variety of scientific projects to aid the war effort.\textsuperscript{263} Through the wartime projects coordinated by the NRC, university scientists proved the value of their research. Physicists guided the development of sonar technology, meteorologists informed gunners and airmen about wind patterns, astronomers studied the trajectories of projectiles to increase artillery range and improved navigation methods for airplanes, and psychologists tested recruits to determine optimal personnel placement.\textsuperscript{264} As they cooperated with NRC representatives and witnessed the contributions that researchers made to the war effort, foundation officers’ perception of university researchers changed. In the words of Robert Kohler, “for the first time, philanthropists perceived scientists as an activist, public-spirited elite, sharing many of their own goals.”\textsuperscript{265} Foundation officers’ greater appreciation for the scientific entrepreneurs that ran the NRC helped them overcome their earlier discomfort with providing university scientists with direct support.

\textsuperscript{263} Ibid., 221–226.  
\textsuperscript{264} Robert Yerkes, ed. The New World of Science: Its Development during the War (New York: The Century Company, 1920).  
In 1918, Hale secured an executive order from President Wilson to establish the National Research Council (NRC) as a separate research organization from the National Academy of Sciences (NAS). When Wilson approved the order on May 10, 1918, “the basic purposes of the Council—the stimulation of pure and applied research, the promotion of cooperation at home and abroad—had the permanent imprimatur of the presidential office.”²⁶⁶ These “basic purposes” had been articulated by Hale as early as 1902, and he was finally seeing them come to fruition. On May 20, 1918, Carnegie Corporation of New York (CCNY) officers registered their endorsement of the newly independent NRC with a $100,000 grant.²⁶⁷ This was the first major philanthropic foundation contribution to the NRC. It was only the beginning.

In the spring of 1919, foundations “initiated a new era” in the support of scientific research through major grants to the NRC. At the time, Elihu Root, who had served as the secretary of war under McKinley and Roosevelt (1899–1904), secretary of state under Roosevelt (1905–1909) and U.S. senator from New York (1909–1915), was the acting president of Carnegie Corporation of New York (CCNY). In March 1919, Root put together a $5,000,000 (equivalent to $61,500,000 in 2005) endowment grant for the NRC that included a fund to construct a permanent building for the organization.²⁶⁸ Root worked closely with James R. Angell, then president of the NRC, and Hale to draft a plan that would guarantee long-term solvency for the organization. As a mugwump and elite reformer, Root believed in the promise of science and the importance of keeping research free from government interference. He had already been pivotal in securing CCNY

²⁶⁶ Daniel Kevles, “George Ellery Hale, the First World War, and the Advancement of Science in America,” 434.
grants for the NRC during the war. To Root, the NRC presented a perfect opportunity for the CCNY to contribute to national progress in peacetime, a belief that Angell and Hale strongly encouraged. The negotiation of the $5,000,000 endowment grant not only solidified the connection between CCNY and the NRC, but it also created a bond between Root and Angell. The following year, Root recruited Angell to take over as president of CCNY.

While the CCNY negotiations were taking place, Hale and Millikan also worked with Rockefeller Foundation president George Vincent to design a NRC postdoctoral fellowship program. According to the plan, the Rockefeller Foundation would provide the fellowship funds, while NRC officers would select deserving candidates and determine grant amounts. On April 9, 1919, the Rockefeller Foundation pledged $50,000 for the one year plus a $500,000 grant for the next five years to support a Research Fellowship Committee led by Hale, Millikan, and Noyes, who became known as “the triumvirate” due to their power as a group to influence foundation officers. The adoption of the fellowship program was the final step in establishing the NRC as “the focal point in the new conception of organized science . . . independent of federal support or supervision.” Through the NRC, the foundations and universities became increasingly intertwined, as “the same group of individuals encountered one another, in slightly different combinations, in committees of the NRC, on the boards of the Carnegie and Rockefeller philanthropies, and as trustees of recipient institutions.” The interlocking directorate was incorporating more scientists into its ranks.

270 Rexmond C. Cochrane, The National Academy of Sciences, 249.
271 Roger Geiger, To Advance Knowledge, 100.
The Rockefeller Foundation and Carnegie Corporation of New York (CCNY) grants were the decisive endorsements of the National Research Council (NRC) as the premier professional scientific organization. A bitter J. McKeen Cattell, who had been fired from Columbia University by Nicholas Murray Butler during the war because he had questioned draft policy, derided the NRC as the "Rockefeller-Carnegie Research Council (R₂C₂)," but most scientists were quite pleased with the new arrangement.²⁷² Hale was understandably thrilled. In the first postwar NRC publication, *The New World of Science*, Hale described what he saw as the next stage for scientific research in the United States:

One of the most striking results of the war is the emphasis it has laid on the national importance of scientific research . . . If scientific methods and the aid of scientific research were needed in overcoming the menace of the enemy they will be no less urgently needed during the turmoil of reconstruction and the future competitions of peace.²⁷³

This idealistic vision of science became the prime motivation for foundation contributions to the NRC and to university research in the 1920s and 1930s.

After the war, Carnegie and Rockefeller philanthropic foundations took a variety of different approaches to financing university research, but support for the NRC never wavered. In addition to the endowment grant, CCNY contributed operating expense funds to the NRC totaling $100,000 in 1920, $170,000 in 1921, and $185,000 in 1922.²⁷⁴ The Rockefeller Foundation began with the fellowship program in physical sciences in

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1919, to which it added $325,000 in 1923 for biological sciences, including anthropology and psychology, and a supplement of $625,000 in 1924 for physical sciences. The Foundation also contributed sizeable grants to NRC committee projects, especially the Committee for Research in Problems of Sex (see Figure 2 on p. 127 of this chapter). Between the establishment of the NRC and the onset of World War II, foundations channeled over $12,000,000 to scientists through the NRC, with 97.5% of those contributions coming from Carnegie and Rockefeller philanthropic foundations (see Figure 1).

**Figure 1:** Foundation Contributions to the NRC, 1919–1941

In the early 1920s, Carnegie and Rockefeller foundations began to seek additional opportunities to finance university science beyond the NRC, with varying degrees of success. When Andrew Carnegie died in 1919, he left CCNY without any clear policies

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275 ibid., 257.
or administrative guidelines. Trustees and Carnegie foundation leaders Elihu Root and Henry S. Pritchett took responsibility for reorganizing CCNY into a closer approximation of "scientific" philanthropy.\textsuperscript{278} Their first move was to recruit James R. Angell (1869–1949) to become president of CCNY. Angell was a well-established leader within the professional scientific community. Before World War I, he had served as professor of psychology, dean of the Faculty of Arts, Literature, and Science, and acting president of the University of Chicago.\textsuperscript{279} As noted earlier, when Root and Pritchett had contacted him about CCNY in late 1919, Angell was serving as the chairman of the National Research Council. With his background as a scientist, university administrator, and NRC leader, Angell seemed like the ideal candidate to redefine the CCNY presidency. When he arrived at CCNY in 1920, Angell "boldly asserted that the Carnegie Corporation's charter was a mandate for promoting higher education," and he promised to channel funds toward graduate programs in the sciences and the arts.\textsuperscript{280} Angell was unable to create a lasting program for university science, though, because he left in 1921 to become the president of Yale.

Angell's departure left CCNY without a steering direction once again. For the next two years, Pritchett stepped in as acting president of CCNY while the trustees cast about for a strong leader who would implement Carnegie's directive to "promote the advancement and diffusion of knowledge and understanding." In 1923, CCNY trustees finally located the individual who would become the dominant figure in the foundation for the next twenty years: Frederick P. Keppel (1875–1943). Keppel arrived at CCNY

\textsuperscript{280} Robert Kohler, \textit{Partners in Science}, 64.
via Columbia University, where he had served as assistant secretary (1900–1902),
secretary (1902–1910), and dean of the college (1910–1917). During World War I,
Keppel had worked closely with Rockefeller advisor and trustee Raymond Fosdick as co-
director of the Commission on Training Camp Activities, an organization with close ties
to the Bureau of Social Hygiene. The Commission had offered recruits “healthy”
recreational and cultural programs as ostensible replacements for “dangerous” pastimes
like visiting prostitutes and carousing in bars.\(^281\) Keppel’s wartime activities had
inaugurated him into the philanthropy interlocking directorate. By the time he was
selected as president of CCNY in 1923, Keppel had served on the boards of several
prominent voluntary associations, including the American Red Cross, the International
Chamber of Commerce, and the Russell Sage Foundation.

Although Keppel maintained a formal allegiance to “scientific” philanthropy, his
approach to governing the CCNY was hardly a model of “rational” benevolence. He
gave grants to cultural and educational institutions that he deemed worthy, based on a
calculus known only to him. As Ellen Condliffe Lagemann contends,

Grant-making during [Keppel’s] long presidency was, at least technically,
organized into programs, although it was actually directed more by hunch,
coincidence, opportunity, friendship, and a wish to help than by clear, specific,
consistently applied “scientific” goals or principles. As in the early years, too,
awards tended to be modest, usually most significant at the local rather than the
national level, and relatively diverse in range of recipients.\(^282\)

\(^{282}\) Ibid., 7.
Since Keppel ran CCNY with almost complete authority from 1923 to 1942, his predilections were effectively foundation policy.

Due to Keppel's eclectic style of foundation leadership, the direct impact of Carnegie Corporation of New York (CCNY) on the development of university science in the 1920s and 1930s is difficult to assess. CCNY science grants were rather small in comparison to Rockefeller Foundation grants, with annual allotments rarely exceeding $2000, and they were usually administered through the Carnegie Institution of Washington (CIW). Keppel wanted to support university research, but he was uncomfortable making decisions about science. To Keppel, it was easiest to turn as much control as possible over to the experts who ran professional associations, as he explained in a draft of his 1928 president's report that he sent to CCNY trustees:

The trustees of an educational endowment today face a very different situation from that of seventeen years ago, when the Corporation was founded. Within this short period, the following national organizations have come into existence: the American Council of Learned Societies, National Research Council, Social Science Research Council... These, with others of the same character, stand ready either to propose to the foundations projects for which they believe financial support would be justified and timely, or to comment on the desirability of such projects... The degree to which these factors are influencing the policies of the Corporation is shown by the fact that of the first $2,000,000 voted from the income of the Carnegie Corporation from the time of its establishment in 1911, 100% was voted in response to direct applications from communities or institutions, while the $2,000,000 voted most recently from that portion of the
income of the Corporation available in the United States may be divided as follows: 32%, or $641,000, was made directly to the institutions concerned; action in 68%, or $1,359,000, was based upon the recommendation of some national organization.\(^{283}\)

As this report illustrates, Keppel depended increasingly on professional academic organizations to administer funds for him. For scientific research, almost all funding went to Carnegie Institution of Washington (CIW) laboratories, including the Eugenics Record Office. Otherwise, throughout the 1920s and 1930s, Keppel made grant decisions on an apparently ad hoc basis.\(^{284}\)

In contrast to the stability that Keppel provided at CCNY, the leadership and policies of the Rockefeller philanthropies changed frequently throughout the 1920s and 1930s, and each change had direct ramifications for university scientific research. The only productive way to discuss the Rockefeller philanthropies is as a network rather than discrete organizations. Theresa Richardson and Donald Fisher capture the essence of the relationship among the Rockefeller philanthropies:

The philanthropic boards associated with the Rockefeller family have been likened to a fleet of ships with the Rockefeller Foundation as the lead. The analogy is not misleading in that it captures the idea that the various boards are interrelated even though they are separately incorporated and legally unique. The analogy further points to the characteristic that a fleet is composed of separate

\(^{283}\) Draft of the report of the president in 1928, pp. 9–12, Frederick Keppel folder 1, box 216, Nicholas Murray Butler Papers, Columbia Archives.

vessels with individual functions and mandates, which nonetheless broadly operate with a common purpose.\textsuperscript{285}

The Rockefeller boards operated as distinct entities while pursuing the same goals. Most importantly, the goal of improving mankind through the advancement of knowledge became central to all of the major Rockefeller boards in the interwar period and, over time, scientific research became a top priority. The officers who managed the three main Rockefeller boards—the General Education Board (GEB), the Laura Spelman Rockefeller Memorial (LSRM), the Rockefeller Foundation—worked together to create policies that channeled increasing amounts of funds to university scientists.

The first Rockefeller officer to put university scientific research front and center was Wickliffe Rose (1862–1931), who became the president of the GEB in 1923. Rose was a philosopher and teacher with a long history of working in philanthropy as a trustee for the Peabody Fund, the Slater Fund, the General Education Board (GEB) and the Rockefeller Foundation. As soon as he took the helm of the GEB, Rose began to argue that the goal of improving mankind could best be achieved by funding university scientific research. As Rockefeller Foundation officers described Rose's philosophy:

Dr. Rose brought to his new posts a profound conviction that human progress in the long run is dependent upon the advancement of knowledge, and that the advancement of knowledge can best be furthered by developing the natural sciences.\textsuperscript{286}


\textsuperscript{286} “Report of Committee on Appraisal and Plan,” December 11, 1934, folder 170, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
Rose privileged the "advancement of knowledge" over all other GEB interests. His decision to emphasize research rather than teaching at an educational foundation represented a significant shift in policy. It was becoming clear that Rockefeller officers in general had overcome their biases against direct support for university research. The conflict between teaching and research had finally been set aside.

Rose believed in "making the peaks higher" by strengthening elite institutions to serve as exemplars and centers of innovation, which led him to concentrate on a few select universities as he executed his plan to fund natural science research. Under Rose's direction in the 1920s, the GEB contributed over a million dollars each to five universities to support research in the natural sciences: University of Chicago ($2,700,000), California Institute of Technology ($2,550,000 for physics and chemistry), Princeton University ($2,000,000), Cornell University ($1,500,000), and Harvard University ($1,500,000). These substantial allotments enabled these universities to construct and equip laboratories, and also supplied enough endowment funds to help the laboratories attract researchers. The GEB grants that Rose administered in the 1920s, coupled with his philosophical commitment to the promotion of natural science, laid the groundwork for future Rockefeller policy initiatives on behalf of university research.

As Rose transformed the GEB into an agency for promoting natural science research, psychologist Beardsley Ruml (1894–1960) was turning the Laura Spelman Rockefeller Memorial (LSRM) into an organization dedicated to the support of social science research. The LSRM had been established as a social welfare agency in 1918 to honor the memory of Rockefeller's wife, and it had initially funded projects like boys'
and girls clubs or the YMCA. The purpose of the LSRM changed dramatically when Rumal took over in 1922. Like most foundation officers of the 1920s, Rumal was already part of the foundation interlocking directorate before he assumed his post at the Laura Spelman Rockefeller Memorial (LSRM). Rumal had been brought into the foundation fold by his advisor at the University of Chicago, James R. Angell.\textsuperscript{288} During World War I, Angell had recruited Rumal to work on the NRC Committee for the Classification of Personnel. When Angell moved on to become president of CCNY, he brought Rumal with him as an assistant. Rumal had briefly been without a post when Angell left CCNY to become president of Yale in 1921, but he was soon selected, on Angell’s recommendation, to work directly for John D. Rockefeller, Jr.\textsuperscript{289} Later in 1921, Angell began to correspond with Raymond Fosdick to suggest that Rumal would be a perfect candidate for the directorship of the Laura Spelman Rockefeller Memorial (LSRM). Even though there were several dozen candidates vying for the LSRM post, Fosdick and Junior were sufficiently impressed with Rumal to push through his appointment as director.\textsuperscript{290}

As a psychologist, Rumal had a deep personal commitment to professional social science research that he translated into LSRM policy. Rumal explained the priority that social science research would have during his directorship to LSRM trustees:

The Memorial’s interest in social science is based on a belief that an important contribution to public welfare will come from a development of those sciences which deal with social forces—economics, sociology, political science, and the

\textsuperscript{288} Stanley Coben, “American Foundations as Patrons of Science: The Commitment to Individual Research,” 236.


\textsuperscript{290} Martin and Joan Bulmer, “Philanthropy and Social Science in the 1920s,” 354.
related subjects, psychology, anthropology and history. The increasing complexity of human affairs, due to the progress in physical science and mechanical inventiveness, has made it imperative that substantial advance be made in those sciences which are commonly termed “social.”

Ruml based his arguments on the promise that professional social scientists navigate the “increasing complexity of human affairs.” He reiterated assertions that had legitimized the development of professional social science around the turn of the century: public welfare and human progress would be advanced through specialized investigation into the component parts of the social machine. By the time Ruml made his statement to the trustees, the professional social sciences were well-established as disciplines within universities, but they were not well-funded. Ruml endeavored to rectify the funding problem.

Ruml’s efforts at the Laura Spelman Rockefeller Memorial (LSRM) were closely linked to the development of the Social Science Research Council (SSRC), an organization founded by political scientist Charles E. Merriam in 1923. The SSRC was modeled on the NRC and, like its predecessor, it served as an intermediary between foundations and university scientists. In the 1920s, the SSRC was effectively an extension of the LSRM. Between 1923 and 1930, the LSRM contributed over $4,000,000 to the SSRC, more than 93% of the organization’s total income. Beyond the SSRC, the LSRM also contributed millions directly to universities to finance social science research centers. Ruml played a significant role in defining and promoting the

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291 Docket of the Board of Trustees Meeting, February 26, 1924, folder 677, box 63, Series III, Laura Spelman Rockefeller Memorial Archives, RAC.
human engineering effort, not only through general support to social science research, but also through direct involvement in the building of psychology programs. His intervention on behalf of his mentor, Angell, and his close friend Robert Yerkes at Yale University was particularly significant because it reveals the critical importance of both the interlocking directorate and human engineering ideals in the formation and application of foundation policies in the 1920s. This flagship program in behavioral sciences at Yale University will be discussed in detail in Chapter Three. Ruml was also relevant because he helped to make the social sciences a consistent priority for Rockefeller philanthropies, including the Rockefeller Foundation, which eventually absorbed the LSRM.

Ruml worked closely with officers from the Rockefeller Foundation as they designed the Division of Studies in 1924 to finance research in the field of “human biology.” “Human biology” was a loosely defined concept that combined eugenics, mental hygiene, psychology, anthropology and a variety of biological science specialties. The architects of the “human biology” program, especially its director, Edwin Embree, were devoted to human engineering through university research. Embree’s Division of Studies program was an attempt to realize goals set by earlier Rockefeller initiatives. The “human biology” program was inspired by the Bureau of Social Hygiene, the social science program inaugurated by Ruml at the Laura Spelman Rockefeller Memorial, and the university grants program that Wickliffe Rose had launched at the GEB. More than Ruml, Rose was the greatest source of inspiration for Embree. Embree frequently quoted a 1923 memo by Rose that stated:
Science is the method of knowledge. It is the key to such dominion as man may ever exercise over his physical environment. Appreciation of its spirit and technique, moreover, determines the mental attitude of a people, affects the entire system of education, and carries with it the shaping of a civilization.\(^{293}\)

Rose’s memo encapsulates the Rockefeller officers’ optimism about the transformative possibilities inherent in scientific research. By the mid-1920s, beginning with Embree, Rockefeller Foundation officers had embraced Rose’s assertions that research in the natural sciences would yield an “increase in knowledge upon which human welfare depends” and “in cultivating [scientific research], universities make, therefore, a notable contribution not only to knowledge, as such, but to the art of living.”\(^{294}\) Specifically, the idea that research into human biology and behavior would improve society had become a significant policy objective at the Rockefeller Foundation by the late 1920s.

Many of the trends that defined foundation intervention in university scientific research in the 1920s and 1930s were the consequence of the trial-and-error period in philanthropy before and during World War I. These trends are critical to understanding how foundations influenced knowledge production in the United States between the world wars. First, the abandonment of grants-in-aid programs through the research institutes, in which individual scientists received small annual allotments to subsidize projects, in favor of major university grant initiatives, in which university departments received large grants over a three to ten year period, was based on administrative difficulties that the officers of the RIMR and CIW encountered in the first decade of the

\(^{293}\) “Report of Committee on Appraisal and Plan,” December 11, 1934, folder 170, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.

\(^{294}\) “The Natural Sciences,” April 11, 1933, folder 6, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
twentieth century. The move away from small grants-in-aid was initially lamented by university scientists, but it ultimately worked to their benefit. Rather than being constrained by small grants that covered only minor expenses, scientists were able to use the sizeable foundation allotments of the 1920s and 1930s to build world-class research programs. Second, the decision to focus on research in universities rather than exclusively on separate institutes reflected the growing realization on the part of foundation officers that universities were destined to be the centers of scientific innovation in the United States. The wartime experience of cooperating with university scientists through the NRC convinced foundation officers that, although professors were preoccupied with teaching obligations, they were also perfectly capable of organizing and running efficient, successful research programs within the universities. Third, the mental and social hygiene movements that captured the imagination of foundation officers before and during World War I inspired the long-term human engineering project that was to unfold in the 1920s and 1930s. Through their involvement in the Bureau of Social Hygiene and related movements, foundation officers tied the control of sexuality and behavior to the reduction or elimination of "dysfunction" and to the improvement of mankind. The desire to investigate and control behavior, an agenda that appears lamentable and misguided today, led directly to foundation programs that supported valuable university research in human behavior and biology. Finally, the emphasis on elite universities that began with the General Education Board (GEB) and Carnegie Foundation Advancement of Teaching (CFAT) continued in the 1920s and 1930s, as foundation officers concentrated almost exclusively on "making the peaks higher" at well-established institutions. The same universities that received the bulk of educational
foundation grants before World War I received the majority of research grants in the interwar period (see Figure 2).

**Figure 2:** Major Rockefeller Grants to University Scientific Research

Foundation officers' overt preference for elite institutions remained highly controversial. In his analysis of philanthropic foundations, F. Emerson Andrews explains the dilemma:

In general a few large universities do receive the bulk of foundation grants. This is so because adequate research facilities and the ablest personnel are concentrated in these places. . . . This does have an unfortunate cumulative effect. Not only do foundations turn first to the larger centers where their search is simplified, but as a result, in smaller colleges research opportunities are not being developed, and many of their abler professors gravitate to the larger schools.  

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As Andrews notes, early foundation intervention in the support of elite universities continued to pay dividends for those institutions in the ensuing decades. Needless to say, the foundations helped to widen the gap between elite universities and left many scientists feeling slighted. The example of J. McKeen Cattell and the Committee of One Hundred of the AAAS is a perfect illustration of the resentment that scientists felt toward the foundation officers who rejected their applications.

In the eyes of those who were unable to secure support, the foundation grant application process was more like a marketing campaign than a fair analysis of quality research. Professor Harold Laski, who wrote critiques of foundations in a collection of essays in the 1920s, claimed that philanthropy was having a deleterious effect on academia. Laski argued that the reliance on grant money gave “the foundations a dominating control over university life which they quite emphatically ought not to have,” which resulted in “the personnel of the university, in a word, [being] dominated by the ‘executive’ type of professor, who is active in putting his goods into the shop-window.”297 Laski touched a nerve with his essays, which were widely read and distributed among members of the academic community. His contentions reformulated the charges leveled at the Rockefeller and Carnegie philanthropies during the Commission on Industrial Relations hearings: foundations were undemocratic organizations run by elitists who made capricious decisions.

Perhaps Laski was right, perhaps not. The only way to judge the foundations is to judge the consequences of their policy decisions and to ponder the alternative possibilities. What if the foundations had shared their wealth with a wider spectrum of

colleges and universities? Would the weaker schools have improved without the incentives imposed by the GEB and CFAT? It is impossible to tell. Similar questions could be asked about scientific research. While it is impracticable to try to imagine what might have been, it is feasible to assess the results of foundation policies by examining projects and researchers who received substantial allotments over long periods of time. It is also worthwhile to evaluate the success of heavily funded programs in terms of their intended policy objectives. In other words, rather than asking about what could have happened if circumstances were different, I will ask: How did foundation officials translate their mandates, particularly the effort to improve mankind, into policies for the support of university research? To what extent did foundation policies and grant decisions affect the types of research questions that university scientists pursued and/or the techniques they used to pursue them?

The answer, I believe, is that foundation officers explicitly connected the improvement of mankind to university research in human behavior and biology through a chain of reasoning that incorporated early interest in eugenics, a preoccupation with fields related to mental and social hygiene, and a continuous fascination with and faith in the transformative power of science. By the end of World War I, professional scientists had successfully established their intellectual legitimacy with such authority that philanthropic foundation officers were willing, even eager, to turn the fate of mankind over to university researchers. At the same time, the promise of university research to improve mankind gave foundation officers renewed confidence that they could shape the future of society if they administered the ample resources at their disposal wisely. As the subsequent chapters will reveal, the authority vested in professional scientists at elite
universities mixed with the funds provided by empowered foundation officers was a potent combination that transformed knowledge production in the United States, for better or for worse.
Intrinsic to the new liberal relations of natural and social disciplines was the project of human engineering—that is, the project of design and management of human material for efficient, rational functioning in a scientifically ordered society. Animals played an important role in this project. On the one hand, they were plastic raw material of knowledge, subject to exact laboratory discipline. . . . On the other hand, animals have continued to have a special status as natural objects that can show people their origin, and therefore their pre-rational, pre-management, pre-cultural essence. – Donna Haraway, *Simians, Cyborgs, and Women*298

**Chapter Three**

**Improving Mankind by Modifying Behavior: Yale University Behavioral Sciences**

After World War I, psychologists became enthusiastic participants in the nascent human engineering effort launched by philanthropic foundations, especially the Rockefeller boards (General Education Board, Rockefeller Foundation, and Laura Spelman Rockefeller Memorial) and the Carnegie Institution of Washington. Over the course of the 1920s, psychologists became the principal sources of authoritative knowledge about human behavior, and they embarked upon an effort to attack the roots of sexual, mental, and social dysfunction. It was not a coincidence that the investigation and control of behavior became a pressing issue for psychologists at the same time as foundation officers made it a top priority. As psychologist Franz Samuelson argues, “the availability of grants surely helped to increase the concerns of psychologists with ‘practical’ problems of behavior and adjustment.”299 The focus on “practical problems of behavior and adjustment” by psychologists inaugurated what I have labeled the second phase of the human engineering effort, which was devoted to improving mankind through the investigation and control of behavior. This second phase was a transitional period that linked the first phase, which was devoted to “movements” (especially social hygiene)


rather than scientific research, with the third phase, which was devoted to the investigation and control of the structure and function of human bodies on a molecular level.

The cooperation between psychologists and foundation officers in the quest to investigate and control human behavior reached its fullest realization at Yale University, where psychologist-president James R. Angell (1869–1949) and “psychobiologist” Robert M. Yerkes (1876–1956) received millions of dollars for the behavioral science programs that they established in the 1920s. Beginning in the mid-1920s, foundation officers, especially representatives from the Rockefeller boards, became intimately involved in the project of building Yale University into a major research center in the behavioral sciences. With small supplements provided by the Carnegie philanthropies, the Rockefeller boards financed the Yale Institute of Psychology and Yerkes’s Primate Laboratories in the mid-1920s and then funded the expansion of these projects into the Yale Institute of Human Relations and Yerkes’s Anthropoid Experiment Station. The total amount allotted to the Yale behavioral sciences during the interwar period exceeded a whopping $7,000,000 (equivalent to over $85,000,000 in 2005).

An examination of the relationships between the organizers of Yale behavioral science projects and foundation officers provides insights into social, institutional, educational, and scientific trends that characterized philanthropic support for scientific research in the 1920s. First, Yerkes and Angell were part of the foundation interlocking directorate that had begun to form in the first decade of the twentieth century. They were both adept scientific entrepreneurs who held leadership positions in the National Research Council (NRC) and other professional associations, as well as “scientistic”
social reform organizations like the American Eugenics Society. Second, the Yale programs were to be interdisciplinary enterprises that overcame the limitations imposed by specialization, an idea that foundation officers epitomized in the motto “cooperation in research.” Third, Yerkes and Angell capitalized on the interest that foundation officers showed in sexual and mental hygiene by pledging that their research programs would be dedicated to the investigation and control of aberrant behavior. They were able to capture and sustain the interest of foundation officers because they promised to demystify human sexuality and to control erratic human behavior, thereby fulfilling human engineering goals. The Yale University behavioral science projects thus represented the main trends that defined foundation support for university science in the 1920s: grant recipients with close connections to foundation officers, a formal allegiance to transcending disciplinary boundaries through cooperative research programs, and the crafting of rationales for research programs that fit foundation interests in human engineering. Yale University was the testing ground for the second phase of the human engineering effort, where foundation officers learned about the possibilities and limitations of the quest to improve mankind through the investigation and control of behavior.

By the 1920s, American psychologists had become the reigning monarchs in the “kingdom of behavior” and had established their credentials as experts in the investigation and control of human conduct. The emphasis on behavior represented a drastic change from the late nineteenth century, when psychology was considered a subset of philosophy and was a science devoted entirely to the study of the mind. According to John O’Donnell, “In the American college of 1870, psychology—true to its
etymology—was virtually indistinguishable from the philosophy of the soul,” but within thirty years the field had been transformed into an experimental science dedicated to laboratory research.300 An analysis of the development of psychology within the United States from the late nineteenth century through the early twentieth century demonstrates that funding concerns continually influenced research trends, which led American psychologists away from philosophy and toward experimentalism, functionalism, comparative psychology, and behaviorism.

Psychology began to emerge as a distinct discipline during the era of scientific professionalization that characterized the late nineteenth century. The field was in flux for much of that time, as psychologists established standards and norms for their field to set it apart from both philosophy and physiology. Part of psychologists’ professionalizing campaign involved building and equipping laboratories, where they used “brass and glass” instruments to conduct scientific experiments on human subjects. The use of complicated instruments in laboratories helped bolster psychologists’ claims that their work involved specialized scientific expertise. The emphasis on experimentation also made psychologists dependent on philanthropy, because they needed funding to build and equip laboratories.

The pioneer of experimental psychology was Wilhelm Wundt of Leipzig, who established the first psychological laboratory at his Institute of Experimental Psychology in 1875.301 In the 1870s and 1880s, Americans were flocking to Germany in droves to obtain higher degrees, and Wundt’s laboratory was the premiere location to study

psychology.\textsuperscript{302} Wundt endeavored to make psychology into an objective science by creating experiments in which human subjects were exposed to stimuli and asked to select a response from a standard set of answers. In other words, his experiments were standardized, under controlled conditions, and thereby repeatable.\textsuperscript{303} The many Americans who studied with Wundt, including William James, G. Stanley Hall, and J. McKeen Cattell, imported a version of his laboratory experimentalism to the United States. They built laboratories where they could replicate Wundt’s tests of reaction-times, sensation, and perception, and using tachistoscopes, timers, and gauges.\textsuperscript{304}

Wundt’s student G. Stanley Hall became a professor of psychology and pedagogics at Johns Hopkins in 1884. In his inaugural lecture, Hall outlined the components of the “new psychology,” a field informed by Darwinian evolutionary theory, experimentalism, and physiology. One important job of the psychologist, according to Hall, was to determine which qualities of behavior were innate and instinctual and which were adaptive and learned.\textsuperscript{305} Hall presented these research problems and questions to his students at Johns Hopkins, where he trained the next generation of American experimental psychologists, including John Dewey, Joseph Jastrow and J. McKeen Cattell.\textsuperscript{306} All of these students became leaders in the field and helped to move the discipline closer to the study of behavior.


\textsuperscript{304} Arthur L. Blumenthal, “Wundt and Early American Psychology,” 130; a tachistoscope is a machine that flashes images on a screen and measures response time.

\textsuperscript{305} O’Donnell, \textit{The Origins of Behaviorism}, 120–121.

\textsuperscript{306} O’Donnell, \textit{The Origins of Behaviorism}, 3.
Jastrow inaugurated the psychology department at University of Wisconsin in 1888 and immediately published his version of the “new psychology” in Cattell’s journal, *Science*. Like Hall, Jastrow argued that the “new psychology” was objective, experimental, comparative, and based on evolutionary theory. To comprehend the intricacies of higher mental functioning, Jastrow contended, psychologists should study animals, children, and primitive peoples. These subjects would allow psychologists to trace the evolution of mind from reflexive and instinctual behavior, which was exhibited by the “lower” forms of development, to the fine-tuned adaptive thought processes of civilized adults.\(^{307}\) Ideas like Jastrow’s became integral to trends that took over psychology in the early twentieth century: an emphasis on animals (comparative psychology), children (child development), and primitive peoples (anthropology) as research subjects, and a growing interest in the evolution of mind and behavior. These trends were woven into a theoretical approach that became known as “genetic psychology.” The idea behind genetic psychology was that the mind arose as an evolutionary adaptation that enabled humans to progress beyond other species. Genetic psychology united the mental testing movement, which was launched in the 1880s by eugenics and anthropometrics pioneer Francis Galton, with child study and animal research, and it became the foundation for the two major movements in American psychology during the early twentieth century: functionalism and behaviorism.\(^{308}\)

The theory of functionalism was introduced by psychologist James Rowland Angell, who will figure prominently later in this chapter as the organizer of the Yale University behavioral science programs. James Rowland Angell was the son of James


Burrill Angell, who had been one of the leading educational reformers of the late nineteenth century as the president of the University of Michigan during its formative years. J. R. Angell had followed in his father’s footsteps by going into academia. He had quickly moved through the ranks in the Department of Psychology at the University of Chicago. When he announced his theory of functionalism in his 1906 presidential address for the American Psychological Association, Angell was serving as the chair of the University of Chicago psychology department. By that point, he had established his reputation as a prominent psychologist. Still, his career as a promoter of psychology was only in the beginning stages. As we will see later in the chapter, Angell would go on to become one of the top scientific entrepreneurs in the United States during World War I before launching the largest behavioral science program in the country as the president of Yale University.

Angell’s theory of functionalism was his attempt to design a methodology that could effectively test the theory of genetic psychology. He suggested that psychologists concern themselves with mental processes as they occurred in realistic scenarios. In other words, functionalist psychologists should study the activities performed by the mind rather than the content of mind. Functionalism was distinctly American. It was based on the same principles as pragmatic philosophy: an emphasis on action rather than contemplation, on doing over thinking, and on the study of behavior as well as the study of consciousness. Some adherents of functionalism embraced animal experimentation because it offered the possibility of comparing instinctual behavior with adaptive, civilized behavior. Accordingly, the group of functionalists who relied on animal experimentation became known as “comparative psychologists.” Angell’s student John

\[\text{O'Neil, The Beginnings of Modern Psychology, 64.}\]
Broadus Watson, along with Robert Mearns Yerkes and E. L. Thorndike, were the most vocal champions of comparative psychology. Yet, to promote comparative psychology and animal experimentation, Watson, Yerkes, Thorndike and their compatriots had to surmount major obstacles. Animal experimentation was costly, and it did not fit neatly into the existing laboratory space of most university psychology departments, where experimental psychologists had only just gained enough support to equip their labs with the instruments for testing human subjects. For comparative psychologists in particular, it was imperative to appeal to potential patrons.

After he completed the Ph.D. with Angell at the University of Chicago, John B. Watson attempted to start an animal research laboratory there, but he was unable to gain adequate financial support. In 1908, Watson moved to Johns Hopkins, where he became professor of experimental and comparative psychology and head of the psychological laboratory.\(^{310}\) As he actively promoted animal experimentation as the most scientifically sound approach to the study of behavior, Watson began to develop his revolutionary theory of behaviorism. Meanwhile, Robert Yerkes was trying to build a program of comparative psychology at Harvard University. Yerkes had entered Harvard in 1897 after graduating from Ursinus College.\(^{311}\) He had hoped to work with E. L. Thorndike, who had been attempting to build a laboratory of comparative psychology, but the program had received so little financial support that Yerkes had turned instead to zoologist Charles B. Davenport.\(^{312}\) Under Davenport's tutelage, Yerkes had conducted research on nerve physiology in amphibians to establish the difference between instinct

\(^{312}\) Davenport would later become the most famous eugenicist in the United States. See Chapter Four of this dissertation.
and habit. This research placed Yerkes squarely within the field of physiology, though he protested that his work was actually “sensory psychophysiology” designed to determine the mental capacity of various organisms.

After Yerkes completed his Ph.D. in 1902, he had to campaign vigorously to be included in the Harvard Department of Philosophy as a half-time instructor. At the time, the leaders of philosophy department saw psychology as the study of consciousness, which required human subjects. Since Yerkes remained devoted to animal experimentation and comparative psychology, he had hardly any institutional support. Harvard president A. Lawrence Lowell convinced Yerkes that “educational psychology offered a broader and more direct path to a professorship and to increased academic usefulness than did [the] special field of comparative psychology.” For professional reasons, then, Yerkes joined the mental testing and educational theory movement. Nonetheless, Yerkes never stopped working toward his dream of building a comparative psychology laboratory.

From 1905 until 1913, Yerkes and Watson corresponded frequently about the field of comparative psychology and the need to promote animal experimentation as the optimal way to study behavior. Yerkes regularly wrote articles about comparative psychology for professional journals and made strong contentions about the need to establish psychology as a legitimate professional scientific discipline. To Yerkes,

313 O’Donnell, Origins of Behaviorism, 192.
314 Donna Haraway, Simian, Cyborgs, and Women, 48.
316 Ibid., 390.
317 Ibid.
comparative psychology offered the optimal route to scientific validity for his field because animal experimentation could yield laws of behavior that were applicable to all forms of life, a rationale equally well suited to Watson’s behaviorism.

In a 1913 lecture at Columbia University that became known as the “behaviorist manifesto,” “Watson announced that psychology should become an objective experimental science of the prediction and control of behavior.” Watson argued both on behalf of conducting animal research in a controlled laboratory environment and for a new conception of psychology devoted to predicting and controlling behavior. For psychologists who remained wedded to the idea that the study of behavior was only a tool for the more important investigation into the inner workings of the mind, Watson was advocating for a revolutionary principle: psychology should be the study of behavior exclusively because consciousness could not be examined scientifically. As Watson wrote in one of his many arguments for behaviorism, “Since you were trained in the system and in the vernacular of James, Angell, Ladd, and Wundt, you said you saw consciousness. What was it? . . . just a masquerade for the soul.” Psychologists who studied consciousness were chasing ghosts. Watson offered an easy solution: “If the study of mind—the analysis of consciousness—is not what psychology is about, what then is its field and what is its goal? . . . Behaviorism is a study of what people do.”

Watson’s formulation of behaviorism offered a connection between psychological research and practical social applications, as well as a strong justification for animal experimentation. In theory, the study of animal behavior could provide insights into

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322 Ibid., 8.
human behavior that could then be used to investigate and attack the causes of
dysfunction. Yerkes embraced a watered-down version of behaviorism as he continued
to campaign for his dream of a comparative psychology laboratory. He was building a
set of rationales for his research that would soon prove valuable. During World War I,
Yerkes finally had the opportunity to demonstrate the value of applied psychology based
on behaviorism and, as a result, to capture the interest of foundation officers in his animal
experimentation plans. At the same time, Yerkes was refining his idea of the most
suitable animals for comparative psychology research. He chose primate research, which
became his overriding preoccupation for the remainder of his career.

Yerkes’s first foray into primate research came as the Great War raged in Europe.
In February 1915, he traveled to Santa Barbara, CA to work in Dr. G. V. Hamilton’s
laboratory, where Yerkes compared “ideational behavior” in monkeys and apes to that of
man. After a six month stay with Hamilton, Yerkes returned to the east coast to join
the war effort. Soon after his return, Yerkes began pleading his case that applied
psychology and, by extension, primate research would help to bring “progress and peace”
through “a safer control of behavior.” In an argument that was to be repeated by
psychologists for decades, Yerkes contrasted the remarkable developments in physical
sciences with the relative “backwardness” of the behavioral sciences, a disparity that was
dangerous for civilization. The outbreak of hostilities in Europe only served to
illustrate the point:

325 Robert Mearns Yerkes, The Mental Life of Monkeys and Apes (1916): A Facsimile Reproduction with an
327 See Frank Samuelson, “Organizing the Kingdom of Behavior,” for a description of the concept of
“cultural lag.”
Surely this war clearly indicates that the study of instinct, and the use of our knowledge for the control of human relations, is incalculably more important for the welfare of mankind than is the discovery of new and ever more powerful explosives. . . . If we really desired above anything attainable on earth the lasting peace of nations, we should diligently foster and tirelessly pursue the sciences of life.  

He went on to suggest that support was needed for "effective systems of bodily and mental hygiene."  

It is interesting to note that the exact arguments that Yerkes delineated in this 1915 article became the basis for NRC programs and foundation policies during and after the war, not to mention justifications for building Yerkes's primatology laboratories and the Yale Institutes of Psychology and Human Relations. Yerkes linked the "welfare of mankind," which was (not incidentally) the Rockefeller Foundation's charter mandate, to the investigation and control of human behavior. He was beginning to outline his understanding of human engineering, which was connected to the social and mental hygiene programs that the Rockefellers were sponsoring. There is no doubt that the Rockefeller Foundation officers were aware of Yerkes's version of human engineering because he sent them a memo in 1915 titled "Knowledge of Behavior and Mind in the Racial and Individual Development is Essential to Intelligent and Effective Social Behavior," in which he used many of the same arguments that he made in the "Progress and Peace" article. In the memo, Yerkes also tied his general arguments about potential applications of psychology to specific experimental programs. One area of concern that

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Yerkes highlighted was research into problems of sex, which was already receiving support through the Rockefeller-sponsored Bureau of Social Hygiene. He connected the need for sex research to his proposal for a primate laboratory:

In order to make safe and rapid progress toward the control of individually and socially pernicious sexual conditions and relations, it is necessary to know intimately and thoroughly . . . the essential facts of sex behavior in the normal individual . . . and . . . the variations or deviations from the normal, usual, or desirable in any of the above classes of facts of behavior and consciousness. . . . It is my contention . . . that an intensive experimental study of the sexual life of monkeys and apes is our best approach to that knowledge of sexuality in man which alone can guide aright our labors in sex education and hygiene. . . . There is urgent need, as an instrument of social progress, of an Institute for Genetic (or Biological) Psychology, the function of which should be the study of behavior, conscious and unconscious, human and infra-human, in all of its manifold forms and relations, in the interests of social control, and for the welfare of mankind.\(^\text{330}\)

Yerkes did a masterful job of combining the social reform concerns of foundation officers, which were linked to ideas gleaned from the eugenics and social hygiene movements, with his personal and professional interests. He crafted a rationale for his primate research based on assumptions that resonated with foundation officers for years to come; specifically, that sex was a primitive, instinctual impulse that represented behavior on a lower level of the evolutionary scale and that researching chimpanzee sexual behavior was the closest psychologists could get to raw data on basic human

\(^{330}\) Memo from Yerkes to the Rockefeller Foundation, “Knowledge of Behavior and Mind in their Racial and Individual Development is Essential to Intelligent and Effective Social Behavior” (1915), folder 1089, box 57, Series II, Yerkes Papers, Yale University Archives, New Haven, Connecticut (henceforth YA).
instincts. In light of these assumptions, Yerkes’s assertion that the study of the sexual behavior of monkeys and apes would contribute directly to the “welfare of mankind” actually made sense to the Foundation officers. This memo helped set the stage for Yerkes to develop close relationships with Rockefeller Foundation officers during the war, relationships that became critical for his postwar career.

Yerkes pressed his case for primate research throughout the following year. He appealed to the scientific community at large in February 1916 with an article in Science titled “Provision for the Study of Monkeys and Apes.” According to Yerkes, by breeding and raising nonhuman primates in captivity, scientists would have the opportunity to know every aspect of the animals’ life histories. Knowledge of the apes from birth to death would allow for a level of control that would prove particularly advantageous when extrapolating from primate to human behavior. Yerkes again tied his research interests to improving mankind by suggesting that primate research would allow psychologists to conduct “systematic and continuous studies of important forms of behavior, of mind, and of social relations” that would shed light on human behavior. The prediction and control of human behavior was the focus, and primate research was a means to this end.

Even though Rockefeller Foundation officers seemed receptive to his arguments about the value of primate research, they were not accepting applications because they were committed exclusively to war work. Yerkes turned instead to the Carnegie Institution of Washington (CIW), but he only received an obtuse reply from CIW

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333 Yerkes, The Mental Life of Monkeys and Apes (1916), 135.
president Robert Woodward, whose mental health was deteriorating at the time.\textsuperscript{334} He decided that he would have to build a better rapport with foundation officers before he applied again for primate research funding.\textsuperscript{335} Yerkes decided to concentrate on contributing psychological research the war effort. As he helped to marshal scientific resources for American war involvement, Yerkes never lost sight of his professional goals. World War I provided Yerkes with an excellent opportunity to make a name for himself and his field, and he planned to use his professional reputation and connections to garner interest in his primate laboratory.

In 1916, Yerkes was well enough known and respected that he was elected president of the American Psychological Association (APA). He dedicated his presidency to mobilizing psychologists for the war.\textsuperscript{336} Yerkes began his mobilization effort by spearheading the formation of an APA Committee on Psychological Examination of Recruits, which he also chaired.\textsuperscript{337} The Committee members designed mental tests to be administered to recruits, whose scores would be tabulated to determine where they were best suited to serve in the army.\textsuperscript{338} Yerkes's testing program appealed both to the Army and to the newly organized National Research Council (NRC), which led to the dual roles that he would assume during the war: chief of the Division of

\textsuperscript{334} Yerkes to Robert Woodward, President of CIW, May 22, 1916, folder 1536, box 80, Series II, Yerkes Papers, YA.
\textsuperscript{335} Woodward to Yerkes, June 1, 1916; Yerkes to Pritchett, June 5, 1916; folder 1536, box 80, Series II, Yerkes Papers, YA.
\textsuperscript{336} Yerkes, “Robert Mearns Yerkes, Psychobiologist,” 397.
\textsuperscript{338} Kevles, “Testing the Army’s Intelligence,” 566.
Psychology in the Office of the Surgeon-General and chairman of the Psychology Committee of the NRC.\textsuperscript{339}

By May 1917, Yerkes had set up a plan for a classification system to assign men to the most appropriate duties for war service based on their responses to psychological tests. The exams measured potential soldiers' "mental alertness or intelligence" and "occupational training, experience, and proficiency."\textsuperscript{340} The men were rated, classified according to physical and mental characteristics, and then "assigned to [a] place in the military machine."\textsuperscript{341} Through the Division of Psychology, Yerkes created the Committee on the Classification of Personnel with James R. Angell to train examiners, administer tests, and correlate scores with personnel decisions. From August 1917 to December 1918, the Personnel Committee tested and rated 1,750,000 recruits.\textsuperscript{342} Although the program was discontinued by the Army in 1918, it had significant long-term impacts for Yerkes, Angell, and the field of psychology.

The tests captured the interest of General Education Board (GEB) officers, who appropriated $25,000 to the NRC to develop a variation of Yerkes's test for use in primary and secondary schools as part of their effort to encourage standardization of education at all levels.\textsuperscript{343} This gave Yerkes and Angell an opening for further negotiations with the Rockefeller boards. The example of psychological testing during the war was cited again and again, especially by Yerkes, to demonstrate that applied psychology could be used to mold a better, more efficient society. As he explained,

\textsuperscript{341} Ibid., 358.
\textsuperscript{342} Kevles, "Testing the Army's Intelligence," 573.
\textsuperscript{343} Robert Mearns Yerkes, "Psychological Work of the National Research Council," \textit{Annals of the American Academy of Political and Social Science} 110 (November 1923): 173.
The theory of psychological service was that human factors should be appreciated, measured, and intelligently used, that so far as feasible chance, personal whim or bias, and convention should be replaced by action in the light of reasonably accurate and thorough information. In a word, that the army should utilize what may be called "human engineering," just as it attempt to use other forms of engineering which have to do primarily with non-living things.\footnote{\textit{Ibid.}}

Yerkes was the first to put a name to the "human engineering" effort that foundation officers had been implicitly planning before and during the war. He placed psychology at the helm of a human engineering project, where it stayed for over a decade. The key promise of psychology as human engineering was that objective science could be used to assess and control "human factors." This argument proved extremely compelling to the foundation officers with whom Yerkes was corresponding on a regular basis.\footnote{Haraway, \textit{Primate Visions}, 66.}

After the war, both Yerkes and Angell maintained leadership positions in the National Research Council (NRC) and continued to develop stronger relationships with philanthropic foundation officers. Angell took over as chairman of the NRC. Yerkes decided to remain in Washington, DC as director for the NRC Research Information Service, a paid position that involved collating research completed during the war. He also remained the chairman of the NRC Division of Psychology and coordinated the Committee on Industrial Personnel Research with Angell. According to Yerkes, he chose to take the NRC post because it offered the best opportunity to promote his primate
research laboratory with foundation officers. In his autobiography, Yerkes recalled his decision and was unabashed about his motives:

I wished to complete and superintend the publication of the official report of our psychological work during the War, and, picking up the threads of my psychological past, to endeavor to find financial support for the systematic utilization of anthropoid research. . . . I was ready and eager to serve the National Research Council . . . in part because the connection enabled me to remain in Washington where conditions seemed particularly favorable for the promotion of my pet project.

While they worked at the NRC headquarters, Yerkes and Angell strengthened their connections with the nation's most adept scientific entrepreneurs, including George E. Hale, John C. Merriam, and William H. Welch, all of whom were thoroughly enmeshed in the foundation interlocking directorate. Yerkes continued to refine his arguments on behalf of his primate laboratory idea and to link his research to the existing human engineering projects that had been sponsored by philanthropic foundations, especially social hygiene, mental hygiene, and eugenics.

Between 1921 and 1922, opportunities for Yerkes to present his rationales on behalf of primate research to foundation officers abounded. In 1921, he was involved in a Carnegie Institution of Washington (CIW) conference on "The Physical Basis of Human Behavior" that brought him together with a number of powerful individuals within the interlocking directorate, and he participated in the inauguration of two NRC Committees that served as conduits for advertising his primate research. At the same

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346 Haraway, Simians, Cyborgs, and Women, 49.
347 Yerkes, "Robert Mearns Yerkes, Psychobiologist," 399–400.
348 Yerkes, "Robert Mearns Yerkes, Psychobiologist," 401.
time, through the NRC Committees, Yerkes solicited attention and support for his research from Beardsley Ruml at the Laura Spelman Rockefeller Memorial (LSRM). Finally, he began to negotiate with James Angell about moving to Yale to build a psychology and primatology program there.

Yerkes was invited to participate in the CIW conference on “The Physical Basis of Human Behavior” in 1921 due to connections he had made through the National Research Council (NRC) and the American Eugenics Society, as well as his close relationship with Angell, president of the Carnegie Corporation of New York (CCNY) at the time, and John C. Merriam, who was both director of the CIW and the chairman of the NRC. Yerkes joined fellow conferees Merriam, E. G. Conklin, C. B. Davenport, C. E. Seashore, E. L. Thorndike, and Clark Wissler (all of whom were in the inner circles of philanthropic foundations) in a joint statement that declared “that the time is now ripe for a determined extension of biological research in the realm of behavior and experience.”349 To Yerkes’s delight, the subject of his primate research laboratory was carefully considered and “the majority of conferees expressed the opinion that the wide utilization of infrahuman organisms may be made to contribute substantially to the solution of human problems.”350 In the summary statement, the conferees referred to their research agenda in terms that Jastrow had described as the “new psychology” as far back as the 1880s and that Yerkes hoped to utilize in his research: “The point was made that the study of ‘the monkey, the baby and the idiot’ is likely to prove of fundamental importance in the further development of psycho-biology.”351 The conferees were

350 Ibid., p. 2.
351 Ibid., p. 4.
basing their arguments on entrenched assumptions about the applications of evolutionary theory, which suggested the best way to understand higher, complex thought and behavior was to analyze "lower," simpler forms of life. The selected research subjects (monkey, baby, idiot) were seen as equally low on the evolutionary ladder and thus virtually interchangeable for the study of behavior and development, a belief that Yerkes exploited in his applications for his primate research laboratory. Basically, he argued that apes were considerably easier to breed and manage in captivity than babies or idiots, making primates the best possible subjects for investigating and modifying human behavior.

Yerkes soon had additional opportunities to present his research rationales to foundation officers. In 1922, he was appointed chairman of two NRC Committees that promised to attack social problems through the scientific investigation of human behavior and biology: the Committee on Scientific Problems of Human Migration and the Committee for Research in Problems of Sex. Both committees received substantial foundation support, especially from Rockefeller boards. The Committee on Scientific Problems of Human Migration was relatively short-lived and Yerkes served as its chairman for only two years (from 1922 to 1924). Nonetheless, Yerkes was able to solicit a sizeable grant from the Laura Spelman Rockefeller Memorial (LSRM) for the Migration Committee, primarily due to the tight connections between foundation officers and committee members. The leaders of the Migration Committee—Yerkes, Raymond Dodge, John C. Merriam, Clark Wissler, and Frank R. Lillie—became ubiquitous in foundation circles.352

352 Robert M. Yerkes to Beardsley Ruml, February 26, 1923, folder 629, box 58, Subseries 6, Series 3, Laura Spelman Rockefeller Memorial Archives, RAC.
As Yerkes helped to shape NRC policies, Beardsley Ruml was transforming the Laura Spelman Rockefeller Memorial (LSRM) into a foundation dedicated to social science research. As noted in the previous chapter, Ruml had a long history with Angell, who had been his advisor and mentor. Ruml also had a close connection with Yerkes because he had worked with Angell and Yerkes during the war as a member of the Committee on Classification of Personnel. The relationships among the three men had appreciable consequences from the future development of behavioral sciences at Yale University in the 1920s. When Ruml began his overhaul of the LSRM to focus on building university research in the social sciences and psychology, he naturally saw his former colleagues and mentors as ideal grant recipients.

The first Laura Spelman Rockefeller Memorial (LSRM) grant that Ruml coordinated for Yerkes was through the NRC Migration Committee. The research plan that Yerkes submitted to Ruml used "migration" as a focal point to organize a program that built on eugenics and human engineering:

This Committee was appointed . . . carefully to consider, from the point of view of natural science . . . the virtual elimination of space as a barrier to movements of men and to race intermixture [and] to prepare a research program which might reasonably be expected to yield ultimately such reliable information concerning physical, mental and social characteristics, relations and values of ethnic groups

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354 Ibid., 355–358.
(races or peoples) as are necessary for the understanding and wise regulation of mass-movements of mankind.\textsuperscript{355}

After reviewing this report, Ruml secured an appropriation of $60,000 to support Migration Committee programs.\textsuperscript{356} Several of the projects supported by the grant were vaguely defined and difficult to organize. One called for an "effort to internationalize or universalize methods of measuring important mental traits," while another called for a "general analysis of human personality."\textsuperscript{357} The appropriation yielded few tangible results and Yerkes abandoned his chairmanship of the committee in 1924. In the end, the membership of the Migration Committee and the alliance with Ruml were much more significant than the projects themselves.\textsuperscript{358} Not only did Yerkes further solidify his relationship with Ruml, which paid huge dividends when the LSRM funded his primate laboratories, but the Migration Committee members eventually moved en masse to staff the Yale Institute of Psychology.

The Migration Committee was overshadowed by its cousin, the NRC Committee for Research in Problems of Sex. The Committee on Sex Problems (as it came to be known) was also chaired by Yerkes and included Migration Committee members Frank R. Lillie, E. G. Conklin, and C. E. Seashore.\textsuperscript{359} The Committee on Sex Problems was the culmination of efforts begun by the Rockefeller Bureau of Social Hygiene coupled with a response to trends in the psychological and biological sciences that brought increasing

\textsuperscript{355} "Report and Recommendations of the Committee on Scientific Problems of Human Migration" by Robert Yerkes, April 2, 1923, folder 629, box 58, Subseries 6, Series 3, Laura Spelman Rockefeller Memorial Archives, RAC.

\textsuperscript{356} Ruml to Vernon Kellogg, May 9, 1923, folder 629, box 58, Subseries 6, Series 3, Laura Spelman Rockefeller Memorial Archives, RAC.

\textsuperscript{357} Yerkes, "Psychological Work of the National Research Council," 177.

\textsuperscript{358} Haraway, \textit{Primate Visions}, 71.

levels of attention to sexuality. While the Bureau of Social Hygiene initially focused on curtailing prostitution and venereal disease through educational or recreational programs devoted to healthy sexual conduct, leaders of the social hygiene movement turned to research scientists after the war. As former Committee on Sex Problems members Sophie Aberle and George Corner contend,

The able men in this movement, though primarily social reformers, were well aware that problems of human conduct basically depend upon biological and psychological factors that were subject to scientific study. Therefore the Rockefeller Foundation, with which they were closely associated, was interested in encouraging basic research.\textsuperscript{360}

The idea that “problems of human conduct basically depend upon biological and psychological factors that were subject to scientific study” went unquestioned by foundation officers. The connection between “problems of human conduct” and sexual behavior was also an unchallenged assumption. Based on their tacit acceptance of a mixture of Freudian psychoanalysis, evolutionary theory, and eugenics, the foundation officers believed that sexual problems or poor breeding would lead to social and mental dysfunction.\textsuperscript{361}

The original organizer of the NRC Committee for Research in Problems of Sex was Earl Zinn, a psychologist who had earned his Ph.D. with G. Stanley Hall at Clark University. During the war, Zinn had worked for the Bureau of Social Hygiene.\textsuperscript{362} In 1921, Zinn had contacted Yerkes to help him “seek the advice of competent scientific

\textsuperscript{360} Aberle and Corner, \textit{Twenty-Five Years of Sex Research}, 4.


\textsuperscript{362} Pickren, “Yerkes, Stone, and Programmatic Sex Research,” 605.
men and work out ways and means to establish the kind of research program in which the Rockefeller group was now seriously interested." Yerkes had immediately set to work gathering representatives of the NRC for a conference about organizing a sex research program. It had quickly become apparent that a sex research committee could provide compelling rationales for Yerkes's primate laboratory, as well as for the general expansion of applied psychology:

Psychological and anthropological approaches to problems of human sexuality dominated the conference discussion. Primate studies, psychoanalysis, anthropological field studies, and endocrine research were all suggested. All conferees agreed that practical applications of any potential sex research must be given priority. Although the discussion roamed over many topics and methods, a common thread . . . was the need to find a way to control sexuality.\footnote{Aberle and Corner, \textit{Twenty-Five Years of Sex Research}, 10.} \footnote{Pickren, \textit{"Yerkes, Stone, and Programmatic Sex Research."} 607.}

The quest to control sexuality through scientific investigation had brought the organizers of the committee back to the same research subjects that the conferees had discussed at the CIW conference on "The Physical Basis of Human Behavior:" nonhuman primates, "savages," children, "primitive peoples," and "normal and pathological human beings at different ages."\footnote{Pickren, \textit{"Yerkes, Stone, and Programmatic Sex Research."} 608; Aberle and Corner, \textit{Twenty-Five Years of Sex Research}, 14–15.}

Yerkes and his fellow committee members set up a rationale for their program that addressed social problems as objects for scientific study, preferably by a cooperative, interdisciplinary group of scholars. The Committee for Research in Problems of Sex had been organized "because a group of responsible American philanthropists, physicians,
and scientists felt an urgent demand for study of human sex behavior with all the resources of modern science." The founders of the committee hoped that "by their concerted effort and with the prestige of the National Research Council they could raise to scientific favor in the United States a subject which up to that time had remained in relative disrepute," and "that they could stimulate and coordinate research in all the related sciences that bear upon human behavior." The primary aim was to establish "a firm structure of scientific knowledge upon which enlightened social programs could ultimately be built by others." The rationale for the research program that the Committee put together was thus a translation of the Bureau of Social Hygiene's effort "to establish a structure of pure research for enlightened social policy on matters such as sex education, family counselling [sic], eugenics, venereal disease, divorce, and birth control" into a professional scientific research agenda. The wide variety of problems that were assumed to be solvable through the control of sexuality revealed how deeply scientists and foundation officers had embraced the idea that, as Michel Foucault contends, "sex was a means of access both to the life of the body and the life of the species."

In real terms, the program for the NRC Committee on Sex Problems was a vehicle to advance the interests of its members and channel funds toward their existing research laboratories. The Committee divided the fields worthy of support into "the study of normal and abnormal human sex behavior, observation and experiment on sex behavior

366 Aberle and Corner, Twenty-Five Years of Sex Research, 1.
367 Ibid., 14.
368 Ibid.
369 Haraway, Simians, Cyborgs, and Women, 49.
of animals, and physiological studies on the role of the nervous system and the endocrine glands in sex behavior." The "observation and experiment on sex behavior of animals" was a nod to Yerkes, while the "physiological studies" on the nervous system and endocrine glands was the primary research interest of Frank R. Lillie, whose heavily supported research at the University of Chicago will be discussed in Chapter Four. The Bureau of Social Hygiene allotted $25,000 to the Committee in 1923 to begin studies in these areas. Over the twenty-five year period from 1923 to 1947, Rockefeller Boards contributed $1,500,000 (approximately $15,000,000 in 2005) to the NRC Committee for Research in Problems of Sex, first through the Bureau of Social Hygiene and, after 1933, directly through the Rockefeller Foundation. Additionally, the Rockefeller Foundation contributed over $1,000,000 to aid projects at Columbia, University of Chicago, Cornell, and Yale that were initially sponsored by the Committee on Sex Problems. Three-quarters of the grant recipients at these universities were either members of the Committee or were close associates of members, with Yerkes, Lillie, and Cannon receiving the largest allotments.

The organization of the NRC Committee for Research in Problems of Sex had offered Yerkes hope for his dream project of a primate laboratory as early as 1922, but he was still working in the NRC Research Information Office in Washington, DC. He needed to move to a university, but he did not want to accept a post until he was sure that he would have a primate laboratory when he arrived. James Angell, after he became

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371 Aberle and Corner, Twenty-Five Years of Sex Research, 45.
372 Aberle and Corner, Twenty-Five Years of Sex Research, 23.
373 Aberle and Corner, Twenty-Five Years of Sex Research, 66.
374 Ibid.
president of Yale University in 1922, made it his personal mission to secure funding for a primate laboratory because he wanted to recruit Yerkes to head the Yale Department of Psychology. Part of Angell’s plan was to have Yerkes take charge of a new Yale Institute of Psychology.\textsuperscript{376} On June 24, 1922, Angell wrote Yerkes to explain his position:

I should be more than glad if we could command the resources to develop here a satisfactory psychological institute . . . More immediately, however, I should be delighted if I could succeed in laying hands on the necessary funds to bring you here and give you an independent opportunity to resume your scientific work and investigation.\textsuperscript{377}

In this letter, Angell both revealed his hope for building a psychological institute and his desire to attract outside sources of funding to support the enterprise. When Angell referred to Yerkes’s “scientific work and investigation,” one assumes that he was referring to finding the funds to build a primate laboratory.

Throughout the fall and winter of 1922, Angell corresponded regularly with foundation officers from the Rockefeller boards and the Carnegie Corporation to gauge the potential for securing funds for a psychological institute with an attached primate laboratory. He reported to Yerkes on January 25, 1923 that the GEB seemed interested in the institute idea:

I had an interesting interview Tuesday with [Abraham] Flexner [assistant secretary of the GEB] and found him in a mood to give serious consideration to

\textsuperscript{376} Angell to Yerkes, June 11, 1922, folder 32, box 2, Series I, Yerkes Papers, YA.
\textsuperscript{377} Angell to Yerkes, June 24, 1922, folder 32, box 2, Series I, Yerkes Papers, YA.
the possible establishment here at Yale as headquarters of such a psychobiological institute as you and I discussed.\textsuperscript{378}

Angell recruited Yerkes in order to launch a major "psychobiology" initiative at Yale. He further suggested to both Yerkes and the GEB officers that Yale could be an excellent "home base" for the NRC Committees on Scientific Problems of Human Migration and Research in Problems of Sex.\textsuperscript{379} Rather than funding an institute for psychobiology, though, the GEB appropriated a conditional matching grant of $15,000 a year for five years to establish a Department of Psychiatry and Mental Hygiene within the Yale Medical School.\textsuperscript{380} Angell decided his application for the Institute of Psychology would be more successful if he incorporated psychiatry into his plans. He also changed foundations. After all, his protégé Ruml was the director of the Laura Spelman Rockefeller Memorial (LSRM).

While Angell tried to find a guaranteed source of funding for the Institute of Psychology, Yerkes decided to initiate a primate research project from his home. In the summer of 1923, Yerkes purchased two chimpanzees from the Bronx Zoo for $2000, which was the majority of his life savings.\textsuperscript{381} The two chimps, Chim and Panzee, became part of the Yerkes family. In the winter in Washington, DC, Chim and Panzee shared bedroom space with the Yerkes children. In the summer, when the Yerkes family resided in their New Hampshire farm, Chim and Panzee lived in a remodeled barn complete with bed boxes, tire swings, and a miniature dinner table where they were taught to eat with

\textsuperscript{378} Angell to Yerkes, January 25, 1923, folder 32, box 2, Series I, Yerkes Papers, YA.
\textsuperscript{380} May, \textit{Toward a Science of Human Behavior}, 36.
forks. Chim was especially beloved by the entire Yerkes family, who treated him alternately as a pet, adopted child and research subject. The life of Chim was lovingly rendered in the aptly titled *Almost Human* (1925), in which Yerkes again laid out his case for the study of apes as a means of investigating and improving mankind. Though Chim and Panzee both died in 1924 (Panzee in January, Chim that following summer), the experience of living with and observing the chimps affirmed for Yerkes that he had found his life calling. He became much more receptive to Angell’s recruitment efforts, and agreed to move to Yale University in 1924 provided that his primate laboratory would be built and funded.

In early 1924, Yerkes and Angell worked as a team to craft an Institute of Psychology with attached primate laboratories that would appeal to Ruml at the Laura Spelman Rockefeller Memorial (LSRM). Yerkes wrote a memo to Angell on February 12, 1924 that outlined a program for the proposed Institute. The memo, which became the basis for the Yale applications to the LSRM, suggested research projects that fit the human engineering agenda perfectly. Raymond Dodge would conduct studies in the “general psycho-physiology of man”, Yerkes would research “comparative and genetic psychology—the study of behavior in ontogeny and phylogeny, utilizing any suitable type of organism, but with intent to focus on human problems and needs”, Karl Lashley would “associate[e] psychology with mental hygiene, psychiatry and neurology through the investigation of normal and abnormal behavior of man and other organisms,” Clark Wissler would analyze the nature of racial differences, and Carl C. Brigham would

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complete “mental measurement” of subjects. The projects dissected human behavior from multiple angles with the underlying assumption that psychological development and dysfunction were intimately connected to variables including physical qualities, race, and evolutionary development (from “lower” to “higher” forms of life). By tying the investigation of human behavior to quantifiable and observable metrics, the control of dysfunction appeared scientifically manageable.

It was no coincidence that the potential new hires for the institute were culled from the NRC Committees on Migration and Sex Problems. Brigham, Dodge and Wissler were on the Migration Committee, while Wissler and Lashley became members of the Committee on Sex Problems soon after they moved to Yale. Yerkes saw the connection with the NRC Committees as a key component of the funding plan, because the NRC would likely “augment” Institute projects “to the extent of ten thousand to twenty-five thousand dollars a year.” That is exactly what happened.

Angell quoted extensively from the Yerkes memo when he submitted applications to Rumr at the LSRM. In his letters to Rumr, Angell presented a vision for the Institute of Psychology that incorporated progressive social reform ideas, assumptions about evolutionary theory, and the ideals of pure, cooperative scientific research:

There is unquestionably great need at the present moment for the development of psychology—using the term in the broadest sense—to contribute light and guidance for the solution of many problems of our present social order. In part this light should come from a thorough-going study of the earlier conditions out

383 Memo from Yerkes to Angell re: “Proposed Academic Center or Institute of Psychology,” February 12, 1924, p. 1, folder 33, box 2, Series I, Yerkes Papers, YA.
384 Memo from Yerkes to Angell re: “Proposed Academic Center or Institute of Psychology,” February 12, 1924, pp. 2-4, folder 33, box 2, Series I, Yerkes Papers, YA.
of which humanity has developed, not only our human ancestors, but also the
more primitive types of animal life in whose activities we may hope to discover
some of the deep lying factors in human nature which occasion us perplexity... I
think it is a conservative statement to say that we are at the very threshold of the
most important advances in our understanding of human activities and our ability,
particularly through educational methods, to control human life for the betterment
of all future generations. The rapidity with which we are able to make the next
great forward steps seems to depend chiefly upon the promptness with which we
can develop a trained personnel of the highest intellectual quality to enter upon
systematic and wide-ranging study of these problems. On the whole, experience
seems to indicate that the wisest method to make a beginning in matters of this
kind is, if possible, to bring together a group of competent scholars who may
work side by side in a flexible but definite organization, enjoying the stimulation
and control which such scientific companionship permits. For many reasons...
there are no inconsiderable advantages to be gained from the connection of such a
group to a university center.\footnote{Angell to Ruml, February 20, 1924, folder 827, box 79, Subseries 6, Series 3, Laura Spelman Rockefeller Memorial Archives, RAC.}

It this was a “conservative statement” about the potential to improve mankind through
science, as Angell attested, it is difficult to imagine how a hyperbolic statement would
read. According to Angell, funding an Institute of Psychology at Yale had the potential
to solve myriad problems that plagued humanity. Although his promises were rather
lofty, Angell made a very compelling argument. To Ruml, the plan of mobilizing “a
group of competent scholars” in a cooperative effort “to enter upon systematic and wide-
ranging study” of the “many problems of our present social order” in the quest “to control human life for the betterment of all future generations” was worthy of substantial support. In June 1924, Ruml offered an LSRM grant of $40,000 a year for five years to Yale University, plus $7500 for “initial expenses incident to the establishment of its institute of psychology.”

Yerkes moved to Yale as soon as Angell informed him about the LSRM appropriation. From the outset, Yerkes had only one thing on his mind:

This research position I accepted with the understanding that I should be free to devote myself to comparative psychobiology and to promote, as might prove practicable, achievement of facilities for the scientific utilization of anthropoid [ape] subjects. . . . Although it did not provide immediately precisely the type of establishment and equipment which I had long desired and labored to bring into existence, it did supply an institutional connection which, largely because of the sympathetic interest and professional knowledge of James R. Angell, promised to be incomparably useful.

Within weeks after moving his family to New Haven, Yerkes set off on an anthropoid research excursion to Cuba, armed with a grant from the Carnegie Institution of Washington. Yerkes spent the summer of 1924 in Madame Abreu’s ape colony in Havana, which exemplified, in Donna Haraway’s words, “the intersecting construction of nonhuman primates as pets, surrogate children, endangered species, research animals,

\[386\] Preliminary Formulation of Plan for Institute of Psycho-biology at Yale University,” March 19, 1924; Ruml to Graves (Yale provost), June 27, 1924, folder 827, box 79, Subseries 6, Series III, Laura Spelman Rockefeller Memorial Archives, RAC.
\[387\] Yerkes, “Robert Mears Yerkes, Psychobiologist,” 402.
colonial subjects, and wild animals.” The Havana ape colony was one of only a few in the world, as the concept of primate studies was in its very earliest stages at the time and caring for “anthropoids” (as Yerkes had begun calling nonhuman primates) was a time-consuming and expensive task. For Yerkes, the ape colony was paradise. It showed what was possible if investors supplied the necessary funds for primate research: a center where “anthropoids” could be bred, observed, analyzed and manipulated to reveal the basic behavioral models for humans.

Yerkes returned from Havana convinced that he needed additional funds, beyond the Laura Spelman Rockefeller Memorial (LSRM) allotment for the Institute of Psychology, to build a primate research station. He and Angell began to exchange ideas about how to approach Edwin Embree, who had recently taken over as director of a new Rockefeller Foundation “Division of Studies” program that was dedicated entirely to the support of university research. Yerkes suggested that they apply for “psychobiological research with the infrhuman primates in connection with the Institute of Psychology” with a concentration “on the investigation of important aspects of ideational and sex behavior.” The ostensible reason for Yerkes’s first letter to Embree on January 5, 1925 was to “inform” him about the activities of the NRC Committee on Sex Problems, but he quickly turned to the subject of his primate research:

In view of the establishment of a “Division of Studies” in your organization and your interest and responsibility, it is quite clear that you should be informed concerning the history, achievement, and plans of our Committee for Research on

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389 Yerkes to Angell, folder 33, box 2, Series I, Yerkes Papers, YA.
390 Yerkes to Angell, folder 1673, box 159, Record Group 2-A, James Rowland Angell Presidential Records, 1921–1937, YA.
Sex Problems. . . I am sure it is at least as important for us as for you that we establish intimate contacts. . . It is clearer than ever that I should consult with you about certain aspects and relations of our anthropoid station. 391 Embree expressed some interest in Yerkes’s ideas. In response, Yerkes began to send Embree copies of his funding applications to the NRC and the Carnegie Institution of Washington (CIW). 392 The CIW had offered small grant contributions for Yerkes to work with his pet chimpanzees and to publish Almost Human (1925). 393 Yerkes wanted to illustrate to Embree both that he was unlikely to receive any additional support from the CIW and that his anthropoid project was a worthwhile investment. The correspondence-forwarding plan was effective: Yerkes convinced Embree to support applications for his primate laboratory to the Rockefeller Foundation officers. 394 In fact, Embree was so impressed with Yerkes’s arguments about human engineering that he incorporated them into his plans for a Division of Studies “human biology” program.

When Yerkes and Embree began their correspondence, the Division of Studies was still in its planning stages. In February 1925, the Rockefeller Foundation officers met to discuss the program of the Division of Studies, and Embree presented his idea of a policy agenda designed around the field of “human biology.” The “human biology” program would include support for the “mental sciences” and eugenics, as well as

391 Yerkes to Embree, January 5, 1925, folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
392 Yerkes to John C. Merriam, President of the CIW, copy to Embree, January 19, 1925, folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
394 Embree to Rockefeller Foundation officers, January 20, 1925, folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
scientific research related to these fields. The program was a classic example of a human engineering effort, albeit a loosely defined one:

Attempts, often at present based on insufficient data, to grapple with practical problems of human biology are seen in current restrictive immigration [referring to the Immigration Act passed in April 1924], in laws for the segregation or sterilization of the unfit, and in revision of methods of correction and treatment of the delinquent and the dependent. The problem is to develop an adequate basis of well demonstrated fact upon which such new procedures may rest. 395

When the officers described projects that would be ideally suited for the human biology program, they integrated the plans that Angell and Yerkes had presented to Ruml and Embree:

Co-operation in building up a university institute for the study of fundamental problems of race biology, including comparative studies of ethnic groups and research in heredity, in the physiology of sex, of reproduction and of development, and in experimental evolution. . . . Support to proposals to concentrate strong departments in the mental sciences and in mental hygiene in university centers. 396

The plan for human biology at the Rockefeller Foundation was clearly influenced by the NRC Committees that Yerkes chaired and by the example of the Yale Institute of

396 Rockefeller Foundation Conference of Members and Officers, Princeton, NJ, February 23–24, 1925, folder 165, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
Psychology. Needless to say, when Yerkes applied for support shortly after this conference, he was received warmly by Embree.

In March 1925, Yerkes drafted a proposal for a primate research laboratory and sent it to Embree, Ruml, and Angell.\textsuperscript{397} As always, he highlighted the potential for anthropoid research to be applied directly to problems of human behavior. The long campaign for support finally paid dividends: in May, Yerkes received official notification from Embree of a grant for $10,000 a year for four years “for the promotion of anthropoid research at Yale University.”\textsuperscript{398} Over the next several years, Yerkes communicated with Embree on at least a monthly basis.\textsuperscript{399} He also happily informed Angell in June that “the Committee for Research on Sex Problems of the National Research Council, supported by funds from the Bureau of Social Hygiene, desires to establish in this institution a center for the study of psychological and psycho-biological problems of sex in infrahuman organisms,” and the primate laboratory accordingly received $7500 for the year 1925–1926 from the NRC Committee on Sex Problems (the Committee continued to support the laboratory for a total of $23,000 by 1928).\textsuperscript{400} Thus, by the end of 1925, Yerkes was receiving ample support for his primate research from several Rockefeller sources. He dedicated the monograph that he completed that year, \textit{Chimpanzee Intelligence}, to his patrons.\textsuperscript{401}

\textsuperscript{397} Yerkes, “Plan for Primate Research in Yale University,” March 11, 1925, folder 1094, box 57, Series II, Yerkes Papers, YA.
\textsuperscript{398} Yerkes to Embree, April 14, 1925, Thompson to Angell, May 28, 1925, folder 1094, box 57, Series II, Yerkes Papers, YA.
\textsuperscript{399} Folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{400} Yerkes to Angell, June 10, 1925, folder 1673, box 159, Record Group 2-A, James R. Angell Presidential Records, 1921–1937, YA; Wade Pickren, “Yerkes, Stone, and Programmatic Sex Research,” 612.
\textsuperscript{401} Robert Mearns Yerkes, \textit{Chimpanzee Intelligence} (Baltimore: Williams and Wilkins Company, 1925), 14.
From 1926 until 1928, Yerkes spent his time observing his beloved anthropoids and collecting information about primate research for an annotated bibliography. The research associates he gathered for his psychobiology group at Yale conducted studies on memory and sexual behavior in primates while Yerkes acted as a “science bureaucrat” (to use Wade Pickren’s term) by monitoring the researchers, calculating expenses, and recording results.  

In his reports to the Rockefeller officers, Yerkes focused on progress that his research group at the Yale Laboratory of Primate Biology, which was considered part of the Institute of Psychology, was making in terms of standardizing anthropometric measurements of chimpanzees. He promised that the measurement procedures would be “applicable to all anthropoid subjects and capable of extension to human subjects.”  

The organization of the laboratory and the lofty rhetoric to support it were the only successes of these early years, but Yerkes felt his dream was coming to fruition. Yerkes had solidified his relationships with Rockefeller officers, virtually guaranteeing that his interests would continue to receive funding.

While Yerkes concentrated on building his Primate Laboratory, Angell began to negotiate with Rockefeller Foundation officers about expanding the Institute of Psychology. Angell first wrote Embree and George Vincent, the president of the Foundation, in October 1926 to seek their opinions regarding an enlarged institute that would move beyond psychology to deal “with the fundamental problems of behavior” using an interdisciplinary approach.

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403 Report for the Institute of Psychology by Yerkes, 1926, folder 827, box 79, Subseries 6, Series III, Laura Spelman Rockefeller Memorial Archives, RAC.
404 Yerkes to Angell, May 21, 1927, folder 34, box 2, Series I, Yerkes Papers, YA.
405 Angell to Vincent and Embree, October 26, 1926, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
discussed the idea while Angell worked with Dean Winternitz of the Yale Medical School to refine the proposal. Rockefeller officers became increasingly involved in the planning process, and a human engineering agenda was incorporated into the plans as the new Institute began to take shape. In his memos to RF officers, Dean Winternitz argued that the proposed institute, which would address the “demand for individuals trained in the science of human behavior,” would fulfill the goals originally established by the social and mental hygiene movements. He explained that “mental hygiene has developed by demonstrating its fundamental importance and value for the practical problems of life,” but that the movement would stagnate without advances in psychiatric and psychological research. According to Winternitz and Angell, then, the proposed Institute of Human Behavior would become a capstone human engineering project where the goals established by the social and mental hygiene movements would be achieved by trained scientists.

The proposed Institute of Human Behavior seemed to be an ideal project for the human biology program advanced by Edwin Embree’s Division of Studies. Unfortunately for the Yale group, though, the Division of Studies was falling into disfavor among the Rockefeller Foundation officers by late 1926. Embree had allocated large sums to a few projects that were run by close friends, especially Raymond Pearl at Johns Hopkins, and the other Rockefeller officers had grown skeptical about Embree’s decision-making priorities. One dilemma was a close association between the recipients of Division of Studies grants and the eugenics movement. Despite earlier direct

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406 Vincent diary, October 26, 1927, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
407 Winternitz memo to Rockefeller Foundation, October 1927, p. 3, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
involvement in the movement, Foundation officers were distancing themselves from eugenics because it was becoming controversial and prominent geneticists were questioning its scientific validity (this will be discussed in detail in Chapter Four). Ultimately, Embree’s Division of Studies was dissolved in late 1927, when George Vincent and trustee Raymond B. Fosdick (who would become president of the Foundation in 1936) inaugurated a reorganization of the Rockefeller boards with the goal of promoting cutting-edge research programs.

Beginning in 1928, the Rockefeller Foundation was reconfigured to include a “complete unification” of the other Rockefeller philanthropies—International Education Board (IEB), General Education Board (GEB), and Laura Spelman Rockefeller Memorial (LSRM)—under a single board of trustees and group of officers. The reorganized Foundation was devoted entirely to the support of research in four divisions: Natural Science (including former IEB and GEB programs), Social Science (including former LSRM programs), Humanities, and Medical Science. With the absorption of the IEB, GEB, and LSRM, the Rockefeller Foundation endowment was greatly expanded. With $19,000,000 from the IEB, $39,500,000 from the GEB, and $63,000,000 from the LSRM, the new Foundation began the year 1928 with a total of $236,000,000 (equivalent to $2,517,000,000 in 2005) “devoted wholly to the production, diffusion, and application of basic scientific knowledge.” The reorganization required a massive reshuffling of personnel, leading both Ruml and Embree to resign. Grants for the Yale Institute of Psychology and Yerkes’s Primate Laboratory were moved from the LSRM and Division

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408 “Report of Committee on Appraisal and Plan,” December 11, 1934, folder 170, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
of Studies, respectively, to the Rockefeller Foundation Division of Natural Sciences. Angell and Yerkes could only hope that the individual selected to direct the Division of Natural Sciences would be as sympathetic to their interests as Ruml and Embree had been.

The reorganization of the Rockefeller Foundation had the potential to provide a windfall for existing projects like the Yale Institute of Psychology and Primate Laboratory. Yet, Yerkes and Angell had to contend with the unpredictability of new personnel and policy objectives. In applications for his primate laboratory, Yerkes had been masterful at linking his research interests with Embree's human biology program. Angell had been equally adept at constructing the case for the Institute of Psychology with Ruml at the LSRM. Both of them were hoping to expand their projects. Angell wanted to transform the Yale Institute of Psychology into the largest, most prestigious center for the study of the behavioral sciences in the country, while Yerkes hoped to find the resources necessary to turn his modest laboratory into a full-fledged experiment and breeding station for nonhuman primates. Further, Yerkes had spent most of his Rockefeller money already on multiple choice tests and anthropometric measurement tools, which he had used to assess four chimpanzees that he was raising in his New Haven Primate Laboratory. By the time of the 1928 Rockefeller Foundation reorganization, Yerkes was feeling constrained in terms of both space and finances. He had acquired a small grant from the Carnegie Institution of Washington (CIW) to survey tropical locations for a potential anthropoid breeding station. He hoped that he could

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411 "Report of the Work of the Institute of Psychology, Yale University, for the Academic Year 1926–1927," folder 828, box 79, Subseries 6, Series III. Laura Spelman Rockefeller Memorial, RAC.
convince Rockefeller Foundation officers to invest in the station by explaining his financial difficulties and also showing them the CIW survey results to demonstrate that a new breeding station was a viable plan.

Yerkes and Angell both felt that their prospects for additional support were excellent because they had established themselves within the foundation interlocking directorate. Angell had served as a trustee of the GEB since 1922 and, after the reorganization, he had become a trustee of the Rockefeller Foundation. In May 1928, when Foundation trustees had begun pursuing Max Mason, a former professor of mathematics at the University of Wisconsin who was serving as the acting president of the University of Chicago, to be director of the Division of Natural Sciences, Angell had been privy to the inside information. He had immediately notified Yerkes to tell him about Mason. Before Mason even arrived at the RF, Yerkes had contacted him about the anthropoid breeding and experiment station. On May 19, 1928, Yerkes wrote Angell,

I have, in pursuance of your suggestion, considered further with representatives of the Rockefeller Foundation possibility of further support of anthropoid research. . . . I am further assured that Dr. Mason will be prepared to discuss the situation with us “well in advance of the November meeting of the Board.”

This was quite a promise, considering that Mason was not scheduled to take office until October 1928, but Yerkes and Angell intended to hold him to it. As soon as Mason arrived to assume the directorship of the Division of Natural Sciences, he received a letter from Angell. Angell explained that he wanted to ensure that Mason knew about the anthropoid experiment and breeding station idea. In his typical style, Angell included

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412 Yerkes to Angell, May 19, 1928, folder 34, box 2, Series I, Yerkes Papers, YA.
casual comments about the perfect fit between Yerkes’s research and the goals of the Rockefeller Foundation:

I regard the providing of such a station as of the first consequence in promoting the solution of a great group of problems which have peculiar importance, both biologically and psychologically, in our generation. Dr. Yerkes is a peculiarly well qualified man to head up such an undertaking . . . May I say this whole question hangs together with the promotion of certain fundamental studies in genetics and in psychology which the Spelman Memorial, and other Rockefeller interests, have been promoting here at Yale and elsewhere, all of which I think are deserving of fuller support.\footnote{Angell to Mason, October 4, 1928, folder 2021, box 164, Series 220D, Record Group 1.1, Rockefeller Foundation Archives, RAC.}

In the letter, Angell managed to advocate for Yerkes’s interests, promote the Yale Institute, and suggest possible avenues for future RF intervention in the biological and behavioral sciences. He concluded by inviting Mason to Yale. Mason accepted.

On October 12, 1928, Mason met with Yerkes at Yale and took a tour of the Institute of Psychology and the Laboratory of Primate Biology. In his diary, Mason noted that the anthropoid experiment station plan had promise, but that he would rather it remain in New Haven instead of being moved to a southern location because “it seemed to me essential that the whole project be a unit and be close to the group in psychology now in existence.”\footnote{Max Mason diary, October 12, 1928, folder 2021, box 164, Series 220D, Record Group 1.1, Rockefeller Foundation Archives, RAC.} It was clear from Mason’s visit and response to the laboratory that he saw the anthropoid experiments as inseparable from the overall Yale behavioral sciences program at the Institute of Psychology. Yerkes realized that, to retain the
interest of the RF officers, he had to illustrate the value of his anthropoid research for the human engineering effort by connecting his work more explicitly with Angell's plan to expand the Institute of Psychology into an Institute of Human Behavior. When he wrote Mason on October 18, 1928 to apply for a five year, $325,000 grant, Yerkes focused on the human applications of his primate research. According to Yerkes, the station would allow for:

systematic, long-continued observation of one or more types of those primates which in every respect are more closely related to man; namely, the anthropoid apes. Such scientific establishment should render possible the utilization of at least one of the anthropoid types initially for the investigation of psychobiological problems, many of which cannot well be attacked with human subjects.\(^{415}\)

Yerkes urged Mason to sponsor a study by an advisory board of inquiry to determine the optimal location for the experiment and breeding station.

The Rockefeller Foundation officers were reluctant to act upon Yerkes's application until Angell's plans for expanding the Institute of Psychology were clarified. It seemed to all involved that the anthropoid breeding station would fit best with Foundation policy if it was explicitly connected to a larger project devoted to the analysis and improvement of human behavior. Throughout the month of November 1928, Angell negotiated with Lawrence K. Frank, the outgoing secretary of the Laura Spelman Rockefeller Memorial (LSRM), about a plan to integrate Yerkes's laboratory, the Psychiatry Department of the Yale Medical School, and the Institute of Psychology into a

\(^{415}\) Yerkes to Mason, October 18, 1928, folder 2021, box 164, Series 220D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
new Institute of Human Behavior. Frank was convinced that the Yale Institute of Human Behavior would be an ideal project to fulfill Rockefeller human engineering goals. He also embraced Yerkes's arguments about the connections between an anthropoid station and the proposed human behavior institute. As Frank explained to Rockefeller Foundation officers:

In the field of comparative psychology and of anthropoid life, it is recognized that many of the more baffling aspects of human behavior will probably receive illumination from a study of the lower organisms, particularly of the infra-human primates such as the chimpanzees. . . . Professor Yerkes has for a number of years been maturing plans looking to the organization of an anthropoid breeding station . . . The present application includes a request for funds to establish this breeding station and, upon recommendation for favorable action by the Directors of the Rockefeller Foundation, this request is being considered by the Memorial as part of the total Yale situation.

Frank went on to recommend an appropriation of $2,000,000. Amazingly, the Rockefeller Foundation officers not only approved the $2,000,000, which would be provided through LSRM endowment funds, but they also continued to discuss an even larger additional appropriation totaling $4,500,000 with Angell.

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416 Lawrence K. Frank to Angell, November 10, 1928, folder 828, box 79, Subseries 6, Series III, Laura Spelman Rockefeller Memorial Archives, RAC; Angell to Frank, November 12, 1928, folder 2021, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
417 Frank docket item for Rockefeller Foundation officers' meeting, "Yale University—Institute of Psychology," November 27, 1928, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
418 Ibid.
419 Angell memo to Rockefeller Foundation officers, December 20, 1928, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
On January 22, 1929, the Rockefeller Foundation officially approved an appropriation of $2,000,000 for "(1) certain specified parts of the program of a proposed Institute of Human Behavior, and (2) a program of anthropoid breeding and research" as well as $2,500,000 for an Institute building and psychiatric research. The RF grants were in addition to a $2,000,000 grant from the LSRM for "psychobiological research," for a total of $6,500,000 (equivalent to over $70,000,000 in 2005) for the Yale behavioral sciences. By any measure, this was a staggering sum, larger than many college endowments at the time. The appropriation even included a supplement of $25,000 "to defray the expenses of a study by a committee of experts of the feasibility of establishing an anthropoid breeding station." The willingness of Rockefeller officers to commit so wholeheartedly to the Yale programs was mainly the result of interrelated factors that exemplified foundation support for university research, especially in the behavioral sciences, in the 1920s. First, the Yale programs were designed, organized, and run by scientific entrepreneurs who were leaders in their disciplines and were firmly enmeshed in the foundation interlocking directorate. Second, the Yale Institute of Human Behavior and affiliated Anthropoid Breeding and Experiment Station were to be cooperative enterprises, in which the scientists involved moved outside of their disciplinary boundaries to address human behavior as a psychological, psychiatric, social, and physiological issue. Third, the Yale programs appeared to fulfill all of the human engineering goals that the foundation officers had been pursuing for years.

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420 Grant Summary, 1/22/1929, folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
421 See also "$7,500,000 Yale Fund for Study of Man," New York Times (15 February 1929), sec.1, pp. 1, 12.
422 Grant Summary, 1/22/1929, folder 2020, box 164, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
The appropriation gave Yerkes the chance to make his almost lifelong dream a reality. He threw every ounce of his energy into the project. Yerkes secured $9000 from the Carnegie Institution of Washington (CIW) to finance a trip to the Belgian Congo by Harold Bingham, with the goal of viewing chimpanzees and gorillas in a natural habitat that could then be replicated in the experiment station.\footnote{Yerkes to Angell, February 11, 1929, folder 1675, box 160, Record Group 2-A, James R. Angell Presidential Records, 1921–1937, YA.} He began a frequent correspondence with the “committee of experts” that the Rockefeller Foundation (per Yerkes’s request) put together to assess the feasibility of the anthropoid station. Several of the committee members were Yerkes’s close associates who had served with him on the advisory board of the American Eugenics Society, on NRC Committees and on the Carnegie Institution of Washington (CIW) “Physical Basis of Human Behavior” conference committee, including chair E. G. Conklin (Professor of Zoology, Princeton University), John C. Merriam (president of CIW), and Clark Wissler (Professor of Anthropology and Psychology, Yale University).\footnote{Report of Advisory Council of the American Eugenics Society, 1926, folder 1108, box 109, Record Group 2-A, James R. Angell Presidential Records, 1921–1937, YA.} The committee worked tirelessly for the station, conducting detailed inquiries into the health risks and relative degree of “normality” that could be achieved for the apes.\footnote{“Informational Memorandum Concerning Feasibility of Plan for Establishment of Anthropoid Breeding and Observation Station,” February 12, 1929, folder 2022, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation, RAC.} With Yerkes’s guidance, the committee settled on a 200 acre plot in Orange Park, Florida as the best location for the station, and the site was purchased in May 1929.

As Yerkes worked on his anthropoid station, the plans for the Institute of Human Behavior continued to develop. As soon as the appropriation was made, Angell and the deans of the Graduate School and Medical School decided to change the name from...
“Institute of Human Behavior” to “Institute of Human Relations,” with the intent of making the emphasis on interdisciplinary research more apparent.\footnote{Mark May, \textit{Toward a Science of Behavior}, 42.} Yerkes was enthusiastic about the project, and wanted to ensure that his laboratories were considered an important part of the enlarged Institute. When he described developments at the anthropoid station to Angell, Yerkes stated that “every effort . . . should be made to integrate the various lines of work . . . to relate properly the whole to other major interests and activities of the Institute of Human Relations.”\footnote{Yerkes to Angell, March 15, 1929, folder 1675, box 160, Record Group 2-A, James R. Angell Presidential Records, 1921–1937, YA.} Yerkes continued along these lines when communicating with the committee of experts organized by the Rockefeller Foundation: “As matters now stand, the academic center for anthropoid research is thought of as a portion of the Institute of Human Relations.”\footnote{“Informational Memorandum on Location and Development of Anthropoid Breeding and Observation Station,” April 6, 1929, folder 222, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation, RAC.} Throughout the year of 1929, Yerkes joined Angell in the mission to promote the Institute of Human Relations (IHR) as a research center devoted to the improvement of mankind through interdisciplinary, cooperative scientific work.\footnote{James R. Angell, “Yale’s Institute of Human Relations,” \textit{Yale Alumni Weekly} (April 19, 1929): 4–8.} As Angell worked with Dean Winternitz of the Medical School to develop the IHR, Yerkes continued to refer to his Anthropoid Station and Primate Laboratory as psychobiology experiment centers that would produce results directly applicable to man.\footnote{Yerkes to Mason, January 28, 1930, folder 2023, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.} In 1930, as construction on both the anthropoid station and the IHR began, everyone seemed to be satisfied that the projects would help to achieve the Rockefeller Foundation human engineering goals while building an outstanding interdisciplinary program to investigate and control human behavior.
The early years of the Institute of Human Relations (IHR) and the Anthropoid Experiment Station were devoted to organization and construction. The members of the Institute of Psychology became the core group at the new Institute of Human Relations, and Yerkes played a leading role there in the early 1930s. The psychologists were joined by an enlarged and strengthened Department of Psychiatry and Mental Hygiene, the Yale Psycho-Clinic and Child Development Center (which received additional Rockefeller funding), and by a smattering of social scientists led by prestigious anthropologists Clark Wissler and Edward Sapir.\textsuperscript{431} As the scientists put together their research plans and the Yale administrators supervised the construction of buildings, Angell devoted his energy to promoting the idea of interdisciplinary cooperation at the IHR. He repeatedly emphasized that the breaking down of disciplinary boundaries was a chief concern:

\begin{quote}
The Institute of Human Relations is so set up as to bring together in fruitful voluntary cooperation specialists in all the branches of work related to these cardinal issues in human life. . . . That it will have immediate and highly valuable results in breaking down departmental lines of isolation and exclusion, which have been so damaging to scientific progress in our universities, cannot be doubted. Nor is it possible that the plan shall fail wholly, for it is based upon the integration of a number of units, each of which is, in its own right, highly important, and even if the cooperative research and training program of the organization largely failed, each of these subordinate units would be producing work of indispensable significance. . . . Complete failure is impossible.\textsuperscript{432}
\end{quote}

\textsuperscript{431} Mark May, \textit{Toward a Science of Human Behavior}, 50–51.

In this report, Angell made an argument that was as common as it was paradoxical. On one hand, he criticized the deleterious effects of departmental segmentation on the university and on "progress." On the other hand, he credited the discrete "units" in the university with pursuing promising research "of indispensable significance," a feat that surely required the specialization that a department structure encouraged.

Like many others before and after them, Angell and the foundation officers who echoed his message both celebrated and attacked departmental specialization. From the time scientists began to organize disciplines and university departments to protect the community of the competent, university administrators, scholars, and foundation officers had decried the limitations of over-specialization. As George E. Vincent, speaking on behalf of the Rockefeller Foundation, announced at the opening ceremonies for the Institute of Human Relations (IHR):

Problems have an annoying way of ignoring the conventional academic departments. Large numbers of the hardest questions seem to hang about on the boundary lines... Cooperation is a safeguard against departmental bias... It is of the highest importance that scientists should meet frequently on the frontiers.\footnote{George E. Vincent, "The Institute of Human Relations and the Broadening Field of Public Health," Opening Ceremonies Address, May 9, 1931, folder 807, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.}

The stated goal was to transcend disciplinary and departmental boundaries while also supporting path-breaking research. Yet, high quality research required a system of training, organization, and professional standardization that only departments and disciplines could provide.

Both the scientists of the Institute of Human Relations (IHR) and the officers of the Rockefeller Foundation tried to negotiate a balance between specialized research and
interdisciplinary cooperation throughout the early 1930s. As was often the case, Foundation officers launched policy initiatives that followed the same principles as university programs that received substantial funding. In the early 1930s, Foundation officers articulated an interdisciplinary plan for the various divisions, led by the Division of Natural Sciences, that looked strikingly similar to Angell’s descriptions of the IHR. In August 1930, Max Mason had been promoted to president of the Rockefeller Foundation and he had decided that his primary objective would be to create interdisciplinary cooperation within the Rockefeller Foundation by bringing the various divisions together to achieve the goal of improving mankind through research. At an officer and trustee conference in October 1930, Mason suggested that the Foundation officers design a comprehensive, interdisciplinary program to coordinate the social sciences, medical sciences, and natural sciences in an effort to investigate “mental health and personality.”

Mason chose the name “psychobiology” for his policy initiative, the term that Yerkes and Angell had used to describe the Yale behavioral science programs. At the time Mason adopted the term, “psychobiology” was a field devoted almost entirely to human engineering. Psychobiologists defined their field based on four assumptions. First, physiology was viewed as intimately connected to behavior. Second, mind was seen as an evolutionary adaptation with one main function: to help humans adjust to their environmental conditions. Third, evolution was understood to be cumulative and progressive. Fourth, it was assumed that evolutionary progress could be controlled more effectively through behavioral modification than through selective breeding or

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434 "The Rockefeller Foundation: A Brief Summary of the Conferences of Trustees and Officers at Princeton," October 1930, p. 5a, folder 166, box 22, Series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
Since these assumptions formed the basis for Mason’s psychobiology initiative, he was essentially building his program on the same combination of human engineering ideas that had provided rationales for the Yale behavioral science projects.

In the first few years of the Institute of Human Relations (IHR) and Anthropoid Experiment Station, then, the goals of the Rockefeller Foundation and Yale University coalesced and the phase of the human engineering effort devoted to the investigation and control of human behavior went into full operation. For Angell and Yerkes, the years spent proclaiming the value of research in the behavioral sciences had yielded plenitude and legitimization for their work. There was a catch, though. Both men had set up nearly impossible expectations for the Yale projects. The IHR was hardly a model of interdisciplinary, cooperative research in the early 1930s. It was run by a six member executive committee chaired by Angell that had no authority over actual research projects. IHR members were free to come and go as they pleased, without any commitments to the group as a whole. Naturally, they focused on their own research projects rather than setting aside their work to contribute to an ill-defined interdisciplinary goal. Transcending disciplines and departments was not so easy after all.

At the Anthropoid Experiment Station, Yerkes struggled to fulfill the lofty promises that he had made to the Rockefeller Foundation. He repeatedly had to reassure Foundation officers that research at his station would contribute to the effort to investigate and control human behavior. Yerkes continued to characterize his work in terms that fit with human engineering goals, as he wrote to Foundation officers in 1932:

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436 Mark May, *Toward a Science of Behavior*, 56.
"I firmly believe today, as ever, that comparative method and infrahuman organisms may and will be made to contribute increasingly and importantly to the solution of a multitude of pressing human problems."\textsuperscript{437} He went on to explain that his laboratories represented "teamwork and an exceptional degree of cooperativeness."\textsuperscript{438} Yet, in actuality, all of his time was devoted to purchasing, breeding, and caring for his chimpanzees. The research program was virtually nonexistent in the early 1930s.

As IHR members neglected interdisciplinary projects and Yerkes doted on his chimpanzees, Max Mason took steps to reinvigorate the Division of Natural Sciences program at the Rockefeller Foundation by recruiting his top mathematics student from the University of Wisconsin, Warren Weaver, to take over as director. Weaver arrived at the RF with a solemn appreciation for the power that came with his position. In Weaver's words:

The free funds for research which were under the control of the Rockefeller Foundation in the year 1932 constituted a very substantial fraction of the free funds available for research in the whole United States. . . . One could have a great intellectual leverage with a relatively small sum.\textsuperscript{439}

Weaver's assessment was correct. In the 1930s, the Rockefeller Foundation supplied one-third of all foundation support for university research and three-quarters of all natural science research funds in the United States.\textsuperscript{440} Moreover, when Weaver accepted the Director of the Division of Natural Sciences position, the nation was in the throes of the

\textsuperscript{437} Robert M. Yerkes, "Robert Mearns Yerkes, Psychobiologist," 404.
\textsuperscript{438} Robert M. Yerkes, "Yale Laboratories of Comparative Psychobiology," \textit{Comparative Psychology Monographs}, 8:3 (February 1932), 14.
\textsuperscript{439} Warren Weaver, Columbia University Oral History Project, Volume Two, April 6, 1961, pp. 283–284, Columbia.
Great Depression. The Rockefeller Foundation’s sizeable resources had to be distributed wisely and judiciously in a time when the future returns on endowment income were uncertain. These issues understandably filled Weaver with trepidation.

Weaver accepted the offer despite misgivings about the enormity of his responsibilities because he felt indebted to Mason. At the University of Wisconsin, where Weaver had earned his B.S. in 1917 and his Ph.D. in 1921, Mason had served as an advisor and mentor. Though Weaver had departed from Wisconsin from 1917 to 1920 to teach at Throop Polytechnic Institute (which later became the California Institute of Technology), Mason had easily lured him back to Wisconsin. From 1920 until 1925, when Mason left Wisconsin to become the president of the University of Chicago, the two men worked side-by-side on a number of projects. They had maintained frequent contact as Weaver had progressed from assistant professor to full professor and chair of the Department of Mathematics at Wisconsin, a position that he had assumed in 1928. As a result of their close partnership and the tremendous respect he held for Mason, Weaver felt that he could not refuse the offer to become Director of Natural Sciences, even though he felt woefully unprepared.

Weaver spent his first year at the Rockefeller Foundation studying past policies and visiting elite universities throughout the United States and Europe to learn more about the latest scientific developments. In these laboratories, investigators were conducting genetic crossing experiments with *Drosophila* and guinea pigs, using electron microscopes to analyze cellular structure, and immersing growing plant and animal embryos in volatile solutions to assess the importance of environment on development.

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He quickly determined that the future for research contributing to the improvement of mankind lay in the biological sciences. As Weaver recalled,

Satisfied as I was being immersed in the physical sciences, I was convinced that the great wave of the future in science . . . was to occur in the biological sciences. The startling visions were just then beginning to open up in genetics, in cellular physiology, in biochemistry, in developmental mechanics—these were due for tremendously significant advances.\textsuperscript{442}

In addition to his decision to focus on building up the biological sciences, Weaver promised to maintain the Foundation’s emphasis on supporting elite universities.\textsuperscript{443} Therefore, by 1933, the central precepts of Weaver’s Natural Sciences program were in place: an emphasis on the biological sciences and a concentration on elite universities with established laboratories led by reputable scientists.

The program that Weaver developed over the course of 1933 completely overhauled the human engineering effort. His new program marked the beginning of another new phase of human engineering, in which the investigation and control of human behavior would be eclipsed by the investigation and control of the “vital processes” that affected human life. The “vital processes” program that Weaver designed will be discussed in great detail in Chapters Four and Five. What is critical to understand at this point with regard to the Yale behavioral sciences is that Weaver was unsure how to fit psychology, “psychobiology” and primatology into his vision for the human engineering effort. He made sure that psychobiology received ample consideration, both because the Yale psychobiology programs were receiving such enormous RF grant.

\textsuperscript{442} Warren Weaver, \textit{Scene of Change}, 59.

\textsuperscript{443} Outline of the Natural Science program by Mason and Weaver for a staff conference, October 18, 1932, folder 1, box 1, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
allotments and because Mason had made the field a top priority. Still, the fit between psychobiology and Weaver's conception of human engineering was awkward at best. As Weaver tried to explain how psychobiology was a part of his agenda, it was obvious that he was straining:

Psychobiology is included here for two sorts of reasons. Recent experimental work, of the most accurate and refined sort, on electrical phenomena associated with the conduction of nerve impulses as well as many other experimental procedures, make the attack on central problems of psychology more promising than before. Furthermore, the formation of thinking and learning patterns and the conditioning of the stream of consciousness by physicochemical factors are truly vital processes.444

As a mathematician and physicist, Weaver had difficulty comprehending the field of "psychobiology." His understanding of psychobiology was a far cry from the types of research that Yerkes and his colleagues claimed they were conducting. The scientists at the Institute of Human Relations (IHR) and at Yerkes' primate laboratories were not focusing on "electrical phenomena," "the conduction of nerve impulses," or the effects of "physicochemical factors" on thinking, learning or consciousness. They were trying to study and modify behavior.

Weaver made persistent attempts to include some version of psychobiology in his reformed human engineering agenda, but his knowledge of the field was minimal compared with his growing familiarity with the latest developments in genetics, embryology, endocrinology and biochemistry. Although he averred that "hope for the

444 "Natural Sciences—Proposed Program" by Weaver, April 11, 1933, pp. 78–79, box 1, folder 6, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
future of mankind depends in a basic way on the development in the next fifty years of a new biology and a new psychology;” his grant allotments showed an overt preference for the “new biology” over the “new psychology.”445 Part of the problem with continuing to emphasize psychobiology was that the pilot programs at Yale were disappointing. Weaver wanted quantitative, verifiable evidence that the existing Rockefeller Foundation appropriations that he was charged with administering were yielding results. He certainly was not going to supplement existing appropriations without some proof that the original funding was being put to good use. His emphasis on quantitative results did not bode well for Yerkes or for the IHR.446 Weaver’s discomfort with the Yale brand of psychobiology was present from the outset, and his uneasy feelings only grew more pronounced as he learned more about the programs.

On March 13, 1934, Yerkes wrote Weaver about an urgent need for supplemental funds to build a chimpanzee maternity ward at the Anthropoid Station.447 Despite Yerkes’s use of terms like “desperate” and “emergency” to describe his situation, Weaver was less than impressed. In May, Weaver went to visit the station, and he saw nothing that convinced him of the value of Yerkes’s project.448 After his visit, Weaver wrote several psychobiologists to inquire about the future of the field. When Hudson Hoagland of Clark University responded with the statement that “the subject of behavior must inevitably be the province of the trained physiologist” because “only by a reasonably rigorous training in the physical and biological sciences can one hope to qualify for effective research in this field,” Weaver agreed enthusiastically and responded that

445 Ibid.
446 Donna Haraway, Simians, Cyborgs, and Women, 58.
447 Yerkes to Weaver, March 13, 1934, folder 2023, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
448 Yerkes to Angell, May 9, 1934, folder 35, box 2, Series I, Yerkes Papers, YA.
Foundation funds should go to psychobiology only if it was tied to biochemistry, biophysics, genetics, and physiology.\textsuperscript{449} The shift toward human engineering through research in the biological sciences had begun, and Yerkes would be left behind.

By 1935, internal Rockefeller Foundation memos were regularly describing disenchantment with the Yale programs. Foundation officers complained that "there is no clearly-bound lead idea which is scientifically sound and there is no plan for [the] working out of such an idea" at either the Institute of Human Relations (IHR) or Yerkes's laboratories.\textsuperscript{450} For Angell and the IHR staff, the Foundation officers' concerns were not pressing because the existing appropriations were more than enough to continue building the program. Yerkes, on the other hand, requested additional funds on a regular basis. In response to Yerkes's ceaseless appeals, Weaver asked Lawrence K. Frank, who was serving as a consultant to the Foundation, to visit the Anthropoid Station and report on the viability of Yerkes's research. Frank's report confirmed Weaver's worst suspicions:

\begin{quote}
In general, the present use being made of the Station is so small . . . that some plan of reorganization would seem highly desirable. It is evident, however, that Yerkes wishes to maintain complete control . . . In the writer's opinion, any request for further financing should be made the occasion for a critical examination of the present organization and administration with a view to
\end{quote}

\textsuperscript{449} Hudson Hoagland to Weaver, October 30, 1934; Weaver to Hoagland, November 5, 1934; folder 37, box 4, Series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{450} "Some Comments on the Organization of the Institute of the Institute of Human Relations," July 1934, folder 808, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
obtaining a wider use of the Station to justify the present capital investment and maintenance costs.\textsuperscript{451}

After reviewing Frank’s report, Weaver decided to visit the Station himself the following month. Weaver returned confused and frustrated by his conversations with Yerkes.

During his visit, Weaver had explained to Yerkes that, since the Foundation was already contributing huge sums to the IHR, it would be difficult to add to the appropriation.\textsuperscript{452} Yerkes had replied “very vigorously” that the Station was independent from the IHR and that it should therefore receive separate funding. In stark contrast to earlier statements that had justified his anthropoid research as part of an interdisciplinary IHR program, Yerkes was now trying to distance himself from the Institute. The two men had an exchange that left Weaver feeling chagrined:

At various times in the discussion Y [Yerkes] emphasizes that this recommendation and expansion of program is vital in order to make proper and effective use of his own ability and experience. . . . WW [Weaver] would have supposed that the natural and desirable thing was that the chimpanzee material be furnished for the researches of [the] group under the general cooperative spirit which was supposed to be characteristic of the whole Institute of Human Relations plan. Y states that this is impossible.\textsuperscript{453}

The conversation confirmed what Weaver had feared for some time: Yerkes was stubborn, his researchers were working in isolation, and the Anthropoid Station was

\textsuperscript{451} Memo from Frank to Foundation officers, re: visit to Yale Anthropoid Experiment Station, February 9–16, 1935, folder 204, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{452} Weaver diary, March 30–31, 1935, folder 204, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{453} Ibid.
contributing almost nothing toward the new human engineering goals that the Division of Natural Science was promoting.

For Yerkes, Weaver’s visit was alarming. He wrote Angell to complain that the Foundation was treating him unfairly. In response, Angell spent the next few months acting as a mediator with Weaver to try to smooth things over for Yerkes. Meanwhile, Angell was also having to defend the Institute of Human Relations (IHR) to Rockefeller Foundation officers. Beginning in October 1935, the Foundation began to “crack down” on the IHR. After a visit to Yale, Alan Gregg (Director of the Rockefeller Foundation Division of Medical Sciences) grumbled that “there has apparently never been a clear and disseminated decision of just what the Institute per se existed for or was going to do in point of program” and, as a result, it was producing “no ideas of surpassing importance.” Further, the IHR was unable to transcend departmental or disciplinary boundaries in any meaningful way. Mark May, the secretary of the IHR Executive Committee, admitted that Angell had “sold the Rockefeller Foundation a bill of goods,” and that “there is a real antithesis of interest between certain orthodox departments and the interests of the Institute” that make the interdisciplinary objectives almost impossible to achieve. Yerkes was privy to these discussions, and he made sure to pull Gregg aside. According to Gregg’s notes, Yerkes “points out that he is not directly tied up with the IHR. (May believes that Yerkes considers RF officers skeptical of the Institute and wishes to dissociate his primate work from the IHR).”

454 Yerkes to Angell, April 1, 1935; Angell to Yerkes, April 5, 1935; Angell to Yerkes, April 22, 1935; folder 36, box 2, Series I, Yerkes Papers, YA.
455 Alan Gregg diary, October 7–12, 1935, re: visit to IHR, folder 808, box 67, Series 200, Record Group 1.1, Rockefeller Foundation Archives, RAC.
456 Ibid.
457 Ibid.
Rather than participating in the reorganization of the IHR, which May initiated in late 1935 in response to Rockefeller criticism, Yerkes chose to distance himself from his colleagues at Yale. He repeated the same arguments that he had always used to justify his Station, with a particular focus on the ability to manipulate and control chimpanzees in a laboratory as a model for the modification of human behavior, but he withdrew any mention of connections with the IHR. Yerkes refused to adapt or to learn from others; he remained isolated and wedded to his long-held ideas about psychobiology as “the construction of a comparative science of personalities enforcing proper relations of mind and body.”

He continued to promise that his chimpanzees could be “standardized, controlled, and produced to specification,” which would make them “peculiarly and uniquely serviceable for those inquiries in which it is desired to approach as nearly as possible to the solution of specifically human problems.”

To Yerkes, the breeding and observation of the chimpanzees was intrinsically valuable. His multiple-choice tests and behavioral modification exercises would supply useful information for human society. He was establishing norms and enforcing them with his chimpanzees: having them eat with utensils at tables, coupling them with monogamous partners, teaching them basic sign language. The experiments would serve as models for modifying human behavior. The advantages of these practices seemed obvious to Yerkes, who was operating according to a human engineering paradigm that was fast becoming obsolete. To Weaver and his fellow Foundation officers, Yerkes’s work lacked innovation and was extremely limited in its application or connection to the developments in other fields. The assumptions behind his anthropoid program were

458 Donna Haraway, Primate Visions, 66.
relics of psychobiology ideas from the previous decade, which were informed more by behaviorism than by the latest developments in the life sciences.

In an effort to recapture the confidence of the Rockefeller Foundation, Yerkes asked Angell for assistance. Angell was aware that Weaver wanted Yerkes to make his chimpanzees readily available as research subjects for investigations in genetics, physiology and biochemistry. To plea Yerkes's case, Angell circumvented Weaver and wrote to Raymond Fosdick, who had taken over as Foundation president in 1936:

Apart from the extraordinary mass of useful and significant experimentation which has been conducted [at the Anthropoid Experiment Station] . . . in the general fields of psychobiology, sex and genetics, and neurophysiology, the animals have been put at the disposition of sundry scientists covering a very considerable range of investigatory interests.460

As soon as Fosdick showed him the letter, Weaver declared Angell's statement to be patently false. Weaver's multiple visits and discussions with Yerkes had made it clear that the Anthropoid Experiment Station was closed to outside investigators, and there was no evidence that any of the chimpanzees had been made available for anything other than Yerkes's behavior research. The Foundation officers agreed that Yerkes's applications should be denied. Still, Yerkes refused to give up and maintained his unrelenting campaign for supplemental funds.461

As Yerkes persisted in his applications for aid, the discussion about the Anthropoid Station among Foundation officers deteriorated into personal insults bordering on mockery. Weaver's diary notes in early 1937 were uniformly critical of

460 Angell to Fosdick, October 23, 1936, folder 36, box 2, Series 1, Yerkes Papers, YA.
461 Officers' Meeting notes, November 9, 1936, folder 2024, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Yerkes. "Y[erkes] works on chimpanzees not on problems," Weaver noted, "The problems Y[erkes] does work on are all very well provided one's goal is to find out all about chimpanzees."\textsuperscript{462} The best researchers had left the Yerkes's laboratory for other positions, which was "tragic evidence of Y[erkes]'s inability to get along with people—even to treat people fairly."\textsuperscript{463} After Lawrence K. Frank returned for another visit to the Station, Weaver reported, "LKF thinks that any discussion of this project must begin with the recognition of the fact that Yerkes is mentally ill."\textsuperscript{464} Furthermore,

LKF strongly resents the time, space, and energy utilized on Y's old-fashioned and relatively uninteresting multiple choice and puzzle experiments, and thinks that the director of the station should have much more scientific interests, including biochemical and physiological interests.\textsuperscript{465}

The officers were in accord that the future for improving mankind involved research in the fields of biochemistry, physiology, genetics, and molecular biology. The reason that the Rockefeller Foundation officers had devoted resources to scientific research was the hope for innovation, for pioneering work that could further human progress. To them, Yerkes was just playing with chimpanzees.

In his exchanges with Angell in the spring of 1937, Yerkes expressed his frustration with Rockefeller Foundation officers:

New York [the Foundation] is thoroughly poisoned against the enterprise [anthropoid station]. There, within a few months, it has changed from white to

\textsuperscript{462} Weaver diary, January 3, 1937; Weaver diary, January 8, 1937; folder 2025, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{463} Weaver diary, January 14, 1937, folder 2025, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{464} Weaver diary, February 17, 1937, folder 2025, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{465} Weaver diary, February 17, 1937, folder 2025, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
black. . . My confidence in the wisdom of Foundation procedure is shaken . . . It is, I confess, not a little humiliating to be treated by the Foundation as though I were either a fool or knave . . . I have literally exhausted my ingenuity in attempting to work constructively with Dr. Weaver in this situation. Angell responded sympathetically: "The real difficulty, I am sure, is that Dr. Weaver, being himself definitely biased, has sought opinions that were almost certain to be unfavorable." Both Yerkes and Angell were forced to recognize that they no longer had personal or professional influence over Foundation officers, particularly Weaver. As a scientist-administrator, Weaver wanted to see results rather than hear empty promises. Moreover, as Angell knew, Yerkes had made a serious tactical error by divorcing his project from the IHR. Angell retired later that year, and Yerkes was left to fend for himself with Weaver.

In sharp contrast to Yerkes, Mark May, who had been appointed as the first director of the Institute of Human Relations (IHR) in late 1936, was heeding Rockefeller advice. May abandoned the notion that the IHR would bring together the Psychiatry Department and Psycho-clinic from the Medical School with psychology and the social sciences from the Graduate School. Beginning in 1937, the IHR was solely devoted to the study of human behavior through specific interdisciplinary research projects coordinated by the director and conducted by all Institute members. The Psycho-Clinic and the Anthropoid Experiment Station officially became independent entities and the psychiatry department returned to the Medical School.

466 Yerkes to Angell, May 7, 1937, folder 37, box 2, Series I, Yerkes Papers, YA.
467 Angell to Yerkes, May 15, 1937, folder 37, box 2, Series I, Yerkes Papers, YA.
468 Mark May, Toward a Science of Human Behavior, 65.
469 John Mills, Control, 163–164.
Although the Rockefeller Foundation officers were somewhat disappointed that
the far-reaching goal of coordinating an interdisciplinary science of man had not been
achieved at the Institute of Human Relations (IHR), they were very pleased to see that
May had reformed the program based on realistic expectations. On April 6, 1938, the
Foundation announced a final appropriation to the IHR for $700,000 over a ten year
period. In an unusual move, the grant was considered outside the purview of a specific
division. In other words, Weaver was not directly involved. Rather, the Foundation
officers as a group (including President Fosdick and Vice President Thomas B. Appleget)
secured the grant as a kind of reward for May’s willingness to reform the IHR. These
officers acknowledged that the IHR was a remnant of an earlier period of human
engineering, but they defended the appropriation by explaining that,

The study of human behavior remains a subject of importance regardless of the
frustrated ambitions of those who invited in 1929 both large support and
considerable attention to an Institute of Human Relations which would undertake
such study. Nine years’ experience has convinced the Yale authorities that the
purpose of the Institute can better be served by the selection and support of
projects which involve the collaboration of scholars from different fields holding
posts in the University than by maintenance of a separate institution with an
indefinably extensive set of interests and objectives. The Rockefeller Foundation officers and the leaders of the IHR had finally accepted the
value of university departments, even though they noted that there was still hope for “an

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470 Grant summary, April 6, 1938, folder 804, box 67, Series 200, Record Group 1.1, Rockefeller
Foundation Archives, RAC.
471 Ibid.
472 Ibid.
integrated, synthetic science, cooperatively managed and oriented to eventual practical applications." With this final grant, the Foundation officers removed the IHR from Weaver's list of responsibilities and closed the chapter of the human engineering project devoted to the psychological investigation and control of human behavior.

Yerkes heard about the $700,000 appropriation for the IHR and was flabbergasted. He had thought that the Rockefeller Foundation was scaling back on new appropriations. The information about the IHR revived his commitment to securing additional funds for the Anthropoid Station and primate laboratories. This time he took a different approach, and even offered to step aside as the director of the station if that would improve his chances. It did. Weaver began entertaining the possibility of renewing support for the station, provided that Yerkes retire and the station be made available for researchers working in the biological science fields that the Division of Natural Sciences was encouraging by this point. In a note to his fellow officers, Weaver suggested a final, tapering grant in which he spelled out these conditions:

The emphasis should be shifted away from the present naturalistic and old-fashioned psychological basis to those physiological, neurological, biochemical . . . problems for which the chimpanzee is uniquely important. There is little hope for this change of emphasis under the present leadership.  

Again, Weaver was highlighting the shift that had occurred within the Foundation and in the scientific community at large during Yerkes's career. Human engineering through the investigation and control of behavior had given way to the investigation and control

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474 Warren Weaver, Docket Item for April Officers' Meeting, March 15, 1939, folder 2025, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
of "vital processes" through biological science research. The shift was incorporated into the final grant that Weaver secured for Yale primate research in 1939, which included a five year, $189,000 grant and a $35,000 grant for a new building for the physiological study of the chimpanzees.\(^{475}\) The grant stipulated that funds would be used for physiological and biological research rather than behavior studies. With the final grant, Weaver washed his hands of the primate business and returned to his focus on building programs in the biological sciences. He saw the whole Yale fiasco as part of the old Natural Sciences program that he had spent his tenure as director reforming and modernizing.

Yerkes retired in 1942 and spent the next year writing his magnum opus, *Chimpanzees: A Laboratory Colony*.\(^{476}\) It was more of a naturalistic journal than a scientific research paper, filled with observations about the daily activities and behavioral characteristics of his chimpanzees. By this time, it was clear that the human engineering project that Yerkes had pursued in his psychobiology research was outmoded.\(^{477}\) The rejection of Yerkes's version of psychobiology in favor of genetics, biochemistry, and molecular biology by Weaver both reflected and contributed to the transformation of university scientific research in the behavioral and biological sciences in the mid to late 1930s. Foundation intervention in the behavioral sciences was the most significant experiment of the 1920s; intervention in the "hard" biological sciences, especially in fields that used the techniques and instrumentation of the physical sciences, would be the most significant experiment of the 1930s. The human engineering effort and the quest to

\(^{475}\) Grant summary, January 20, 1939, folder 2027, box 165, Series 200D, Record Group 1.1, Rockefeller Foundation, RAC.


improve mankind would continue, but the scientific research projects that received the
bulk of Rockefeller Foundation support in the 1930s would be conducted by biologists
who would investigate the body on a molecular and chemical level rather than by
psychologists who observed behavior in order to modify it. It is to those projects that we
will turn in the next two chapters.
The behavior of man is conditioned biologically by such factors as heredity, nutrition, endocrine balance, neurological integration, and other elements of the body’s internal economy. Each of these is a subject of intricate complexity, but fortunately all animal life embodies similar or related mechanisms, and thus the experimental method becomes available to biology. . . . In a deeper sense, then, [Weaver’s] program is new. For, although man remains the measure of all the ends that are being sought, the road toward these goals is determined, not by its classification as human biology, but by its ability to contribute fundamental knowledge of the vital processes which underlie human biology.—Raymond B. Fosdick, President of the Rockefeller Foundation (1936–1948) 478

Chapter Four
Foundation Intervention in the Biological Sciences

Over the course of the 1920s and 1930s, philanthropic foundations became increasingly involved in the support of the biological sciences. For both Carnegie and Rockefeller philanthropic boards, funding for the biological sciences in the 1920s essentially meant funding for eugenics. The emphasis on eugenics was less about policy than about the fact that grant recipients had close relationships with foundation officers. Dependence on biologists who supported eugenics tied foundations to a movement that was fast becoming scientifically obsolete by the late 1920s. The rejection of eugenics by leading scientists led foundation officers, particularly representatives from the Rockefeller Foundation, to re-evaluate their understanding of biology and to adjust their approach to the human engineering effort.

Officers from the Carnegie Institution of Washington (CIW) responded very differently from Rockefeller Foundation officers to the backlash against eugenics. While CIW officers continued to finance eugenics research, thus contributing to an outmoded scientific program, Rockefeller Foundation officers overhauled their policies and endeavored to update their biological science programs based on current scientific trends. When Warren Weaver took over as the director of the Rockefeller Foundation Division

of Natural Sciences in 1932, he decisively broke away from earlier biological science policies and released the human engineering effort from the specter of eugenics. He dedicated his policy initiatives to supporting the latest developments in biological sciences at major universities. In a series of fact-finding missions, Weaver discovered that biologists were pursuing questions about the nature of life using increasingly complex and precise instrumentation, based on a growing conviction that analyzing the structural mechanics of organisms could explain every aspect of human development. There was a heightened interest among biologists in the fundamental questions of heredity and evolution: the nature of variation within and across species, the physicochemical processes that organized developing embryonic cells into differentiated functions, the structure of the proteins and chromosomes on which the physical basis for inheritance was inscribed. To Weaver, research in these fields had the potential to unravel the secrets of human life. He framed his policies for the Rockefeller Foundation Division of Natural Sciences as a “new science of man” that would coordinate the many biological science specialties that were developing in the 1930s—embryology, cytology, endocrinology, genetics, biochemistry—into a unified system for improving mankind. If biologists could be mobilized behind the “new science of man” agenda, the human engineering effort could be redefined and revitalized.

In the process of defining his new policies, Weaver initiated what I have labeled the third phase of the human engineering effort: the investigation and control of the structure and function of human bodies on a molecular level. Weaver incorporated multiple fields into his “new science of man” program, which focused on supporting interdisciplinary “cooperation in research” projects that would investigate the “vital
processes” that governed human life. To encourage interdisciplinary “cooperation in research,” he employed two tactics. First, he funded entire biology departments or divisions at elite universities in which the specialists representing various biological disciplines pledged to work together to address research questions related to “vital processes.” The biology departments or divisions at Stanford University, University of Chicago, and California Institute of Technology (Caltech) received major grants under this initiative. Second, Weaver used Rockefeller Foundation support to help the biological sciences “catch up” to the physical sciences by encouraging physical chemists to create partnerships with biologists and then equipping the interdisciplinary research teams with the latest instrumentation of the physical sciences, especially spectroscopes, mass spectrometers, and x-ray crystallography machines. He successfully facilitated partnerships of this type at University of Chicago and Caltech.

Weaver’s formulation of his “science of man” policy based on “vital processes” was a response to a long period of trial-and-error, as foundation officers experimented with policies intended to improve mankind through biological research. This chapter examines that period of trial-and-error, beginning with the funding of eugenics and ending with the implementation of Weaver’s policies for the Rockefeller Foundation Division of Natural Sciences. I begin the chapter by tracing the history of the ill-fated but heavily supported eugenics programs at the Carnegie Institution of Washington (CIW), which I then compare to Edwin Embree’s “human biology” program at the Rockefeller Foundation. Initially, the Rockefeller Foundation followed a similar path to that of the CIW, with a small number of well-connected eugenicists receiving the majority of the grant allotments under Embree’s Division of Studies. In contrast to the
CIW's Eugenics Record Office, though, Embree's "human biology" program lasted only a few years before it was dismantled by other Rockefeller Foundation officers. The case study of Embree's support for Raymond Pearl of Johns Hopkins elucidates why the "human biology" program fell into disfavor: Embree financed Pearl's research primarily because he was a close friend and trusted confidant, not because he was conducting groundbreaking biological investigations.

After Embree's program collapsed, the Rockefeller Foundation officers engaged in a number of policy experiments in an attempt to clarify the optimal way to achieve their human engineering goals through the support of biological science. Weaver's "new science of man" program was the culmination of their efforts, as it combined human engineering and "cooperation in research" goals without abandoning scientists who were connected to the foundation officers through the interlocking directorate. In this chapter, I analyze how Weaver tried, with mixed results, to foster interdisciplinary "cooperation in research" at Stanford University and University of Chicago. I ultimately argue that the only university that built a biological science program that was explicitly tied to Weaver's "new science of man" policy was Caltech, which is the subject of Chapter Five. Nonetheless, Rockefeller Foundation intervention at Stanford and Chicago furthered the careers of outstanding biologists, helped to build and equip laboratories with the latest instrumentation, and encouraged research in several of the "vital processes" sub-fields. Moreover, Weaver was able to facilitate research partnerships between biologists and physical scientists at University of Chicago that yielded a promising application for spectroscopic research. The biology programs that received major grants were all coordinated by scientists who were part of the interlocking directorate, were designed to
cultivate “cooperation in research,” and were expected to advance and refine the human engineering effort, but they produced widely varied results. This chapter is much about the unintended outcomes of foundation intervention in the biological sciences as it is about foundation officers affecting purposeful change in the programs that they funded.

The history of foundation intervention in the biological sciences begins with the Carnegie Institution of Washington (CIW) and the development of the largest eugenics research facility in the world. To understand CIW support for eugenics, it is necessary to examine the background of the architect of the CIW’s Station for Experimental Evolution and Eugenics Record Office, Charles B. Davenport (1866–1944). Davenport earned his Ph.D. in zoology from Harvard University in 1892, where he learned biometrics, morphology (the “study of form”), and eugenics under the tutelage of Karl Pearson. While at Harvard, where he remained as an instructor from 1892 to 1899 (Yerkes was one of his students), Davenport became a leading proponent of biometrics, which he continued to promote after he became a professor at University of Chicago in 1899. Biometrics, the statistical studies of biological populations, had been introduced by eugenics pioneer Francis Galton in the 1870s and spread to the United States in the 1880s by Davenport’s advisor, Pearson. According to Galton, the laws governing heredity

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could be analyzed "in terms of units of statistical deviation" from the norm.\textsuperscript{482} For example, if a majority of members of a given family were "feebleminded," the "norm" for the family was "feeblemindedness." Consequently, roughly 80\% of offspring in the family would be "feebleminded" to some degree, while approximately 10\% would be of sound mind and approximately 10\% would be completely incapacitated. Based on the logic of biometrics, all "hereditary" character traits were expected to gravitate toward a statistical mean established by previous generations of family members, from native intelligence to a predilection toward criminal behavior.\textsuperscript{483} Biometricians claimed that the use of statistical analysis to compare populations in terms of personality, intellect, and physical features would explain evolutionary riddles like variation and selection on a species level, rather than in terms of individual development.

Biometrics seemed to provide "a means of determining the elusive laws of human heredity," but the methodology hardly went uncontested.\textsuperscript{484} In 1900, the rediscovery of Gregor Mendel's "Versuche über Pflanzenhybriden," an 1866 paper that traced inheritance through the crossing of sex chromosomes, provided an explanation for the mechanism of heredity that led many scientists away from biometrics.\textsuperscript{485} Based on his studies of pea plants, Mendel argued that each parent organism carried two "factors" for each trait. The factors could be either dominant or recessive, with the dominant factors masking the recessive ones. Mendel's laws stated that each parent contributed exactly

\textsuperscript{482} Daniel Kevles, \textit{In the Name of Eugenics: Genetics and the Uses of Human Heredity} (New York: Alfred A. Knopf, 1985), 15.

\textsuperscript{483} Kenneth Ludmerer, \textit{Genetics and American Society: A Historical Appraisal} (Baltimore: Johns Hopkins University, 1972), 2.

\textsuperscript{484} Charles Rosenberg, \textit{No Other Gods}, 89.

\textsuperscript{485} Virtually every history of science has a reference to the rediscovery of Mendel. Two sources that were valuable for this chapter: Lois Magner, \textit{A History of the Life Sciences}, 3rd ed. (New York: Marcel Dekker, 2002), 387; Garland Allen, \textit{Life Science in the Twentieth Century} (New York: John Wiley and Sons, 1975), 47.
two factors for each trait (either both dominant or both recessive or one dominant and one recessive), and that factors for a given trait did not affect any other traits.\textsuperscript{486} Therefore, Mendel’s laws offered a greatly simplified process for determining the mechanism of heredity. If scientists could identify the factors that each parent contributed, then they could also calculate the statistical probability that a given trait would be passed from the parents to the offspring.

When biologists tried to apply Mendel’s conclusions about pea plants to the study of human heredity, complications abounded. Debates over the application of Mendel’s laws to humans grew extremely divisive. A significant group of biologists abandoned the study of humans and decided to test Mendelian theory using readily available and easily manipulated research subjects like guinea pigs and fruit flies. They restricted their conclusions to the results obtained in the laboratory, and thus moved away from suggesting that there were direct applications for humans. Biometricians like Charles Davenport, however, saw Mendel’s laws as a vindication of their work to establish patterns of traits through the statistical analysis of human “pedigrees.” Yet, as Charles Rosenberg explains, “the very attitudes which inspired scientists to apply Mendelianism immediately to the inheritance of human traits made an objective study of these traits impossible.”\textsuperscript{487} Since human genetics was barely distinguishable from eugenics for much of the first third of the twentieth century, assumptions about eliminating inherited behaviors and predilections through selective breeding colored biometricians’ collection and interpretation of data. Of all the human geneticists of this stripe, Davenport “was the

\textsuperscript{486} Garland Allen, \textit{Life Science in the Twentieth Century}, 48.
\textsuperscript{487} Charles Rosenberg, \textit{No Other Gods}, 89.
most active and prominent.\textsuperscript{488} He was deeply committed to the eugenics movement and to the idea that human progress could best be achieved through improvements in the germ cells that were passed from one generation to the next. Davenport asserted that control over heredity "stands as the one great hope of the human race; its savior from imbecility, poverty, disease, immorality," a stance that he maintained until the end of his life.\textsuperscript{489}

In 1904, Davenport successfully persuaded the Carnegie Institution of Washington (CIW) to establish a Station for Experimental Evolution at Cold Spring Harbor with him at the helm.\textsuperscript{490} At his Cold Spring Harbor laboratory, Davenport designed a research program that included the biometric analysis of human pedigrees to trace "inherited" diseases, physical traits, and behavioral tendencies. Over the next several years, the Station for Experimental Evolution became a highly regarded laboratory complex, while Davenport built a reputation as both a leading expert on eugenics and research into heredity. In 1910, Davenport, an increasingly adept scientific entrepreneur, persuaded the heiress to the Harriman railroad fortune to sponsor a Eugenics Record Office (ERO) at Cold Spring Harbor and recruited Harry H. Laughlin to act as superintendent.\textsuperscript{491} The Eugenics Record Office was organized for two main purposes: "to carry out research on human heredity, especially the inheritance of social traits" and "to educate laypersons about the importance of eugenics research and the applications of eugenics findings for public policy."\textsuperscript{492} Mrs. Harriman continued to supply Davenport and Laughlin with funds and facilities for the better part of a decade,

\textsuperscript{488} Ibid.
\textsuperscript{489} As quoted in Rosenberg, No Other Gods, 91.
\textsuperscript{490} Daniel Kevles, In the Name of Eugenics, 45.
\textsuperscript{491} Mark Haller, Eugenics: Hereditarian Attitudes in American Thought, 65.
including donations of a 75 acre plot, a house with a fireproof storeroom, and an annual operating budget of $20,000. By 1918, when she ceded control of the ERO to the CIW, Harriman had contributed over $800,000 (equivalent to approximately $13,000,000 in 2005) to the enterprise.\textsuperscript{493} Once the CIW had taken administrative control, Davenport consolidated the ERO with his Station for Experimental Evolution to create the Carnegie Institution of Washington Department of Genetics, which he and Laughlin ran together.\textsuperscript{494} Throughout the 1920s and 1930s, the Department of Genetics received a larger share of Carnegie funds for the biological sciences than any other applicant. From 1918 to 1939, the CIW allocated $125,000 annually to support Davenport and Laughlin as they collected pedigree information and conducted morphological studies.\textsuperscript{495}

Under Davenport’s guidance, the CIW Cold Spring Harbor outpost became both a bastion of eugenics and a leading laboratory for biometrics research. On the one hand, Davenport made some important discoveries using his pedigree method, particularly in terms of discovering hereditary patterns for albinism, Huntington’s chorea, and otosclerosis.\textsuperscript{496} The contributions of his research were limited, however, because he chose to focus considerable attention on human behavioral characteristics. The “inherited” traits that he traced included a vast spectrum of human qualities: alcoholism, insanity, epilepsy, criminality, “pauperism,” and “feeble-mindedness.”\textsuperscript{497} For example, after conducting a study of 350 “wayward girls,” Davenport concluded that prostitution was the direct result of a measurable inherited trait: “innate eroticism.”\textsuperscript{498}

\begin{flushleft}\textsuperscript{493} Kevles, \textit{In the Name of Eugenics}, 55. \\
\textsuperscript{494} Haller, \textit{Eugenics: Hereditarian Attitudes in American Thought}, 66. \\
\textsuperscript{496} Charles Rosenberg, \textit{No Other Gods}, 92. \\
\textsuperscript{497} Daniel Kevles, \textit{In the Name of Eugenics}, 46. \\
\textsuperscript{498} Charles Rosenberg, \textit{No Other Gods}, 93.\end{flushleft}
Laughlin made similar arguments about a number of other forms of social or behavioral
“dysfunction:” the “feebleminded” had inherited a “general nervous weakness,” while the
poor had inherited “relative inefficiency” and “mental inferiority.”

Because of their allegiance to eugenics, Davenport and Laughlin limited the
Carnegie Institution of Washington’s capacity to contribute to innovations in the
biological sciences. Davenport and Laughlin advocated a viewpoint that was
questioned by the leading university biologists of the 1920s. Biologists who rejected
biometrics had launched what Garland Allen has dubbed a “revolt against
morphology.” The “revolt” was largely in response to revolutionary genetics research
by T. H. Morgan and his famous Columbia University “fly lab.” In 1915, the
geneticists of the Columbia fly lab had published their “epoch-making” text, *The
Mechanism of Mendelian Heredity*, in which they posited that genetic “factors” were
physically located on chromosomes. After the publication of Morgan’s “chromosome
theory,” genetics research “was restricted principally to mapping new genes” that were
“viewed as discrete physical units located at definite points on chromosomes.”

Biologists embraced laboratory experimentalism, using animal or insect subjects and the
latest instrumentation to investigate questions about inheritance, species variation, natural
selection, and embryonic development. Experimentalism “triumphed in a clear and

499 Daniel Kevles, *In the Name of Eugenics*, 46.
visible way” over biometrics in major university biology departments. Eugenics remained a popular political and social movement, but it was based on a methodology that the generation of scientists who were making breakthroughs in genetics, embryology, cytology and physiology found highly suspect.

Rather than feeling chastened by the skepticism of other scientists, Davenport and Laughlin only escalated their advocacy for eugenics and biometrics throughout the 1920s and 1930s. At the Second International Congress on Eugenics in 1923, Davenport presented a paper in which he argued that “not only mental but also emotional states have a hereditary basis” that can be determined through pedigree analysis. He applied his findings about the “hereditary basis” for mental and emotional states to his studies of race, which he used to “prove” that various ethnic groups were biologically distinct and inclined toward racially-determined behavior. In one of his studies, Davenport argued that Poles were “independent and self-reliant though clannish,” Italians tended toward “crimes of personal violence,” and “Hebrews” were inclined toward thievery. In 1929, Davenport and fellow eugenicist Morris Steggard used Carnegie funds to publish *Race Crossing in Jamaica*. Davenport and Steggard construed races as different species and used outmoded mental testing to argue that blacks were intellectually inferior. They asserted further that sterilization laws should be implemented to weed out the weaker members of each race in order to further human progress.

Meanwhile, Laughlin became known for his campaigns to sterilize the “unfit” and to restrict immigration. Throughout the 1920s, Laughlin was the most “passionately

508 Daniel Kevles, *In the Name of Eugenics*, 46–47.
outspoken advocate of sterilization in America,” as well as the reigning authority on the importance of immigration restriction for the maintenance of racial purity. In 1922, he testified before Congress to argue that immigrants from southern and eastern Europe “were biologically inferior” and “jeopardized the blood of the nation.” That following year, Laughlin delivered two papers at the Second International Congress of Eugenics: “The Present Status of Eugenical Sterilization in the United States” and “The Nativity of Institutional Inmates.” In the first paper, he contended that increased use of sterilization measures was necessary “for purging the human stock of its more degenerate and worthless strains,” which could be determined scientifically because “degeneracy is of a hereditary nature.” In his second paper, Laughlin warned that the only way “to prevent further deterioration of the American racial values through undesirable immigration” was to deport any immigrant who was “socially inadequate,” which he defined as “feebleminded, insane, criminalistic, epileptic, inebriate, diseased, blind, deaf, deformed, [or] dependent.”

Laughlin grabbed public attention for his cause when he testified as a scientific expert in the court case that became known, once it reached the Supreme Court, as Buck v. Bell. The case centered on whether Carrie Buck, a “moron” who had been committed to the Virginia Colony for Epileptics and Feebleminded in 1924, should be sterilized lest she give birth to “feebleminded” offspring. Although he never met any of the women of the Buck family, Laughlin determined that Carrie, her mother, and her daughter were

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510 Daniel Kevles, In the Name of Eugenics, 103.
511 Ibid., 108.
members of the "shiftless, ignorant, and worthless class of anti-social whites of the South" and that Carrie's offspring were doomed to become "mentally deficient." He maintained his position despite the fact that no one had found Carrie's mother or daughter to be "feebleminded." Laughlin's testimony was incorporated into the Supreme Court verdict, in which eight of the nine justices decided that the state of Virginia should be allowed to sterilize Carrie Buck. In his majority opinion, Justice Oliver Wendell Holmes made the notorious statement that "three generations of imbeciles are enough."

In the early 1930s, Laughlin began to voice his support for Nazi raceology and eugenics. Officers at the Carnegie Institution of Washington (CIW) started to worry that Laughlin's sympathy for Nazism would tarnish the Carnegie name, not to mention severely undermine the results that the Eugenics Record Office (ERO) published. The president of the CIW, John C. Merriam, came to feel that the ERO was becoming a burden. In 1934, Merriam gently nudged Davenport into retirement, leaving Laughlin without an ally to bolster his increasingly virulent, pro-Nazi eugenic claims. After Davenport's departure, the CIW officers organized a committee of prominent anthropologists and geneticists to evaluate the ERO. The committee "concluded that the Eugenics Record Office was a worthless endeavor from top to bottom, yielding no real data," and demanded that Laughlin "cease from engaging in all forms of propaganda and the urging or sponsoring of programs for social reform or race betterment such as

514 Daniel Kevles, In the Name of Eugenics, 110.
515 Buck v. Bell. 274 U.S. 200, 47 S. Ct. 584 (1927); United States Supreme Court, United States Reports 274 (1927), 207.
518 Cravens, Triumph of Evolution, 180.
sterilization, birth control, inculcation of race or national consciousness, restriction of immigration, etc."519 Laughlin completely ignored the request.

From 1935 to 1939, Laughlin completed his quest to affiliate the Carnegie name with Nazism: he wrote in support of the Nuremberg Laws and quoted liberally from Mein Kampf when testifying before the Senate Immigration Committee. In 1936, the University of Heidelberg awarded Laughlin an honorary degree, which he accepted both as "a personal honor" and as "evidence of a common understanding of German and American scientists of the nature of eugenics."520 Since the Nazis had taken control over German universities by 1935, recognition by the University of Heidelberg was effectively a pat on the back from the Nazi regime. CIW president John C. Merriam wanted to silence Laughlin, but he never took the final step of forcing him to retire or asking him to step down. When Merriam retired in 1938, his successor, Vannevar Bush, immediately asked Laughlin to retire.521 Laughlin dragged out negotiations, long enough for him to publish Immigration and Conquest under the Carnegie label in 1939, in which he likened immigration to an infestation of rats. An exasperated Vannevar Bush demanded that Laughlin leave by the end of 1939 and unceremoniously closed the ERO.522

Carnegie intervention in the biological sciences during the 1920s and 1930s provided the officers of the Rockefeller philanthropies with a model of exactly what not to do. The officers of the Carnegie Institution of Washington (CIW) had ceded control of the biological sciences to Davenport and Laughlin. The CIW officers were especially reliant on Davenport, whom they trusted as an accomplished scientist to construct a solid

520 Daniel Kevles, In the Name of Eugenics, 118; Edwin Black, War against the Weak, 389–395.
522 Charles Rosenberg, No Other Gods, 96.
research program for the investigation of heredity and evolution. Their investment yielded few returns. As Mark Haller asserts,

Davenport . . . was a pioneer in the study of human genetics, and he had the facilities and support to create a world center for the painstaking and important investigations in the heredity of man. That he did not do so was a tragedy resulting largely from his scientific methods and his temperament.\(^{523}\)

Not only did the CIW officers depend too heavily on Davenport, who clearly let them down, but they also lacked any guiding principles for their science policy. Lastly, CIW officers placed too much emphasis on internal laboratories, and missed opportunities to cooperate with the university researchers who were becoming the pioneers in genetics.

An examination of the Rockefeller Foundation over the same time period that the CIW was enmeshed in the ERO reveals how foundation officers who initially fell into similar traps emerged with a much more effective set of policy solutions. The Rockefeller boards were drawn into funding research in the biological sciences immediately after World War I through the National Research Council (NRC). As mentioned in previous chapters, Rockefeller boards channeled millions of dollars through the NRC between 1916 and 1940, originally through fellowship programs and, later, in support of committee projects.\(^{524}\) In terms of support for the biological sciences, the NRC Committee for Research in Problems of Sex played the most prominent role. The members of the Committee on Sex Problems were conducting research and formulating rationales for their projects that fit the Rockefeller human engineering effort perfectly. Exposure to the NRC Committee on Sex Problems also strongly influenced the

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development of Edwin Embree’s “human biology” program for the Rockefeller
Foundation Division of Studies, which was implemented from 1925 to 1927 and was the
main source of Rockefeller funding for the biological sciences in the 1920s.

As I argued in Chapter Three, Edwin Embree’s “human biology” program for the
Rockefeller Foundation Division of Studies was an example of the second phase of the
human engineering effort, when the improvement of mankind was understood as the
investigation and control of humans as a species, primarily through behavioral
modification. The main component of the second phase of the human engineering effort
was the Yale program in the behavioral sciences, but other aspects of the “human
biology” program corresponded to the second phase as well. Specifically, like his
contemporaries at the Carnegie Institution of Washington, Embree supported biologists
who were active in the eugenics movement. These biologists were as preoccupied as the
psychologists with the idea that influencing human sexual behavior would improve
mankind. In fact, the biologists who received the largest allotments under Embree’s
program promised to apply their research directly toward regulating human sexual
behavior through eugenics.

Embree was not a trained scientist. When he made decisions about Division of
Studies program initiatives and grant allotments, he relied on scientists who had become
his friends or who had earned the trust of foundation officers through their affiliations
with the National Research Council (NRC). Of course, Robert Yerkes was one of the
scientists who heavily influenced Embree. Much of the “human biology” program was
also based on advice and guidance that Embree received from his close friend Raymond
Pearl (1879–1940), a biologist at Johns Hopkins. The partnership with Embree was extremely beneficial for Pearl, whose Institute for Biological Research received $175,000 from the Rockefeller Foundation. Yet, Embree’s reliance on Pearl quickened the demise of the Division of Studies. When he tied the fortunes of his Division of Studies program to Pearl’s agenda, Embree firmly aligned himself with eugenicists rather than the physiologists, embryologists, and geneticists who were taking over professional biology. The decision came back to haunt him.

Raymond Pearl was an avid promoter of eugenic population control, and his research reflected his belief that modern science could be mobilized “to improve society through the use of known biological principles.” In a presentation before the Second International Congress of Eugenics in 1923, Pearl had argued that the overpopulation of the planet by defectives was the single greatest threat to human civilization. Pearl framed his argument in positive human engineering terms that emphasized the control that humans could have over their future if they followed eugenics principles: “Man, in theory at least, has it now completely within his power to determine what kind of people will make up the earth’s population.” While he tempered his stance somewhat over the course of the 1920s, Pearl remained devoted to biometrics in his studies of human genetics, even though his methodology was fast becoming outmoded. His program

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525 Letter from Embree to Pearl, February 16, 1925; Letter from Embree to Pearl, May 28, 1925, folder 1791, box 145, Series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC; Kohler, Partners in Science, 125–126.
528 Ibid.
529 Grant of $175,000 for Raymond Pearl, 5/27/25, folder 1791, box 145, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC; “Report of the Director of the Institute for Biological Research,”
was exactly the type of research that the directors of the Rockefeller Foundation Division of Natural Sciences revolted against in the early 1930s.

Pearl created other problems beyond his stalwart adherence to eugenics and biometrics. His Institute for Biological Research was controversial at Johns Hopkins because it operated independently from the university’s Department of Biology. Because he was running a separate Institute, Pearl was able to maintain complete control over his grant allotments despite the fact that, in the judgment of his peers at Johns Hopkins, he misused much of his appropriation. One of his most flagrant abuses of power was to give himself the unusually high salary of $15,000 (equivalent to over $160,000 in 2005), a sum that far outstripped his colleagues at Johns Hopkins (or any other major university). Therefore, when Embree devoted the bulk of the Division of Studies funds to Pearl’s Institute, he contributed to a research project that was fraught with both scientific and logistical tribulations. Embree’s fellow Rockefeller Foundation officers were well aware of the issues presented by Pearl’s case, and they were greatly displeased. At the same time, Embree compounded his problems with his fellow officers by devoting large sums to other controversial eugenics projects, including grants to Charles Davenport at the Carnegie Institution of Washington for his Eugenics Record Office research.

When the Rockefeller Foundation officers dissolved the Division of Studies in 1927, they explained that their decision resulted mainly from their lack of confidence in Embree’s understanding of “human biology.” Thomas Appleget, the vice president of the

June 1926; Memo by Embree, 9/24/26, folder 1792, box 145, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

530 Memo from Hanson to Weaver, May 1938, folder 1795, box 145, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Rockefeller Foundation, summarized the consensus surrounding the short-lived Division of Studies:

I think that the initial appropriation made in 1925 [to Pearl] was not only a bad thing to do but was badly done. Raymond Pearl was one of Embree’s enthusiasms. The appropriation which he recommended to the Foundation not only embarrassed Johns Hopkins, but subsequently was a source of embarrassment to the Foundation.\footnote{531}

When the Foundation officers dismantled the Division of Studies, they were not only rejecting eugenics. They were also reacting against Embree’s tendency to vest too much authority and interest in a few trusted individuals. As demonstrated by the example of the Carnegie Institution of Washington, the approach of concentrating on select researchers (Carnegie’s “exceptional man” philosophy) rather than on carefully administered policy initiatives was extremely problematic. Rockefeller Foundation officers were determined to avoid the “exceptional man” approach as they reformed their policies in the late 1920s and early 1930s.

Although the Division of Studies was a “source of embarrassment,” Embree did help shift the Rockefeller Foundation toward the support of biology. In the Rockefeller Foundation Annual Report for 1926, Embree wrote, “Biology is not only a premedical science and as such of interest to the Foundation, but it is a general science underlying all forces of the body, mind, and social organization.”\footnote{532} This belief in the fundamental importance of biological research made a lasting impression on Foundation policy, especially after the reorganization of 1928. As noted in the previous chapter, the

\footnote{531} Memo from Appleget to RF officers re: “Biology at Johns Hopkins,” May 9, 1939, folder 1796, box 145, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

reorganization “signaled a shift from traditional Rockefeller concerns with education and applied science to support of scientific research.”

“As Pure” research was the stated focus, but the question of which fields deserved the most support remained. The consistent interest that Rockefeller officers, including Embree, had shown in human engineering helped to push the directors of the newly organized Division of Natural Science toward biology. Still, it took several years and a few policy experiments to arrive at the signature program that Warren Weaver developed to support the biological sciences.

As mentioned in Chapter Three, in May 1928, the Rockefeller Foundation trustees recruited mathematician Max Mason to be the director of the Division of Natural Sciences. Mason outlined an “advancement of knowledge” program that was mostly a continuation of previous loosely defined policies. His program, which was officially adopted at the beginning of 1929, stated priorities including “aid to established institutions of high rank” and “aid to established leaders through institutions in which leaders are working.” The institutions and leaders would be identified by Foundation officers in cooperation with “one or more experts” and “established organizations” (ostensibly the NRC). The policy lacked any clear guidelines regarding which research fields were worthy of support. While Mason inspired the confidence of his fellow officers because he was charismatic and brilliant, he was less than inspiring as an architect of Natural Sciences policy.

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Soon after Mason drafted his "advancement of knowledge" policy for the Division of Natural Sciences, the trustees promoted him to president of the Rockefeller Foundation, a post that he assumed on August 30, 1930. He appointed Hermann Spoehr, an organic chemist who had conducted research at the Carnegie Institution Washington "Desert Laboratory" while serving as an adjunct professor at Stanford University, to be his replacement.\textsuperscript{536} Spoehr developed a broadly defined program in the natural sciences based on the idea that all sciences should be connected in an effort to understand living beings.\textsuperscript{537} One aspect of Spoehr's policy that had lasting significance for the Rockefeller Foundation Division of Natural Science was his conviction that the methods and instruments of the physical sciences should be applied to the biological sciences. As he wrote in his first report in 1930,

Research in the physical sciences has given us new senses by the aid of which it has been possible to gain a clearer understanding of the nature of matter and energy. . . . By means of these methods it has been possible to subject existing conceptions to vigorous experimental test and to develop fundamental ideas regarding the constitution of matter. These methods are rapidly finding wide application in the elucidation of the chemistry of both inorganic and organic compounds; they . . . are certain to influence profoundly many aspects of the biological sciences.\textsuperscript{538}

For Spoehr, support in these areas meant fluid research funds (not designated for a particular project) for natural sciences at MIT ($170,000), University of Minnesota

\textsuperscript{537} Program and Policy Report, PRO-22, October 29, 1930, pp. 89–90, folder 167, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.  
\textsuperscript{538} Rockefeller Foundation, \textit{1930 Annual Report}, 189.
($150,000), and Washington University ($100,000), as well as endowment funds to
California Institute of Technology for general purposes ($500,000), Johns Hopkins
University to develop the biological sciences in the wake of Pearl’s departure ($387,500
over ten years), Harvard University for astronomy ($500,000), and Princeton University
for geology ($100,000). For specific projects, Spoehr added to the ongoing support for
Robert Yerkes’s anthropoid research ($750,000) and initiated what would become a
longstanding investment in biology at the University of Chicago ($150,000).539 As these
grant allotments demonstrate, Spoehr was falling into the patterns of his predecessors:
supporting promising institutions and scientists or continuing existing programs without
delineating clear policy objectives. Spoehr’s Division of Natural Sciences initiatives
added to the Foundation’s efforts to contribute to scientific research programs in elite
universities, but the administration of funds was done on a rather trial-and-error basis.

Meanwhile, Max Mason introduced terms into the Rockefeller Foundation
Natural Sciences vocabulary that became integral to 1930s policies: “vital processes” and
“psychobiology.” At an officer and trustee conference in October 1930, Mason
suggested that the Foundation design a comprehensive, interdisciplinary program that
could bring together the social sciences, medical sciences, and natural sciences in an
effort to investigate “mental health and personality.”540 He asserted that the officers
should build on the Division of Natural Sciences, which had already begun the process of
“strengthening institutions and assisting men whose researches have seemed of most
importance for determining the laws of the physical behavior of matter, and the nature of

539 “Natural Sciences” by Warren Weaver, April 11, 1933, folder 6, box 1, series 915, Record Group 3.1,
Rockefeller Foundation Archives, RAC.
540 “The Rockefeller Foundation: A Brief Summary of the Conferences of Trustees and Officers at
Princeton”, October 1930, p. 5a, folder 166, box 22, series 900, Record Group 3.1, Rockefeller Foundation
Archives, RAC.
vital processes." When Hermann Spoehr resigned in August 1931, Mason decided to recruit his close friend and former graduate student, Warren Weaver, to help him clarify his ideas about "vital processes" and "psychobiology" for the Division of Natural Sciences.

By the time Warren Weaver was hired as Director of the Rockefeller Foundation Division of Natural Sciences in 1932, the biological sciences had been established as an interest for the Foundation, but many of the projects that the officers supported were still connected to the areas covered by "human biology" and related fields. Weaver wanted to compare the projects that the Foundation was sponsoring to the research that the top biologists in the world were completing. He thus spent his first year at the Rockefeller Foundation studying past policies and visiting elite universities throughout the United States and Europe, where he witnessed investigators conducting genetic crossing experiments with *Drosophila* and guinea pigs, using electron microscopes to analyze cellular structure, and immersing growing plant and animal embryos in volatile solutions to assess the importance of environment on development. When he compared what he saw in these laboratories to the "human biology" projects that were receiving support from the Rockefeller Foundation, including the behavioral science projects at Yale and Pearl’s Institute at Johns Hopkins, Weaver determined that a radical policy change was needed. He arrived at the conclusion that "human biology," eugenics, and the version of "psychobiology" that focused on the modification of behavior were imprecise and essentially unscientific approaches. Weaver turned instead to the ideas that Mason had introduced, especially the concept of "vital processes," to help him define a policy for the

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541 Ibid., 3a.
Division of Natural Sciences that would support the innovative research that he had
witnessed in the world-class laboratories that he had visited.

On January 27, 1933, Weaver presented his “Science and Foundation Program” to
his fellow officers and trustees. He began with a statement of purpose that laid out the
assumptions on which the Natural Sciences policy would be based:

Our understanding and control of inanimate forces has outrun our understanding
and control of animate forces. This, in turn, points to the desirability of an
increased emphasis, within science, on biology and psychology, and on the
special developments in mathematics, physics, and chemistry which are
themselves fundamental to biology and psychology.543

The program thus rested on the belief, previously introduced by Spoehr, that the
biological sciences needed an extra boost to reach the level of rigor of the physical
sciences. As he stated in his April 1933 report, titled “Natural Sciences—Proposed
Program,”

Biology is today in a position somewhat analogous to that occupied by physics
and chemistry many years ago. It has, that is, advanced out of the stage of
qualitative observation and into the stage of detailed quantitative analysis . . . .
The evidence is clear that the time is ripe to bring to bear the powerful technics of
mathematics, physics and chemistry onto the basic problems of biology.544

To fulfill the objectives of grafting the biological sciences onto the physical sciences,
Weaver focused on providing the latest instruments for quantitative and structural

543 “Science and Foundation Program” by Weaver, January 27, 1933, folder 6, box 1, series 915, Record
Group 3.1, Rockefeller Foundation Archives, RAC.
544 “Natural Sciences—Proposed Program” by Weaver, April 11, 1933, 77–78, folder 6, box 1, series 915,
Record Group 3.1, Rockefeller Foundation Archives, RAC.
analysis to biological laboratories. Supplying laboratories in elite universities with electron microscopes, ultracentrifuges, spectrosopes, spectrographs, micropipettes, and machines for x-ray crystallography became a critical element of the program.

After reviewing the problems that Embree and Spoehr had faced when trying to construct a natural sciences program, Weaver decided that he needed a policy with specific priorities. As he wrote in the Foundation’s 1933 Annual Report,

During the years immediately following 1929, when the Rockefeller Foundation’s program in the natural sciences was first organized, there was recognized in selecting projects for aid, some preferential emphasis upon certain fields of interest; but the primary emphasis was not upon field but rather upon the outstanding leadership of the chosen men or institutions. . . . A highly selective process is necessary if the available funds are not to lose significance through scattering.\(^5\)

Weaver added that the fields of concentration should “contribute in a basic and important way to the welfare of mankind,” thus staying within the parameters of the Foundation statement of purpose.\(^6\) The question remained: which fields would best serve the welfare of mankind? Again, Weaver turned to researchers at elite universities, his fellow Foundation officers, and representatives from the NRC for guidance. He concluded that the main focus of the natural sciences program would be Mason’s suggestion of “vital processes,” an umbrella term that included the subfields of endocrinology, genetics, biology of reproduction, and psychobiology.

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\(^{6}\) Ibid., 197–198.
The choice of these subfields reflected Weaver’s reliance on his closest advisors, including Simon Flexner (Director of the Rockefeller Institute for Medical Research), Walter B. Cannon (professor of physiology at Harvard), William H. Howell (professor of physiology at Johns Hopkins), and Frank R. Lillie (Dean of the Division of Biological Sciences at the University of Chicago).\textsuperscript{547} Cannon and Lillie were members of the critically important NRC Committee for Research in Problems of Sex.\textsuperscript{548} As described in Chapter Three, the NRC Committee on Sex Problems was a foundation favorite because it connected seamlessly with Rockefeller-sponsored social hygiene programs. The success of the Committee on Sex Problems was also the product of trends that were transforming the biological sciences in the 1920s. In the decade after World War I, there was a rapid expansion of research in reproductive science, which was greatly accelerated through the efforts of Committee on Sex Problems members. “By capturing the Committee,” Adele Clarke contends, Lillie and Cannon “gained broad legitimation for reproductive sciences” as well as “significant funding” through their “direct relationships” with Foundation officers.\textsuperscript{549} Weaver was responsive to the influence of Committee on Sex Problems members, and he incorporated the reproductive sciences into his policy.

In the 1920s and 1930s, the reproductive sciences included three major areas of biological research: genetics, embryology, and endocrinology. Geneticists, following the breakthroughs introduced by T. H. Morgan and the Columbia fly lab, focused on

\textsuperscript{547} Report of the Committee on Appraisal and Plan, December 11, 1934, folder 170, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.


\textsuperscript{549} Adele Clarke, “Embryology and the Rise of American Reproductive Sciences,” 118.
mapping genes on chromosomes to determine the physical basis for heredity. Their main concern was the transmission of traits from one generation to the next. Embryologists examined the differentiation of cellular function during the growth and development of organisms to determine how the mechanisms of natural selection influenced individual beings. In many respects, endocrinologists filled in the gaps left between genetics and embryology by concentrating on the biochemistry of hormones and enzymes, "internal secretions" that affected the expression of genetic traits and the process of cellular differentiation. These emerging disciplines fit together seamlessly and formed the basis for the Natural Science program. Weaver also included the subfield of psychobiology, which he saw as critical for adding the variable of human behavior into an otherwise physiological, mechanistic program.

Weaver wanted to tie the "vital processes" subfields of the "biology of reproduction," genetics, endocrinology, and psychobiology together to create a new "science of man." His conception of the "science of man" both built on and departed from earlier Foundation interests in eugenics and behaviorism. Weaver saw his "vital processes" program for biological sciences as part of a comprehensive, interdisciplinary project, including all of the Foundation divisions, to investigate and control the human body and mind.\textsuperscript{550} By "vital processes," Weaver meant analysis of the "physico-chemical factors" that affected human development and behavior. Since "physico-chemical factors" were quantifiable substances—hormones, nutrients, cells—the Natural Sciences policy needed to include provisions for building and equipping laboratories where scientists could conduct precise research in the fields of "genetics, endocrinology, and the

\textsuperscript{550} "Natural Sciences—Proposed Program" by Weaver, April 11, 1933, 78–79, folder 6, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
biology of reproduction.” In Weaver's understanding, research in these three fields would yield clear, tangible results that could be applied to improving mankind.

According to Weaver, genetics research into the growth and duplication of genes, changes during chromosomal conjugation, the relationship between genes and expressed traits, and mutations was critical to understanding the “vital processes” that shaped human life. Endocrinology, the “biochemical, neurological and psychological aspects of internal secretions in general” and “closely related nutritional studies,” required support because researchers were discovering that hormones and enzymes played as significant a role in development as genetic material. The biology of reproduction, which was already receiving substantial funding through the NRC Committee for Research in Problems of Sex, overlapped substantially with the other fields. Research in the biology of reproduction included studies in psychobiology, embryology, fertility, and the “neuropathology and neuropsychology of sex.”

The policy as a whole would address fundamental questions about human behavior and social progress, as Weaver explained:

Can we obtain enough knowledge of the physiology and psychology of sex so that man can bring this aspect of his life under rational control? Can we unravel the tangled problem of the endocrine glands and develop a therapy for the whole hideous range of mental and physical disorders which result from glandular disturbance? Can we develop so sound and extensive a genetics that we can hope in the future to breed superior men? . . . In short, can we rationalize behavior and create a new science of man?

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551 Ibid.
552 Rockefeller Foundation, 1933 Annual Report, 199.
In this exhaustive list of questions, Weaver set up an agenda that revitalized and reformed the human engineering effort. His optimism about the implications of his program was staggering, as he explained that the "science of man" policy offered renewed "hope for the future of mankind." 553 Weaver's tremendous optimism was evident in his presentation of his complete program to Foundation officers and trustees. In a grand, sweeping statement, Weaver outlined the connections among his chosen subfields:

In research upon the anatomy and physiology of the nervous system lies the promise of understanding not only the control of movement and sensation but also essential factors in the emotions of an individual. . . . In some ten places in the body, chiefly in ductless glands, are secreted chemical substances, called hormones, which, although present in fantastically small amounts, affect a wide variety of physiological and psychological processes. . . . In purifying the internal secretions biochemistry has offered the refinement of procedure necessary for clear understanding. . . . Still another form of biological research in the dawn of development is the science of genetics. It is being proved that cases of resistance to disease, nervous and physical constitution, mental defects and emotional types of behavior are hereditable, i.e., that in the germ plasm of the organism lie factors predisposing to results hitherto ascribed to the environment. 554

Therefore, the policy would include the following subfields:

1) psychobiology: psychology, psychiatry, neuropsychiatry, etc.

2) internal secretions: hormones, enzymes, etc.

553 Ibid.
554 "Activities of the Foundation in 1933," pp. 7-8, folder 168, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
3) nutrition: vitamins, etc.

4) radiation effects: ultraviolet, X-rays, cosmic rays, mitogenetic rays, etc.

5) biology of sex: physiology of sex, fertility, etc.

6) experimental and chemical embryology: fertilization and sex determination, transplantation, regeneration, organizers, etc.

7) genetics: chromosomes, genes, cytology, etc.

8) general physiology: cell physiology, nerve conduction, electrical effects, osmosis, permeability, etc.

9) biophysics and biochemistry: spectroscopy, microchemistry, basic studies.

The ultimate aim and the central problem of the program is the analysis and rationalization of human behavior. . . . ‘Psychobiology’ is the single principal topic, all others being viewed as contributory.  

As I argued in the previous chapter, Weaver ostensibly devoted his program to “psychobiology,” but he completely redefined the term from its prior usage by behaviorists. In Weaver’s version of “psychobiology,” the emphasis was exclusively on the “physico-chemical processes” that governed the human body and mind. The best way to investigate human behavior, then, was through the “hard” biological sciences rather than through the types of observations that, for example, Yerkes conducted in his primate laboratories. Weaver’s policy objectives showed that he intended to continue and further the human engineering effort that his predecessors had launched, but that he saw the previous implementation of the effort as misguided. He spent his first few years at the Foundation dismantling the remnants of the “human biology” program while he sought

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555 “Activities of the Foundation in 1933,” pp. 4–5, folder 168, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.
the most promising opportunities to fund projects that fit his "new science of man," "vital processes" program.

Weaver continued to rely heavily on members of the NRC Committee on Sex Problems and to emphasize sex research as he began to implement his Division of Natural Sciences policies. The Foundation officers as a whole remained dedicated to the study of sex research as part of their human engineering effort. As they explained,

Of all the recognized interests of the program, none stands closer to practical application than the field of endocrinology and the interrelated field of sex research. Moreover, these fields of research are fundamental to the broad, common program of the Foundation, which seeks a rational understanding of human behavior.\[556\]

The focus on sex research meant that support for NRC Committee on Sex Problems members was a top priority throughout the 1930s. Committee members Frank R. Lillie and F. C. Koch of the University of Chicago received large Foundation grants, as did P. E. Smith and E. T. Engle of Columbia University and Walter B. Cannon of Harvard University. Not coincidentally, the biology departments of the University of Chicago, Columbia, and Harvard were among the recipients of major Natural Science grants under Weaver's direction. These universities were designated by the NRC Committee on Sex Problems as centers of research into the biology of sex, a distinction that helped sustain the interest of Foundation officers.

The NRC Committee on Sex Problems was one avenue that helped Weaver find projects that fit within his program, but his other resources for narrowing down potential

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\[556\] Minutes of Officer Conference, 4/17/35, folder 2, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
grant recipients were limited. To further refine his implementation of the "new science of man," "vital processes" policy, Weaver decided to emphasize two types of programs. Both programs were intended to promote "cooperation in research." For the first type of program, Weaver sought or coordinated projects that used the latest "quantitative and analytical techniques of chemistry, physics, and mathematics" to conduct biological research. At an officers' conference on April 17, 1935, Weaver explained:

The program in experimental biology has a special interest in researches in which the analytical work and quantitative procedures of chemistry, physics, and mathematics are directed toward the solution of basic biological problems. In studying the opportunities for Foundation co-operation, the officers have been impressed by the number of important centers where physical and biological leaders are thinking and working together on such problems.557

What did Weaver mean when he referred to the application of "the analytical work and quantitative procedures" of the physical sciences to biological research? One aspect was to create "research pairings—or 'mixed marriages'—between those with expertise in chemistry, physics, or mathematics and those concerned with biological questions and skilled in handling biological materials."558 The other aspect was instrumentation. Weaver sought laboratories that were already well-supported by their universities and run by accomplished researchers, and then stocked them with the latest equipment: ultracentrifuges to separate and weigh molecules in solution, electrophoresis to separate ionic particles, spectrographs to determine the relative quantities of elements and

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557 Minutes of Officer Conference, 4/17/35, folder 2, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
compounds in a substance, Geiger counters to track radioactive isotopes, mass spectrographs to track inert isotopes, and x-ray diffractors to locate atoms within molecules. As will be shown in Chapter Five, Weaver developed this aspect of his policy in close collaboration with scientific entrepreneurs from Caltech.

The second type of program that Weaver initiated involved major grants to entire divisions or departments of biological sciences that promised to coordinate interdisciplinary projects to bring together research in "vital processes" subfields. Weaver described the ideal situation for a grant of this type:

It has been gratifying to discover that there exist groups, sometimes including practically a whole department of biology, members of which are working together on problems which fall in several of the recognized subfields of interest of the Foundation program. Grants to such groups not only serve the immediate purposes of the concentrated program, but have the added significance of building up strong centers which may long continue to exemplify the effectiveness of such an approach to biological problems. Such grants, while strictly within the spirit of the present concentrated program, preserve many of the high values attaching to more general support.

During the 1930s, three universities qualified for the major departmental grants:

California Institute of Technology (Caltech), the University of Chicago, and Stanford University. Caltech and Chicago received considerably larger allotments for biology programs than any other university, followed by Stanford, Columbia, and Harvard (in

560 Minutes of Officer Conference, 4/17/35, folder 2, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
that order. As I will demonstrate in Chapter Five, the Caltech experiment was the ultimate exemplar of the results of university and foundation “cooperation in research” initiatives in the biological sciences. The other programs had less obvious success.

Harvard’s biology department was completely overshadowed by the Medical School, and Rockefeller Foundation contributions had little lasting effect on the development of research there. At Columbia, foundation intervention had an appreciable impact on several projects, particularly research by Henry C. Sherman in nutrition and the joint project between Harold Urey and Hans T. Clarke on the biological effects of heavy hydrogen. Yet, it is difficult to assess the role that Weaver played in the Columbia projects because scientists there also received substantial Carnegie support.

The officers and trustees of the Carnegie Corporation of New York had an extremely close relationship with Columbia, to an extent comparable to the close ties between the University of Chicago and the Rockefeller boards. Frederick Keppel, the president of the Carnegie Corporation in the 1930s, had been a dean at Columbia for over a decade. The president of Columbia University, Nicholas Murray Butler, was a close friend of Keppel and a long-time member of the Carnegie Corporation board of trustees. Due in large part to the direct involvement of Keppel and Butler, the Carnegie philanthropies contributed thousands to Columbia biology before Warren Weaver began to implement

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561 CIW Grant Files, Sherman, Henry C. 1911–1932, folders 1–2, CIW Grant Files, Carnegie Institution of Washington, Washington, DC (henceforth “CIW”); Henry C. Sherman to Frank Blair Hanson, May 3, 1937, Grant summary, RF 37084, $16,500 to Henry C. Sherman for research in nutrition for three years beginning July 1, 1937; “Three Score Years and Seventeen” from Confidential Monthly Trustee Bulletin, December 1938, folder 1643, box 133, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC; Heavy Hydrogen: Urey, Rabi, Leighton, folders 1–3, CIW Grant Files, Carnegie Institution of Washington Archives, CIW; CCNY Grant Files, folder 1, box 86, Carnegie Corporation of New York Archives, Columbia; folders 1637–1639, box 133, series 200D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

his Natural Sciences policy. When the Rockefeller Foundation officers began to express interest in Columbia biology projects, they communicated with officers from the Carnegie Corporation and Carnegie Institution of Washington before making any financial commitments. Rockefeller intervention at Columbia was thus in the form of a supporting role.

The biological sciences programs that Weaver supported at Stanford University and the University of Chicago offer fitting case studies for the Rockefeller Foundation effort to engender "cooperation in research" to create a new "science of man." In both cases, the grants that Weaver approved were premised on the idea that various biological science specialties would be integrated into research programs that would address "vital processes" from multiple disciplinary perspectives. The biological science programs at University of Chicago and, to a lesser extent, at Stanford University received massive Foundation allotments because they were run by well-connected scientific entrepreneurs who promised to coordinate interdisciplinary research programs that would advance the "science of man" human engineering agenda. Like the Yale behavioral science programs, the results of these major biological science grants were mixed.

The School of Biological Sciences at Stanford received substantial funding from the Rockefeller Foundation Division of Natural Sciences in the 1930s, ostensibly to facilitate interdisciplinary cooperation among biochemists, physiologists, and geneticists. Weaver correctly perceived that Foundation intervention was a necessity for Stanford, which was in serious financial trouble by the time Taylor applied for a grant on behalf of his division in 1933. Stanford's economic struggles had begun during World War I, when expenses related to the construction of a new medical school and the costs of
mobilization had decimated the endowment fund. Beginning in 1920, Stanford’s trustees and president, Ray Lyman Wilbur, appealed to the major foundations for help. In the mid-1920s, Stanford leased land to the Carnegie Institution of Washington for its Laboratory of Experimental Taxonomy and Genetics (known as the Desert Lab) in the hopes that the relationship would lead to further investments. An associated agreement stipulated that Desert Lab researchers would also serve as adjunct professors at Stanford. One important consequence of this arrangement was the appointment of Desert Lab researcher Hermann Spoehr as adjunct professor of organic chemistry at Stanford.

When Spoehr became Director of Natural Sciences at the Rockefeller Foundation in 1930, he continued to maintain contact with his former Stanford colleagues and served as a liaison between the Foundation and the university.

In the early 1930s, Stanford lost an opportunity to receive a $750,000 conditional grant from the General Education Board (GEB) because the university trustees failed to meet the matching funds requirement. President Wilbur appealed to Spoehr, who ensured that Stanford scientists remained informed about developments at the Foundation. Nonetheless, Stanford’s applications languished until Weaver began to implement his “vital processes” program. Stanford professor C. V. Taylor contacted Weaver as soon as he heard about the new Division of Natural Sciences policy. On December 26, 1933, Taylor wrote Weaver,

> Following your statement to Dr. Blinks and me that we might apply directly through you for research aid specifically on biological problems borderline to physical science, may I enquire if your Foundation could find it possible to

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564 Ibid., 216–217.
565 Ibid., 220–222.
contribute toward the support of irradiation and other chemophysical studies on unicellular organisms here at Stanford.\textsuperscript{566}

Taylor had obviously discussed the Natural Sciences program with Weaver and he crafted his application to fit several vital processes subfields: radiation effects, physiology, and biochemistry. He also emphasized that the Stanford biology laboratories were utilizing the instrumentation of the physical sciences, thereby connecting his program with another of Weaver's initiatives.

When Weaver visited Stanford to assess the potential for biological research development, Taylor showed him an x-ray machine used to irradiate protozoa and “the finest micromanipulation equipment in the world.” The biology laboratory was also equipped with an ultracentrifuge and high vacuum equipment. Considering the financial difficulties of the past decade, the laboratory was in fine shape. Yet, Taylor asserted, the laboratories required additional ultraviolet equipment to compare the effects of radiation at different frequencies, funding for the maintenance of existing instruments, and a “microtechnician” to calibrate the microelectrodes and microthermocouples. With supplementary funds from the Rockefeller Foundation, Taylor explained, the researchers of the School of Biological Sciences would be able to conduct a vast array of experiments related to vital processes subfields. Projects included investigations into bio-electric phenomena in plant cells, ultraviolet radiation, biochemistry of microorganisms, embryonic development, the relationship of food and nutrient distribution to the growth of organs, and effects of x-ray bombardment on the production of cysts.\textsuperscript{567} Weaver was

\textsuperscript{566} C. V. Taylor to Weaver, December 26, 1933, folder 103, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{567} Weaver diary, March 1933, folder 103, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
understandably enthusiastic about the prospects for Stanford biology and the Foundation responded with a $10,000 initial grant for the year beginning July 1, 1934.\textsuperscript{568}

Over the next few years after the initial grant, Weaver and his assistant director, Frank Blair Hanson, visited Stanford multiple times to assess the progress of the various biological investigations that the Foundation was supporting. Both officers were consistently impressed with the quality of research in Taylor’s laboratories. The officers used their regular visits to convey to Taylor that Stanford would benefit from a cooperative, interdisciplinary biological sciences program that fit with Foundation policy. Taylor understood and responded with proposals for expanding the School of Biological Sciences to include genetics and endocrinology in addition to the existing disciplines of anatomy, bacteriology, physiology, embryology, and biochemistry. The Foundation officers noted in late 1934 that Stanford had tremendous possibilities in these areas and presented the School of Biological Sciences as a top candidate for a major grant:

The School of Biology at Stanford appears to be a promising development.

Within the next five years there will be concentrated here a large group of able and well-trained biologists, using the most modern physical and chemical methods. T[aylor] is now looking for a broadly trained geneticist.\textsuperscript{569}

The officers saw the potential for Stanford to become one of the “university centers of research and advanced training” that the Natural Sciences policy was designed to

\textsuperscript{568} Grant summary, RF 34052 for $10,000 to Stanford University Division of Biological Sciences, folder 103, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{569} Memo by Hanson re: visit to Stanford, November 7–8, 1934, folder 103, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
promote. Accordingly, the Foundation supplied Taylor's department with long-term grants that totaled $50,000 for the years 1935 to 1939.\textsuperscript{570}

In a statement to his fellow officers, Weaver highlighted how Stanford biological research corresponded with the goals of the Natural Sciences program:

This group does work on bioelectric phenomena, on ultra-violet radiation, on developmental mechanics, on bacterial chemistry and pigments, and on cell metabolism and development. The individual interests of the group are sufficiently distinct to preclude undue overlapping, yet the training, common objectives, and compatibility of the men are such as to insure effective collaborative activity.\textsuperscript{571}

Weaver was clearly convinced that the Stanford School of Biological Science would become a model of interdisciplinary research in "vital processes." Despite the fact that Stanford biologists made little effort to increase the level of collaboration among the various research groups, Taylor continued to promote the idea that his program exemplified the kind of cooperative, interdisciplinary work that the Foundation Natural Sciences policy encouraged in its "university centers." When Taylor applied for a considerably larger annual appropriation of $50,000 in October 1938, he claimed that the researchers "would have as their common focus the solution of problems centering around 'the properties of the simplest protoplasmic unit or primordial cell'," which would bring the disparate experiments together into a single cooperative enterprise.\textsuperscript{572}

\textsuperscript{570} Rockefeller Foundation, 1935 Annual Report, 167.
\textsuperscript{571} Officer meeting minutes, 4/17/35, folder 2, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{572} Letter from Taylor to Weaver, October 5, 1938, folder 106, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Weaver and Hanson were too impressed with the general quality of Stanford research to dispute Taylor’s claims of interdisciplinary collaboration. In a memo to the Foundation officers discussing the possibility of a large appropriation for Stanford biology beginning in 1939, Weaver noted, “In terms of general scientific strength and special program interest, we have always rated this situation along with biology at Chicago as the two strongest and most attractive opportunities in the country.”

On May 5, 1939, the Foundation officially notified Stanford of a final grant for $200,000 (equivalent to approximately $2,600,000 in 2005) to support research at the School of Biological Sciences. In the grant summary, the officers explained,

The situation at Stanford as regards the competence, research interests, and productivity of the staff, and as regards the physical facilities ... constitutes an exceptionally effective opportunity for chemophysical attack on important biological problems. ... This is an extraordinarily able group of young investigators with interests centered in modern experimental biology.

This summative statement was almost identical to the remarks about Stanford that officers had made in 1933. The emphasis on creating a university center that would be an exemplar of interdisciplinary cooperation was gone. Instead, the Foundation officers celebrated Stanford for the same reasons that they had initially supported the biological sciences there: the laboratories were well-equipped with the latest instruments and the department was staffed by promising researchers. Some of the officers might have felt

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573 Memo by Weaver re: Stanford University—School of Biological Sciences, March 2, 1939, folder 107, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
574 Letter from Norma Thompson, Rockefeller Foundation secretary, to Ray L. Wilbur, May 5, 1939, folder 107, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
575 Grant summary, RF 39035 for $200,000, 4/5/39, folder 103, box 8, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
some disappointment about the lack of explicit correlation between Stanford biology and Foundation Natural Science policy objectives, but they could not deny the overall success of the program. In the instance of Stanford, then, the role of the Foundation was to help rescue a university that was struggling financially and to encourage and equip a promising set of researchers who were conducting experiments related to "vital processes" subfields. Whether or not Stanford served as an example to other universities in terms of collaborative research became immaterial.

The Stanford School of Biological Sciences received sizeable grants from the Rockefeller Foundation in the 1930s, but the allotments were miniscule compared to the grants that the University of Chicago Division of Biological Sciences received during the same time period. Rockefeller Foundation intervention in the biological sciences at the University of Chicago was more extensive than in any other university, with the notable exception of Caltech. The Foundation's heavy investment in Chicago biology was the product of a long history of collaboration between Rockefeller philanthropy and Chicago science. By the time Weaver inaugurated his "new science of man," "vital processes" program in 1933, Chicago scientists had already spent many years working closely with representatives of various Rockefeller philanthropic interests. Prior to the 1930s, University of Chicago biologists had built close relationships with officers from both the General Education Board (GEB) and the Rockefeller Foundation. When Weaver became involved, he was able to ensure with little difficulty that the Chicago Division of Biological Sciences research program conformed exactly to the expectations of the Rockefeller Foundation, particularly his efforts to promote "cooperation in research" and to achieve his version of human engineering goals. The ease with which Weaver
developed a productive working relationship with Chicago biologists was facilitated by the lengthy pre-existing collaboration between Rockefeller officers and Chicago biologists.

Beginning in the late nineteenth century, scientists at the University of Chicago relied on support from the Rockefellers. Between 1892 and 1920, the Rockefeller family gave a total of $35,000,000 (equivalent to approximately $500,000,000 in 2005) to Chicago—including $20,000,000 for endowment, $10,000,000 for buildings and equipment, and $5,000,000 in general funds—a sum that more than doubled the combined contributions of all other donors.\textsuperscript{576} During that period, the University of Chicago became established as a prominent research institution for the experimental life sciences due to a combination of the availability of ample resources and the leadership qualities of the successive chairmen of the Department of Biology (later the Division of Biological Sciences).\textsuperscript{577}

The first chairman of the Department of Biology, C. O. Whitman, ensured that the disciplines of zoology, anatomy, physiology, and neurology were well-supported to the extent that each specialty split into a separate department within the first year after the founding of the University. Whitman, who was one of the top zoologists in the United States, bolstered Chicago’s reputation as a locus of professional biological research by serving as the director of the foundation-sponsored Marine Biological Laboratory at Woods Hole (MBL) for the first twenty years of its existence (1888–1908).\textsuperscript{578} When

\textsuperscript{576} "University of Chicago: Conclusions" by Thomas Appleget, May 1939, folder 6843, box 657, subseries 4, series 1, General Education Board Archives, RAC.


\textsuperscript{578} "The Department of Zoology" by Frank R. Lillie, submitted to President’s Office, December 24, 1919, folder 8, box 8, series III, Frank R. Lillie Presidential Papers, University of Chicago Archives, Chicago, IL (henceforth “Chicago”).
Whitman died suddenly in 1910, he was succeeded by his former student Frank R. Lillie (1870–1947), who also became the director of the MBL. Lillie served as the chair of the Department of Zoology until 1931, when he was appointed as the first Dean of the Biological Sciences.

Lillie, who had earned his Ph.D. at Chicago with Whitman in 1896, launched his accomplished career as a zoologist when he returned to his alma mater as a professor in 1900. Within a decade of his return to Chicago, Lillie began to outshine his former advisor. As Adele Clarke argues, “Lillie’s research on the freemartin was probably the most famous piece of reproductive science to come out of the Department of Zoology at the University of Chicago.”\textsuperscript{579} The freemartin is a sterile, female, fraternal twin of a male that shares placental vessels with her twin in utero. Lillie noted that cow freemartins began as genetic females but developed male genitalia due to their exposure to hormones through the placenta. In 1917, Lillie published his conclusions: hormones were primarily responsible for sexual development. The freemartin research undermined strict hereditarian beliefs and heightened interest in the role of hormones in human reproduction. Over the next two decades, Lillie followed this line of research to show how sexuality was both genetically and physiologically determined, and to provide insights into the nature and function of hormones.

Lillie's research led him directly to the NRC, where he became a founding member of two groups in 1921: the Committee for Research in Problems of Sex and the Eugenics Committee, both chaired by Robert Yerkes.\textsuperscript{580} Through his work on these committees, Lillie forged personal relationships with foundation officers and had found

\textsuperscript{580} Folder 8, box 3, series I, Frank R. Lillie Presidential Papers, Chicago.
an ideal forum to promote Chicago biology. Lillie also became closely associated with
the eugenics movement through the Eugenics Committee, which caused some of his
fellow biologists to question his professionalism. It is interesting to note that, although
Lillie's hormone research clearly undercut the scientific claims that eugenicists made
about heredity, he continued to ally himself with the social and political goals set forth by
the eugenics movement. Lillie was committed to the promises of human engineering.
However, he also believed that the oversimplified, biometrics-based version of eugenics
was justifiably criticized as unscientific. In 1923, he abandoned his formal affiliation
with the Eugenics Committee and began referring to his research interests as "genetic
biology" rather than eugenics.

Lillie soon began to campaign for foundation support for his "genetic biology"
research at the University of Chicago. In 1924, he wrote to Wickliffe Rose of the
General Education Board (GEB) to suggest that "genetic biology" was the field of the
future and was worthy of financial support to the greatest extent possible:

By this I mean not merely research in heredity of the kind so brilliantly developed
by T. H. Morgan in this country; but also, and very especially, work in the
physiology of reproduction, in the biology of sex, in the physiology of
development, and in experimental evolution. . . . The practical application of this
field is to the human society of the future. . . . We are at a turning point in the
history of human society . . . the populations press on their borders everywhere,
and also, unfortunately, the best stock biologically is not everywhere the most
rapidly breeding stock. The political and social problems involved are fundamentally problems of genetic biology.\textsuperscript{581}

Lillie was using arguments that were incredibly appealing to foundation officers in the 1920s (and beyond), especially that his research would contribute to the future of human society and help to solve "political and social problems." Yet, his prescription for the application of "genetic biology" still looked strikingly similar to eugenics. In fact, when Lillie followed his letter with an application to the GEB for a grant to support an "Institute of Racial Biology," Rose rejected it largely because he was uncomfortable with the eugenic implications. That same year, though, the GEB officers showed their support for University of Chicago scientific research by appropriating $2,000,000 (equivalent to $21,700,000 in 2005) for endowment.\textsuperscript{582} Interestingly, in his application for the GEB grant, Ernest DeWitt Burton, the president of the University of Chicago from 1923 to 1925, had referred specifically to Lillie and his colleagues in the biological sciences as exemplars of innovative researchers.\textsuperscript{583} Therefore, despite the reluctance that GEB officers had shown regarding support for the Institute for Racial Biology, they had ultimately taken Lillie's research into consideration when appropriating the massive endowment grant. In visits to the University of Chicago during the 1920s, the GEB officers always stopped to see Lillie.\textsuperscript{584}

\textsuperscript{581} Lillie to Rose, June 17, 1924, folder 104, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{582} General Education Board to Ernest DeWitt Burton on May 28, 1934, folder 6839, box 657, subseries 4, series 1, General Education Board Archives, RAC.
\textsuperscript{583} Burton to GEB on May 5, 1924, folder 6839, box 657, subseries 4, series 1, General Education Board Archives, RAC.
\textsuperscript{584} Memo by Thorkelson titled "University of Chicago," June 20, 1924, folder 6839, box 657, subseries 4, series 1, General Education Board Archives, RAC; "University of Chicago," appraisal by Thorkelson for the Executive Committee Meeting, December 21, 1926, folder 6840, box 657, subseries 4, series 1, General Education Board Archives, RAC.
In 1925, future Rockefeller Foundation officer Max Mason took over as president of the University of Chicago, and he immediately began to solicit additional General Education Board (GEB) support for scientific research, including Lillie’s biology department. Mason exchanged frequent visits and letters with GEB officers in which he argued that Chicago needed new laboratories, equipment, and endowment funds to improve the physical and biological sciences.\textsuperscript{585} He repeated the arguments that Lillie had made in applications and found that the GEB officers were convinced by the proposals. In 1927, the GEB appropriated $1,000,000 (equivalent to $10,500,000 in 2005) to the University of Chicago “to improve existing facilities for graduate instruction and research in the physical and biological sciences.”\textsuperscript{586} By 1928, then, the GEB had provided millions in endowment and building funds for Chicago science. Still, only part of the funds had been designated for the biological sciences.

In early 1929, Lillie resumed his inquiries about his Institute of Racial Biology and the possibility of funding specifically for a biological sciences program. His timing was perfect. Administrative reorganizations at both the University of Chicago and the Rockefeller philanthropies had created a highly receptive environment for Lillie’s requests. Max Mason had resigned as president of the University of Chicago to assume the job of director of the Division of Natural Sciences at the newly reorganized Rockefeller Foundation. He was replaced at the University of Chicago by Robert Hutchins, who became a close friend. Mason ensured that Chicago biology was a top Foundation priority for years to come, and on May 22, 1929, the Rockefeller Foundation

\textsuperscript{585} Folder 6862, box 660, subseries 4, series 1, General Education Board Archives, RAC.
Division of Natural Sciences appropriated $150,000 for Chicago biology for a five year period beginning July 1, 1929.\footnote{Grant summary, RF 29083 for $150,000, 5/22/29, folder 103, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.}

Lillie saw the appropriation as a new opportunity to promote his Institute of Racial Biology. He understood that eugenics was considered obsolete, and even offensive, by most scientists and by the Rockefeller Foundation. Nonetheless, he also perceived that University of Chicago administrators and Foundation officers, while dismissing the term "eugenics," were still committed to the overall goal of using natural and social science to serve the cause of "racial betterment." In other words, Lillie was cognizant of and enthusiastic about the human engineering effort that the Foundation was advancing throughout the early twentieth century, and he believed that he could channel the interest in improving mankind toward his proposed institute.

Over the course of the 1930s, Lillie corresponded frequently with Rockefeller officers about the racial betterment project and clearly connected foundation human engineering goals to his research interests. He remained sensitive to the controversial nature of his proposals, and worked hard to formulate rationales for his research in "racial biology" that were not explicitly tied to eugenics. As Lillie explained in a report on June 23, 1930:

The proper field of a university zoological department is, in my opinion, evolution and racial biology. I am convinced that the subjects have a very great present social significance, and that in the future we must have institutes of racial biology where the problems of genetic biology and physiology will be studied . . . I do not mean that the Department of Zoology should be turned into an institute of
eugenics, for much of the propaganda along such lines is ill-advised and ill-timed.

But the fundamental biological subjects on which racial betterment depends should be studied for its own sake.\(^{588}\)

He proceeded to outline an interdisciplinary biology program that embodied many of the same principles and fields that the Rockefeller Foundation would embrace in its Division of Natural Sciences policies: genetics, cytology, embryology, and biology of sex. The Department of Zoology already had excellent researchers in the field of genetics, H. H. Newman and Sewall Wright, but Lillie argued that "we should have an additional member of the Department in this field, which is one of the most important in reference to evolution and racial problems." Embryology and the biology of sex were also especially significant:

Indeed the work in embryology, which includes my own personal interests, has the widest philosophical implications. It deals with a part of life that is determinative for all. How far it can be brought under control is of great social importance.\(^{589}\)

Lillie added that the goals for the interdisciplinary program included cooperation with social sciences and psychology. For this comprehensive and socially relevant program to develop adequately, Lillie explained, he would need at least $1,000,000 for buildings, supplies, and appointments. He and Hutchins turned to the Rockefeller Foundation for the necessary funds. They knew that Chicago biological sciences had a favorable audience with Mason and that their project proposal fit well with existing Foundation policy goals. As the Rockefeller Foundation officers continued to refine their Division of

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\(^{588}\) Lillie to Hutchins, June 23, 1930, folder 2, box 109, series II, University of Chicago Presidents' Papers, 1925–1945, Chicago.

\(^{589}\) Ibid.
Natural Sciences policies and the University of Chicago administrators continued to experiment with academic reforms in the early 1930s, the fit between Chicago biology and Foundation goals became even better.

Beginning in 1930, Hutchins inaugurated a restructuring of University of Chicago science “to co-ordinate research, to make it convenient and natural for men to work together, and to facilitate the planning of research on a broader scale than was possible in a single department.” Under Hutchins’ new plan, the science departments would be consolidated into divisions to maximize laboratory space and interdisciplinary cooperation. As Hutchins explained in a letter to the Rockefeller Foundation officers, “Each department retains a reasonable degree of autonomy, but the divisional organization provides excellent means for integrating and correlating the work of the several departments.” Hutchins was promoting the “cooperation in research” ideal that Mason was advancing at the Rockefeller Foundation.

In 1931, Lillie was appointed dean of the new University of Chicago Division of Biological Sciences, which consolidated the various life sciences departments. In their correspondence with Rockefeller Foundation officers, Lillie and Hutchins promoted their Division of Biological Sciences as a model program, an interdisciplinary attack on the fundamental questions about human life. One of Lillie’s first actions as dean was to write to Mason to apply for additional funds to support the interdisciplinary program. Again, Lillie suggested that the best route would be to establish an Institute of Genetic Biology and Evolution, which was a fresh name for a stale Institute of Racial Biology application. One important addition to the application was the inclusion of the biochemistry of

591 Hutchins to Arnett, April 9, 1931, folder 6842, box 657, subseries 4, series 1, General Education Board Archives, RAC.
hormones and the biophysics of radiation as research specialties.\textsuperscript{592} Mason responded enthusiastically to Lillie’s proposals and many of the ideas in the letter were soon incorporated into Foundation Division of Natural Sciences policies. Yet, the application was rejected. Mason asked that Lillie resubmit the Institute application in a year because the existing commitments to Chicago were already so great, not to mention the support that Lillie was receiving through the NRC Committee for Research on Problems of Sex.

Lillie applied again for his Division of Biological Sciences in February 1932, and included a request for the Rockefeller Foundation to separate his research funding from the NRC Committee on Sex Problems. He contended that the Foundation should support his sex research directly by folding it into a larger grant for an Institute of Biology.\textsuperscript{593} The Foundation officers were unsure how to handle the situation, especially considering that the current NRC Committee on Sex Problems grant for $30,000 annually was guaranteed through 1933. At a staff conference, the officers explained that Lillie’s proposal presented a “difficult” dilemma because the “present state of the University” was “largely the result of contributions already made” and future prospects seemed “particularly hopeful,” but the idea of offering two grants to the same researcher for the same project seemed untenable.\textsuperscript{594} They postponed the application for another year.\textsuperscript{595} Meanwhile, Weaver had taken over as director of the Rockefeller Foundation Division of Natural Sciences and had begun to develop his “vital processes,” “new science of man”

\textsuperscript{592} Lillie to Mason, June 5, 1931, folder 104, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{593} Lillie to Mason, February 16, 1932; Letter from Lillie to Mason, February 20, 1932, folder 104, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{594} Conference notes, April 2, 1932, folder 104, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{595} Weaver to Lillie, April 6, 1932, folder 104, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
policies. He soon turned to Lillie’s program at the University of Chicago Division of Biological Sciences as a source of inspiration.

Weaver spent several days at the University of Chicago in September 1933 for the express purpose of devising a long-term plan to finance Lillie’s Division of Biological Sciences, which he hoped to use as a model for other “vital processes” policy initiatives. Over the course of the visit, Weaver and Lillie decided that the best route would be to roll the University of Chicago portion of NRC Committee on Sex Problems funds into an annual grant of $50,000 that would also support the Division of Biological Sciences as a whole. Weaver promised to return in November to finalize the plans with Lillie.\footnote{Weaver diary re: meeting with Lillie at University of Chicago, September 8–11, 1933, folder 105, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.}

During the November visit, Weaver confirmed that plans for an Institute would be replaced by an interdisciplinary program within the Division of Biological Sciences and that Lillie would receive his biology of sex funding directly from the Foundation rather than through the NRC Committee on Sex Problems.\footnote{Weaver diary re: visit with Lillie at University of Chicago, November 6, 1933, folder 105, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.} When Weaver wrote Hutchins to congratulate him on the $50,000 annual grant, he stressed that Lillie had demonstrated the correspondence between research in the Chicago Division of Biological Sciences and the new policies for the Rockefeller Foundation Division of Natural Sciences:

I have at various times had the pleasure of discussing with Dean Lillie the present program enthusiasms of this division. Dean Lillie understands that support is being concentrated in the field of vital processes, with special emphasis on the sub-fields of psychobiology, internal secretions, nutrition, radiation effects, biology of sex, experimental and chemical embryology, genetics, general
physiology, biophysics and biochemistry. I mention this fact because it is our common understanding that the researches to be supported by this grant will fall within this general field. 598

Many of Lillie’s projects fell within the parameters of the vital processes program before it even existed. This was not coincidental: the University of Chicago and Rockefeller Foundation biology programs developed in tandem due to the close collaboration between Lillie and Weaver. It was therefore unsurprising that Weaver found much to celebrate in the Chicago program:

The Division of Biological Sciences at the University of Chicago is unusual, if not unique, in that it embraces the whole range of pre-clinical and clinical subjects. Under the leadership of Professor Lillie there has been developed one of the world’s foremost centers for biological research. The physiology, biochemistry, and genetics of sex there receive particularly able and energetic attention, as do other fields which contribute directly to psychobiological knowledge. With the assistance of past grants, the Chicago group has obtained results of the highest importance. The future work of this center will play a leading role in this development of the Foundation’s new program. 599

Weaver was explicit about his dedication to University of Chicago biological sciences. His statement that “the future work” at Chicago would “play a leading role” in the development of his Natural Sciences program could not have been clearer.

598 Weaver to Hutchins, December 19, 1933, folder 105, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
599 Grant summary, RF 33105 for $50,000, 12/13/33, folder 103, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
As the June 30, 1935 expiration date for the existing $50,000 annual grant neared, Weaver joined Lillie in claiming that the fate of the Chicago Division of Biological Sciences depended upon continued Rockefeller Foundation intervention. As Weaver told his fellow officers,

Future support to this recognized center is considered to be one of the outstanding opportunities in the Natural Sciences program. Dean Lillie writes that the Foundation grant supports much of the most important research work in the preclinical departments.600

After Weaver presented his case, the Foundation appropriated $150,000 for the three-year period beginning July 1, 1935 to “aid in the research activities of approximately twenty members of the Chicago faculty working in various fundamental fields of biology.”601 A grateful Lillie reassured the Foundation officers that the money was well-spent:

The significance of the present aid in the acceleration of basic work and the furtherance of interdepartmental cooperation has been repeatedly emphasized in these reports. With the inevitable curtailment of the University’s research budgets during the past five years [due to the Great Depression], these benefits have become increasingly apparent. With the addition of the work on sex the present grant is by far the greatest single material factor in the development of basic scientific work in the non-clinical departments of the Division of the Biological Sciences.602

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600 Minutes for officer meeting, 4/17/35, folder 2, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.
602 “Report for 1934–35 on the Grant for Biological Research Made by the Rockefeller Foundation,” November 8, 1935, folder 107, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Ostensibly, Foundation support for the Division of Biological Sciences at Chicago was justified in every conceivable way: it was an interdisciplinary, cooperative program dedicated to "vital processes" subfields that could serve as the "university center" for the Midwest.

In addition to the Division of Biological Sciences projects and grants, Weaver and Lillie collaborated to initiate a project that would link the biological and physical sciences to conduct experiments using spectroscopic analysis. The spectroscopy project began in January 1934, when Mason sent Weaver a telegram advising him that he should visit Chicago to see the "photoelectric spectrophotometer" that two young plant physiologists, Frederick Zscheile, Jr. and Elmer S. Miller, had constructed while serving as NRC research fellows. Weaver immediately went to the campus and was fascinated by the device, which measured the absorption spectra of plant pigments with extraordinary accuracy, but he informed them that the Foundation "had no interest in promoting work on plant substances." To secure Foundation support, Zscheile and Miller agreed to shift their research focus to the spectroscopic analysis of enzymes, vitamins, and hormones. Weaver was still not satisfied, however, because Foundation policy dictated that experienced, accomplished researchers should direct projects. As he wrote Lillie on January 22, 1934, "Both of them are young and relatively inexperienced. . . It is clear that the situation would be very much more promising and attractive to us if a project could be carried on under the general direction of an experienced member of your faculty." In response, Lillie recruited Thorfin R. Hogness, a physical chemist, and F. C. Koch, a biochemist, to run the project. When Lillie submitted the official

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604 Weaver to Lillie, January 22, 1934, folder 166, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
application, the request was for funds to support a "spectroscopic investigation of human body fluids and tissues" run by Hogness and Koch; Zscheile and Miller would serve as technical assistants. To appease Weaver, Lillie had transformed the plant physiology spectroscopy project that Zscheile and Miller had worked hard to create into human engineering-related investigation staffed by new personnel.

Weaver's participation in the spectroscopy project only increased. After he secured a grant of $11,750 for the spectroscopy project in February 1934, Weaver made it clear that he intended to remain intimately involved in the research program: "This project is the opening step of a campaign which the officers propose to engage in actively." He certainly fulfilled his promise. Weaver visited Chicago soon after the grant went into effect and met with the researchers. As his notes reveal, Weaver directly influenced the research problems that Hogness and Koch chose to address. He wrote that he was "much gratified at the active interest which Hogness and Koch are taking in the program in spectroscopic biology at Chicago" and "especially in the fact that the principal enthusiasm seems to have turned from chlorophyll to the analysis of hormones." This statement did not convey the full story, though. In 1935, Weaver learned that the biologists and physical chemists were having trouble cooperating because Hogness was annoyed that Koch seemed obsessed with finding new sex hormones. Since one of Weaver's primary interests was to use spectroscopic analysis to trace the relationships between abnormal sexual development and hormone secretions, he set about the task of changing Hogness's mind.

605 Grant summary, RF 34020 for $11,750, 2/16/34, folder 166, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC; Rockefeller Foundation, 1934 Annual Report, 138–139.
606 Weaver diary re: University of Chicago visit with Hogness, Koch, Zscheile, Miller, July 9, 1934, folder 166, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Weaver (using Foundation money) sponsored a six-month trip tour of European laboratories for Hogness to help him learn more about the spectroscopic analysis of biologically relevant substances and molecules. Upon his return, Hogness initiated projects based on spectroscopic techniques that he learned in Otto Warburg's laboratory in Germany.\(^\text{607}\) Weaver was reassured about the project and enthusiastic about the direction of Hogness's research:

Professor Hogness has become so convinced of the importance of these new spectroscopic methods that he proporses to devote his entire energies to work with biological materials . . . Work planned for the near future includes the isolation of a new hormone found in the pancreas, the continuation of work on the nature of the Vitamin D’s, the attempt to isolate Vitamin A, the determination of the chemical structure of the newly found sterility Vitamin E, the differences between normal and pathological blood, the differences between normal and pathological body tissues, and many other problems of fundamental importance to biology and medicine. Of all the Foundation ventures in the applications of spectroscopic methods, this project is outstandingly the most successful and productive.\(^\text{608}\)

The Foundation appropriated $14,000 for the year beginning July 1, 1935 for the project, followed by a grant for $42,800 for the four years beginning July 1, 1936.\(^\text{609}\)

Over the next decade, Weaver would refer to Chicago spectroscopy research as his model for numerous other similar projects in the United States and Europe. Weaver's overriding concern became interdisciplinary cooperation between the physical and


\(^{608}\) Officer meeting minutes, 5/15/36, folder 168, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC; Rockefeller Foundation, *1935 Annual Report*, 139.

biological sciences, which was certainly influenced both by Chicago spectroscopy and by Linus Pauling’s research at Caltech. In fact, Weaver and Pauling corresponded regularly about the implications of Hogness’s spectroscopic research and the possibility of creating a similar program at Caltech. Pauling wrote Weaver on May 12, 1936 after learning about the $42,800 grant:

I have again looked into the spectroscopic work on biological substances which Hogness is doing at Chicago. I am greatly impressed by its importance and promise. It occurs to me that this work could be a most important part of our program of applying new physico-chemical methods to biological problems. . . . What is your advice in regard to this? Do you think that we could start to formulate a plan involving a somewhat larger amount of physico-chemical work on biological substances than we are now carrying on?610

Weaver responded that he thought incorporating research akin to Hogness’s at Chicago would be an excellent idea for Caltech. As will be demonstrated in the next chapter, Weaver strongly influenced Pauling’s research in the “borderline” areas between the physical and biological sciences, and the Chicago project was a likely source of inspiration.

Weaver’s enthusiasm about Chicago biological sciences continued to build, and his pledges of support became even more substantial. In the summer of 1936, Weaver began writing to William H. Taliaferro, who had taken over from Lillie as dean of the Division of Biological Sciences in 1935, to suggest that the Foundation would consider supplying endowment funds for Chicago biology. The endowment would mean that the

610 Pauling to Weaver, May 12, 1936, folder 168, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Rockefeller Foundation would provide a permanent contribution to Chicago biological science, which was a remarkable offer. As Taliaferro explained to Hutchins,

Regarding the question of putting our present grant on a permanent basis, Dr. Weaver feels that it has become such an integral part of our whole research program that it cannot be decreased to support new work, but rather it should be increased to support the work for which it is now used.\[^{611}\]

Weaver was actually encouraging the University of Chicago biologists to increase the amount that they were requesting! Taliaferro and Hutchins were naturally thrilled at the possibility of securing endowment funds, and they assured Weaver that they would meet any conditions. On February 16, 1937, Weaver presented his argument on behalf of an endowment grant for Chicago to his fellow officers. First of all, Weaver explained, the Foundation had already been funding the Division of Biological Sciences for many years, as well as “a program in the application of spectroscopic methods to the study of organic substances, such as hormones and vitamins, of importance to biology.” He continued,

The University of Chicago presents an outstanding—probably the outstanding—opportunity for advancing the present Natural Science program. The current grant of $50,000 annually is actually not enough satisfactorily to provide for the long-term research activity of the many groups involved. Still less is it sufficient to provide for programs important to biology but arising outside the biological departments. . . . A great university like Chicago has a responsibility for maintaining a considerable program in the more traditional fields of biological

\[^{611}\] Taliaferro to Hutchins, August 3, 1936, folder 4, box 109, series III, University of Chicago Presidents’ Papers, 1925–1945, Chicago.
research... but our program also seeks to develop the newer researches which occupy the border regions between biology and the physical sciences.\textsuperscript{612}

Weaver and Taliaferro began to correspond regularly about the best way to craft a successful application for endowment funding. In particular, Weaver urged Taliaferro to indicate clearly how Chicago research corresponded with the priorities of the Foundation Division of Natural Sciences program.\textsuperscript{613} From December 1937 through February 1938, Weaver and his assistant director, Frank B. Hanson, helped Taliaferro hammer out the details of a conditional endowment grant in which the Foundation would provide $1,500,000 and Chicago would raise $500,000, with an additional three year grant of $180,000 to sustain the program while the university raised the matching funds.\textsuperscript{614}

When the Foundation approved the grant in early 1938, Weaver contacted Lillie to congratulate him even before he officially notified the Chicago administration:

I have just been dictating a letter to President Hutchins informing him concerning a recent action of the Trustees of the Rockefeller Foundation pledging $1,500,000 for endowment of biological research at Chicago on condition that the University secure by June 30, 1941, $500,000 for the same purpose... and also concerning an interim grant which provides up to $60,000 annually for biological research over the next three years. I cannot let this occasion pass without dropping a note to you. I do not need to tell you how great a role your own scientific and

\textsuperscript{612} "Support of Biological Research—University of Chicago," memo by Weaver, 2/16/37, folder 108, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{613} Weaver to Taliaferro, March 25, 1937, folder 108, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{614} Weaver to Taliaferro, December 29, 1937, folder 108, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC; Taliaferro to Hanson, January 11, 1938; Hanson to Taliaferro, January 14, 1938; Taliaferro to Hanson, January 25, 1938; Hanson to Weaver, January 28, 1938; Weaver to Hanson, January 29, 1938; Taliaferro to Hanson, February 1, 1938; Hanson to Taliaferro, February 5, 1938, folder 109, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
administrative leadership has played in building up this fine program to such a
point that our Trustees were willing to contribute so largely toward its
stabilization. This is not exactly the plan which you and I have had the pleasure
of discussing so many times in the past, but I feel confident that you will be happy
to receive this news. It is even more clear to me that you have the right, beyond
that of any other individual, to learn this news with deep personal satisfaction.\textsuperscript{615}

Weaver saw the endowment as the culmination of a collaborative enterprise between his
Division of Natural Sciences at the Rockefeller Foundation and the Division of
Biological Sciences at the University of Chicago as designed by Lillie. The fact that
Taliaferro was the current Dean was less important than the fact that many years of close
cooperation between the Foundation and the University of Chicago had yielded a
program that both organizations could celebrate.

In his summary of the endowment grant for Foundation officers and trustees,
Weaver traced the history of biological sciences at Chicago from C. O. Whitman to Lillie
and Taliaferro. Throughout its history, according to Weaver, the leaders of Chicago
biology had intended to create an interdisciplinary, cooperative research program that
addressed issues central to the welfare of humanity. In his teleological account, the
Division of Natural Sciences endowment grant had provided the final necessary element
of an ideal program:

The Division of Biology at Chicago not only contains the usual interests in
zoology and botany, but also includes the preclinical fields of biochemistry,
physiology, bacteriology, and anatomy. Within this wide area—the whole natural

\textsuperscript{615} Weaver to Lillie, April 21, 1938, folder 109, box 8, series 216D, Record Group 1.1, Rockefeller
Foundation Archives, RAC.
and proper field of the life sciences—it has been found possible to develop a balanced and integrated research program of such high character that, viewed as a whole, this staff probably includes the leading group of biological investigators in the country. . . . [Rockefeller Foundation] support, which has already existed at or above the present level for some seven or eight years, is now thoroughly knit into the scientific life of the whole biological group at Chicago. . . . The research program in biology at Chicago places major emphasis upon those studies,—utilizing the most modern experimental techniques and drawing eagerly upon chemistry, physics, and mathematics for assistance,—which are basic to forwarding our understanding of himself.\footnote{616}

This statement encapsulated the intimate connections between Chicago biology and the Rockefeller Foundation Natural Sciences program. Since Weaver avoided specifics, though, it was unclear exactly how the Chicago program contributed to the project of defining a new "science of man."

In 1939, the University met the requirements of the conditional grant with a pledge from the Clara A. Abbott Trust and received the $1,500,000 from the Foundation. The endowment funds supported a variety of projects, with genetics and spectroscopic biology receiving the largest allotments.\footnote{617} After this point, the research projects with the closest correlations to the Foundation Natural Science program yielded mixed results.

For example, Charles Manning Child was a biologist who took an interdisciplinary

\footnote{616} Grant summary, RF 38036 for $1,500,000 conditional on matching funds of $500,000 and RF 38037 for $180,000, 4/6/38, folder 103, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.

\footnote{617} "Report of Research Under the Dr. Wallace C. and Clara A. Abbott Memorial Fund of the University of Chicago," 1939–1940, folder 114, box 9, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
approach to the study of human behavior. During his twenty-one years as a professor in the Department of Zoology at Chicago (1916–1937), Child had constructed a theory of genetics that “pointed toward an integration of biology, psychology, and sociology within a progressive evolutionary worldview that placed at the pinnacle of development the institutions and values of Anglo-American culture.” Since Child retired in 1937, he was unable to benefit from the permanent endowment grant. As another example, the prospect of interdisciplinary sex research projects had stalled. Beginning with his breakthrough research on the freemartin, Lillie had the potential to build lasting programs dedicated to questions of fundamental importance for reproductive science and endocrinology. Yet, when he retired, the hormone research group that he had led effectively collapsed. After Lillie and Child retired, the Department of Zoology moved more and more toward exclusive devotion to genetics research, led by Sewall Wright. In 1931, Wright had published his most influential paper, “Evolution in Mendelian Populations,” in which he argued that gene frequency and attendant selection mechanisms were dependent upon variations observable only in large populations over long periods of time. He spent the remainder of his career developing a “quantitative theory of gene action in the case of the coat color of the guinea pig.” Wright continued to be a leader in his field, but his research was hardly the result of interdisciplinary cooperation. The same could be said of C. R. Moore, who remained at the height of the

620 “Biology Departments and Personnel in the Division of Biological Sciences at the University of Chicago,” May 1938, folder 109, box 8, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
fields of reproduction and endocrinology throughout his career but never cooperated with other investigators within his department.\textsuperscript{621}

Weaver’s major grants programs, which were designed to further interdisciplinary cooperation among researchers in the various biological sciences specialty fields at Stanford and the University of Chicago, helped to build solid overall divisions of biological sciences at the two universities but did not create many opportunities for scientists to transcend their disciplinary boundaries. Like the behavioral scientists at the Yale Institute for Human Relations discovered, it was neither practical nor particularly advantageous for most researchers to put energy into interdisciplinary cooperation. The major exception to this generalization was the collaboration between physical and biological scientists that Weaver orchestrated through his “mixed marriages” initiative.

The spectroscopy research conducted by Hogness, professor of physical chemistry, and Koch, professor of biology, exemplified the kind of interdisciplinary project that Weaver emphasized in the mid-to-late 1930s. In 1939, the Foundation officers described the project as “a striking example of the successful application of instruments and techniques of physical chemistry to biological problems.”\textsuperscript{622} As the Caltech case study in Chapter Five will illustrate further, the role of the Foundation in supplying instruments including electron microscopes, ultracentrifuges, and spectroscopes, as well as the equipment necessary to complete experiments using x-ray crystallography, ultraviolet radiation, and electrophoresis, had a tremendous impact on biological science. The role of Weaver and the Rockefeller Foundation in encouraging the application of the instrumentation of the physical sciences to biological research was

\textsuperscript{621} Horatio Hackett Newman, “History of the Department of Zoology at the University of Chicago,” 233.  
\textsuperscript{622} “New Knowledge of Respiration,” Trustees Confidential Monthly Bulletin, October 1939, folder 171, box 12, series 216D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
of far greater importance than the attempt to cultivate a "science of man" through a program in "vital processes." Weaver's real innovation was to transform the human engineering effort into policies that encouraged the use of fine-tuned instruments to dissect and analyze the "physico-chemical processes" and molecular structure of organisms.
The rise of the new [molecular] biology was an expression of the systematic cooperative efforts of America’s scientific establishment—scientists and their patrons—to direct the study of animate phenomena along selected paths toward a shared vision of science and society. – Lily E. Kay, *The Molecular Vision of Life* 623

Chapter Five

“Cooperation in Research” at the California Institute of Technology

During the 1930s and beyond, California Institute of Technology (Caltech) professors, led by Linus Pauling, made immense strides in genetics and bio-organic chemistry. Their stellar achievements were made possible by the development at Caltech of unusually close working relationships among scientists, university administrators, and officers of philanthropic foundations, especially from the Rockefeller Foundation. Although the Great Depression left many academic programs struggling for survival, Caltech experienced a period of vigorous growth and innovation in the 1930s, largely due to the continuous infusion of philanthropic foundation money. As described in the previous chapter, other universities received ample foundation support for biology programs, but Warren Weaver and the Rockefeller Foundation contributed more to Caltech science, in terms of a combination of ideas and capital, than any other institution.

Throughout the interwar period, Caltech scientists constructed laboratories and designed research agendas in close cooperation with foundation officers. Foundation investments in the 1920s helped Caltech develop from a struggling technical college into a prestigious research institution. Using philanthropic funds, three of the nation’s most adept scientific entrepreneurs—NRC leaders George E. Hale, Arthur A. Noyes, and Robert A. Millikan—had joined together to organize and direct the development of science at Caltech. By the early 1930s, when Warren Weaver became heavily involved

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in the building of Caltech research, the "triumvirate" of Hale, Noyes, and Millikan had established world-class physical science programs and were looking to expand the biological science programs. Caltech provided an outstanding opportunity for Weaver to test his Division of Natural Sciences policies and, ultimately, to adjust his program to reflect what he learned from the scientists there.

In the process of negotiations with Weaver, Caltech scientists officers created a model biological sciences program that became a top research and training center in the fields of genetics, molecular biology, and biochemistry. The Caltech biological science projects utilized the instruments and methodology of the physical sciences to an unprecedented degree, especially in Linus Pauling's laboratory. The successes that Weaver and his fellow officers witnessed at Caltech helped them refine Natural Science policies, while the continuous intervention by Weaver encouraged Caltech scientists to mold their projects to suit Foundation priorities. Representatives from the two institutions—Caltech and the Rockefeller Foundation—built increasingly close personal and professional relationships as they cooperated to create what become one of the most renowned institutions in the world in the biological sciences.

The scientists at Caltech were especially receptive to Weaver's Natural Science policies because they had always made "cooperation in research" a top priority. In correspondence, grant applications, and published documents, both foundation officers and Caltech leaders celebrated "cooperation in research" and the goal of moving fluidly across disciplinary boundaries. In the formative years of Caltech, "cooperation in research" had meant primarily physicists and chemists working as a group to coordinate research projects and to run the administration of the university. In was only later, after
significant foundation intervention, that “cooperation in research” came to mean incorporating the methods and instruments of physics and chemistry to investigate the structural mechanics of living beings. However defined, “cooperation in research” paid off handsomely. The physical sciences-based approach and the impetus to cooperate yielded valuable insights into the structure of molecules and the nature of biochemical reactions, particularly Linus Pauling’s revelations about the structure of antibodies, hemoglobin, protein, and DNA.

In the only monograph dedicated to the history of Caltech, Lily Kay argues that Rockefeller Foundation officers compelled Pauling and other Caltech scientists to focus on biology research that would yield techniques for manipulating, and ostensibly “improving,” human life. I agree with Kay’s analysis of the motives of the Rockefeller Foundation officers, though I would add that Weaver built on trends that Caltech scientists had already inaugurated as much as he imposed policy initiatives on them. Harsher critics of Weaver including Pnina Abir-Am have argued that the Rockefeller Foundation limited the theoretical options available to grant recipients by “forcing” biologists to use physical science instruments and methodology. She further contends that the efforts of the Rockefeller Foundation in the biological sciences yielded few lasting accomplishments in the field of molecular biology. Abir-Am raises interesting issues, but her characterization of the scientists as passive tools of the foundations is overdrawn, as is her dismissal of the impact of Foundation intervention in the field of molecular biology. Rather, like Kay and others, I will focus on the opportunities that

624 Lily E. Kay, The Molecular Vision of Life.
Weaver offered for scientists who had already begun to move in directions that were supported by Rockefeller Foundation policies. 626

In this chapter, I will build on these accounts and enter the discussion by arguing that foundation intervention in Caltech science was, on the whole, highly productive. I will emphasize points that I feel these historians and sociologists, who have certainly made important and informative arguments, have not fully explored in their accounts. First, I will show that Carnegie philanthropic foundation officers were intimately involved in the early stages of Caltech’s development of the physical sciences and that their contribution helped to make the later expansion to biology possible. Second, I will demonstrate that the physical scientists who organized Caltech in its early years were critical players in the intellectual and financial organization of the biological sciences. By incorporating these two points along with other facets of the relationship between scientists and foundation officers, I will reveal how intersecting social, financial, political, and scientific considerations led to the breakthroughs that Caltech scientists made in molecular biology and “bio-organic chemistry.” The projects at Caltech not only fulfilled the hopes and expectations of foundation officers (especially Weaver) and scientists, but they also contributed to the creation of new models of biological science rooted in the investigation of the infinitesimally small. This chapter thus demonstrates that, when all the components fell into place, foundation intervention helped to facilitate the development of a world-class university, superior research programs, and innovative scientific approaches to the study of living beings.

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To understand how Caltech was able to become the model of "cooperation in research" in the 1930s, it is necessary to begin with the scientists who effectively designed the university: George E. Hale (1868–1938), Arthur A. Noyes (1866–1936), and Robert A. Millikan (1868–1953), known as the "triumvirate." John Servos has argued that the entire history of science and research at Caltech can be credited to the triumvirate, including the role that foundations played. 627 Caltech, according to Servos, was the direct result of the triumvirate's plans to build "a world-class research and educational institution" that would be "funded by national philanthropic foundations and local business enterprises" and would be dedicated to "launching a cooperative attack on the fundamental constitution of matter from the physical, astrophysical, and physico-chemical bases." 628 Although Servos may have overstated the case, there is little doubt that the remarkable growth and development of Caltech science in such a short time would have been impossible without the vision, organization, and recruitment efforts spearheaded by Hale, Noyes and Millikan.

Hale, Noyes and Millikan guided the creation of the National Research Council during World War I after spending the early years of the twentieth century leading national professional organizations and cultivating close relationships with philanthropic foundation officers. Not only did the triumvirate scientists acquire huge sums of money from foundations throughout their careers, which greatly aided their ability to construct science programs at Caltech, but they also played central roles in Caltech's administration. The scientist as administrator was a distinctive facet of Caltech, where an Executive Council comprised of trustees and professors made decisions about hiring.

628 John W. Servos, "The Knowledge Corporation," 175.
firing, salaries, and budgets. Therefore, scientists at Caltech had an extraordinary amount of influence over every aspect of the university’s development.

As explained in Chapter Two, George E. Hale had established himself as a leading astronomer and promoter of scientific research by the turn of the twentieth century. As an undergraduate astrophysics major at the Massachusetts Institute of Technology (MIT) in 1890, Hale had invented the spectroheliograph, a photographic device for analyzing solar elements. Hale’s father, a wealthy businessman, had financed the spectroheliograph and had served as an intermediary between his son and the corporate elites who funded scientific research. Upon accepting his first job at the University of Chicago in 1893, Hale had utilized his father’s business connections to solicit funding for a new observatory. He caught the attention of entrepreneur Charles T. Yerkes, who had contributed the necessary funds to enable the University of Chicago to construct the largest and finest telescope in the United States. The success of the telescope project, which was dubbed the “Yerkes Observatory,” had launched Hale into the forefront of scientific management and promotion. Hale had used his growing clout to garner support for The Astrophysical Journal, which he established in 1895.

In 1902, Hale joined two organizations that transformed him from a capable astrophysicist into a full-fledged promoter of science: the National Academy of Sciences (NAS) and the advisory board of the newly minted Carnegie Institution of Washington (CIW). As a thirty-three year old, Hale was the youngest member of the NAS, but his age did not inhibit his ambitions. He was “destined to effect” the “greatest changes” in

the professional organization "since its inception." Within a year of his election to the NAS, Hale had become a member of the Executive Council, after which he proceeded to take leadership positions in multiple committees. Membership in the NAS was both an honor—establishing Hale as a leader in his field—and an opportunity for him to connect with influential scientists from major universities throughout the United States. It was through the NAS that Hale met Arthur Amos Noyes, Robert Andrews Millikan, and others who would become critical to the development of Caltech. The NAS also provided a forum for Hale’s concept of cooperation in research, a concept that made a decisive impact on the organization of science during and after World War I.

Acting as an advisor to the Carnegie Institution of Washington (CIW) was equally important for Hale’s future as a scientific promoter and organizer. He befriended the trustees and advisors who distributed Carnegie funds, developing relationships that later became critical when he sought financial backing for his many scientific enterprises. In 1903, Hale had encouraged the CIW to build an astronomical observatory with a large reflecting telescope in the Southwest, which had led to the construction of the Mt. Wilson Astronomical Observatory in Pasadena, California. Hale had taken over as director of the Mt. Wilson Observatory immediately after it was completed. As director of the Observatory, he continued to garner admiration for his technological innovations, including a giant spectrograph that could measure the sun’s magnetic field. He also became a prominent figure in the Pasadena community.

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632 Ibid., 178.
In 1907, Hale was appointed as a trustee of Throop Polytechnic Institute, a small technical school that was located a few miles from Mt. Wilson Observatory. Throop Polytechnic had been founded in 1891 in Pasadena by Amos Throop, who had made his fortune in the railroad industry in southern California and wanted to encourage technical education in the region. By 1905, Throop’s initial investment and building donations totaled a mere $150,000, and the school was barely able to meet faculty payroll. To help the school stay afloat, successive Throop Polytechnic presidents Walter Edwards and J. B. Scherer had applied for financial assistance repeatedly, and unsuccessfully, to Wallace Buttrick, director of the General Education Board (GEB).\textsuperscript{634} Though Throop Polytechnic was basically a vocational and teacher preparatory school, Scherer had presented it as an ideal candidate for GEB funding: a non-sectarian institution dedicated to the promotion of engineering in an underserved region. After Hale joined the board of trustees, the leaders of Throop Polytechnic saw the addition of a well-connected and prominent scientist who already had philanthropic foundation support as an opportunity to present a stronger case to the GEB. In 1909, Scherer wrote GEB officers that the willingness of George E. Hale to join the board of trustees indicated that Throop Polytechnic was a worthwhile investment. Moreover, Scherer asserted, the GEB could “give a tremendous impetus to higher education in the West” by contributing toward the Institute’s endowment.\textsuperscript{635} Buttrick informed Scherer that the GEB would not fund a strictly

\textsuperscript{634} Walter Edwards to Wallace Buttrick, November 22, 1905; Edwards to Buttrick, January 2, 1907; Buttrick to Edwards, March 12, 1907; Edwards to Buttrick, April 2, 1907, folder 6466, box 611, subseries 4, series I, General Education Board Archives, Rockefeller Archive Center, Tarrytown, NY (henceforth RAC).

\textsuperscript{635} J.B. Scherer to Buttrick, January 4, 1909; Scherer to Buttrick, January 20, 1909; Scherer to Buttrick, April 4, 1909; Buttrick to Scherer, May 28, 1909; Letter from Scherer to Buttrick, July 10, 1909, folder 6466, box 611, subseries 4, series I, General Education Board Archives, RAC.
technical school. Throop Polytechnic executives continued to apply to the GEB for assistance throughout the 1910s, but they were always rejected.

Hale, who was soon joined by fellow scientists that he recruited from the NAS, came to Throop Polytechnic’s rescue. Throughout the 1910s, Hale worked on tightening his relationships with foundation officers and fellow scientific entrepreneurs. By leading the mobilization of scientists for World War I through the National Academy of Sciences (NAS)-National Research Council (NRC), Hale solidified his connections with figures from both foundations and major universities who would become critical to the transformation of the struggling Throop Polytechnic into the powerhouse California Institute of Technology (Caltech). The two most important figures were A. A. Noyes and Robert A. Millikan, who became his closest allies at the NAS, the NRC, and, later, at Caltech.

While Hale was a student at MIT in the early 1890s, Noyes was beginning his career there as a professor of chemistry. In fact, Hale had been one of Noyes’s students. At MIT, Noyes had established himself as one of the preeminent chemists in the U.S. and had essentially defined the field of physical chemistry.636 Like Hale, Noyes had begun his career as a scientific entrepreneur early and had built close ties to foundations. Noyes was one of the earliest grant recipients from the Carnegie Institution of Washington (CIW), which began supporting his research in 1903. In addition to building relationships with foundation officers, Noyes had established his leadership credentials through professional associations. He was elected president of the American Chemical

Society in 1904 and appointed to the NAS the following year. Noyes joined NAS committees that Hale had designed to organize public meetings of scientists for the dual purpose of advertising scientific advances and encouraging cooperative research. He and Hale wanted to promote scientific research to the public at large to a much greater extent and to make the results of scientific investigations more accessible to the layman. Noyes’s sensitivity about the need to explain scientific principles and developments in clear, comprehensible terms remained a defining characteristic of his professional career. He was celebrated as much for his accomplishments as a teacher and promoter of science, where he utilized his skills as an interpreter of esoteric ideas, as he was for his achievements as a researcher.

As he became increasingly involved in NAS activities, Noyes spent more time with Hale and learned about Throop Polytechnic. Hale was desperate to recruit Noyes to establish a chemistry department there. Eventually, in 1915, Hale convinced Noyes to spend several months each year teaching students and conducting research at Throop Polytechnic. Even though Noyes remained on the faculty of MIT, Hale heralded his friend’s arrival at Throop in letters to potential donors in May 1916 as a crucial step toward improving American university research. “I really believe this is the greatest chance we have ever had to advance research in America,” Hale wrote, “and I am going to give Throop the chance to lead the way.” As part of his effort to recruit Noyes for a permanent position as chair of a new chemistry department, Hale raised the money to

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build the Gates Chemical Laboratory in January 1917. Nonetheless, Noyes officially remained at MIT until after World War I.

While Noyes commuted between the east and west coasts, Hale began negotiating with Robert A. Millikan in the hopes of bringing him to Throop Polytechnic to initiate a physics program. Millikan had completed his Ph.D. in physics at Columbia University in 1895, after which he had been appointed as an assistant to renowned physicist A. A. Michelson at the University of Chicago. While at Chicago, Millikan had joined a group of professors who became leaders in the professional science organizations that acted as intermediaries with philanthropic foundation boards. Hale was a part of the group, as were future foundation notables and advisors Frank R. Lillie, James R. Angell, and Trevor Arnett (longtime trustee of the Rockefeller Institute for Medical Research, Rockefeller Foundation, General Education Board).  

Millikan had advanced rapidly from assistant to full professor at Chicago by conducting groundbreaking research in physics, particularly investigations into the impact of electrical conductivity on radioactive substances and on the ionic properties of gases. In 1907, with the support of mentor A. A. Michelson, Millikan had begun experiments to determine the charge of the electron, research that ultimately earned him the Nobel Prize in Physics in 1924.  

After his appointment to full professor of physics at Chicago in 1910, Millikan continued to accumulate prizes and accolades, and he was ubiquitous at major scientific gatherings for decades. Most importantly, Millikan was inducted into the National Academy of Sciences in 1915. Through the NAS, he reunited with Hale and formed a lasting friendship with Noyes. By late 1915, Hale fully expected

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that both Millikan and Noyes would end up at Throop Polytechnic, but World War I intervened.

As I described in Chapter Two, World War I provided the opportunity for Hale, Noyes, and Millikan to coordinate American scientific research on an unprecedented scale. Through their efforts to organize science for the war, the triumvirate was born. Hale had originally outlined a plan for the mobilization and coordination of scientists in *National Academies and the Progress of Research* (1915). As Hale had conceived it, the purpose of the NAS was two-fold: to promote, fund, and organize scientific research and to gain a responsive, influential audience with national leaders. Unfortunately, the NAS was performing poorly on both accounts. Hale believed that war mobilization could rejuvenate the NAS. He was right. Hale spearheaded the formation of the Committee on the Organization of the Scientific Resources of the Country for National Service, which included Millikan, Noyes, Edwin G. Conklin (Professor of Zoology at Princeton), and Simon Flexner (Director of the Rockefeller Institute for Medical Research and a Rockefeller Foundation trustee). This committee formed the basis for the National Academy of Sciences (NAS)-National Research Council (NRC), which was signed into law by President Wilson in July 1916. Under the auspices of the NAS-NRC, the Hale-Millikan-Noyes triumvirate established their leadership credentials as never before.

During the American involvement in the war, Hale, Noyes and Millikan contributed much to the mobilization of science and to the building of the NRC as an organization. Hale acted as a liaison between scientists and the government while aiding

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to efforts to "improve available military devices."\textsuperscript{645} Millikan, who was also an officer in the Navy, became a leading member of the Executive Committee of the NRC Division of Physical Sciences. In terms of research, Millikan focused on improving sonar technology in cooperation with the Signal Corps.\textsuperscript{646} Noyes dedicated his war years to a specific project: isolating nitrogen compounds for use in explosives.\textsuperscript{647} By the end of the war, all three men had proven the value of their research to foundation officers, industry representative, and military leaders.

From the outset, the triumvirate of Hale, Noyes and Millikan had envisioned a postwar role for the NRC that involved promoting scientific research by soliciting funds, coordinating interdisciplinary projects, and publicizing results.\textsuperscript{648} As NRC member Frank B. Jewett characterized the division of labor among the members of the triumvirate as they went about the business of ensuring the long-term success of professional American science: Hale was the "chief of staff" who "provided the imagination," Noyes was the "wise counselor," and Millikan was the "dynamic commander, leader of the field forces."\textsuperscript{649} Together, they made a formidable fundraising and organizing team. The triumvirate would maintain their roles after Hale convinced them to move their center of operations to Pasadena.

After the war, Hale renewed his effort to recruit Noyes and Millikan to Throop Polytechnic. Both Noyes and Millikan were spending part of the year teaching at


\textsuperscript{648} Kevles, "George Ellery Hale, the First World War, and the Advancement of Science," 431.

\textsuperscript{649} Cochrane, \textit{The National Academy of Sciences}, 215.
Throop, but they remained tied to MIT and Chicago, respectively. To attract Noyes and Millikan as permanent professors, Hale made promises that came to define the Caltech experiment. The scientists would have the chance to develop new laboratories and research programs, complete control over financial resources and administrative decisions, and the opportunity to participate in an institution that privileged the physical sciences above all other subject matter. Hale marshaled resources from his corporate and foundation connections to guarantee substantial funding for Noyes, who already had the new Gates Chemical Laboratory at his disposal. In 1920, Noyes accepted Hale’s offer and convinced the trustees of Throop to change the name of the school to the California Institute of Technology. Noyes added to the connections between Caltech and philanthropic foundations because he “brought income to the Gates Laboratory from research grants given him by the Carnegie Institution of Washington together with the promise of future foundation support for his researches.”

Once he was established in Gates Laboratory, Noyes promptly began developing his physical chemistry program by designing experiments to investigate atomic structure and radioactivity using x-ray crystallography. By the mid-1920s, Noyes had established the Gates laboratory as the national center for X-ray crystallography, which attracted graduate students from throughout the world. Noyes also joined Hale in an effort to recruit Millikan to serve both as the head of the physics department and as the president of Caltech.

According to Millikan, Hale was his “most ardent wooer” but Noyes was his principal draw to Caltech. Millikan wanted to become part of a university that focused on physics as “the basis of all engineering and of all biology” and the arrival of Noyes

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651 Ibid., 177.
proved that Caltech was moving in that direction. The completion of the Norman Bridge Physics Laboratory in 1920 also surely contributed to his attraction to Caltech. Still, Millikan was reluctant to accept the position of university president because he wanted to make physics research his top priority and he was concerned that administrative obligations would interfere with his work. To reach a compromise, Millikan consulted “old friends”: Frederick Keppel, trustee of the Carnegie Corporation of New York (soon-to-be president), and George Vincent, president of the Rockefeller Foundation. With their advice in mind, Millikan corresponded with Hale and the head of Caltech’s Board of Trustees, Arthur Fleming. The result of these negotiations was an innovative experiment in university governance: the Executive Council. Instead of a university president, all budget, appointment, promotion, and salary decisions would be made by a council comprised of four trustees and four faculty members. The Executive Council created the ideal situation from the standpoint of Hale, Noyes, and Millikan because it would enable the scientists to control every aspect of the university. With sufficient funding, the triumvirate could achieve their scientific and administrative goals. Their chief concern was, as Hale asserted, that the “spirit of cooperation that played so large a part during the war” be “applied to the lasting advantage of science and research.”

As the Chairman of the Executive Council, Millikan joined Hale as an active fundraiser for Caltech. Both men, along with their colleagues on the Executive Council, believed that Caltech should be supported entirely by private sources of funding. While Hale emphasized a desire to avoid government interference in scientific work, Millikan

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was more concerned about the dampening effect that relying on taxpayer money would have on the quality of Caltech students. Millikan incorporated his point about the importance of private contributions into an oft-repeated argument that he made on behalf of Caltech. When addressing corporate leaders and foundation officers, Millikan contended that the U.S. lagged behind Europe in scientific research because educational institutions had failed to seek and encourage the most capable students, a problem that Caltech was perfectly situated to remedy. "The progress of civilization is determined by the very few men of vision and capacity which each age develops," he stated, but public universities were unable to be selective. "[The state university's] obligations to the taxpayer force it, and properly force it, to do the best it can with all the material which comes to it. . . . It cannot raise its standards to admit only the most promising."\(^{654}\)

Millikan explained that, as a school in an optimal location that was "untrammeled by the inertia of tradition," Caltech was an ideal university environment for educating the best and brightest in the fundamental sciences. Further, with its flexible departmental and administrative structure, Caltech was equipped "to teach the whole country a lesson in the big problem of logical and effective interdepartmental cooperation and organization."\(^{655}\)

Hale and Millikan reiterated the theme of cooperation in research with an emphasis on the physical sciences, which they referred to as the "fundamental sciences," when they launched a campaign to secure philanthropic foundation funding in 1921. Their approach was quite effective. In May, the General Education Board (GEB) appropriated $300,000 toward Caltech's endowment and $15,000 a year for two years to

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\(^{654}\) Assembly address, January 6, 1920, folder 9, box 20, Series IV, Millikan Papers, California Institute of Technology, Pasadena, CA (henceforth Caltech). Emphasis his.

\(^{655}\) Ibid.
supplement professors' salaries. In the congratulatory letter, GEB secretary Trevor Arnett referred to a recent visit that he and Wallace Buttrick had made to Caltech. Apparently, the arrival of Millikan and Noyes coupled with the refurbished facilities had convinced them that Caltech had "outstanding potential." Encouraged by the GEB contributions, Hale wrote to Carnegie Corporation of New York president James R. Angell, a close friend and fellow former member of the NRC Executive Council, to request "partial support of a comprehensive research on the constitution of matter, the nature of radiation, and the genesis of the elements." The interdisciplinary project would be conducted by Millikan, Noyes, mathematical physicist H. A. Lorentz, physicist Paul Epstein (a Millikan recruit), and chemist R. T. Tolman (a Noyes recruit and former student). The project proposal outlined a program that encapsulated the intellectual direction that Caltech would take for the next decade:

Matter occurs in nature under the widest variety of composition and form. The physicist, who approaches the complex problem by the simplest and most direct route . . . evolves powerful methods of research which enable him to penetrate to the core of the atom . . . The chemist, concerned primarily with the union of atoms into molecules . . . necessarily attacks matter of greater complexity. . . . In each of the branches of science involved, the methods and instruments of research have advanced to a very high degree of perfection. Discovery has followed discovery,

656 Trevor Arnett to Arthur Fleming, May 27, 1921, folder 6466, box 611, subseries IV, series I, General Education Board Archives, RAC.
657 Hale to Angell, June 4, 1921, folder 1, box 69, Carnegie Corporation of New York (CCNY) Grant Files, Carnegie Corporation of New York (CCNY) Archives, Columbia University, New York, NY (henceforth "Columbia").
now in one subject, now in another, each throwing new and increasing illumination into the other fields.\textsuperscript{658}

The proposal sheds light on the assumptions about scientific research that the Caltech scientists made. First, their understanding of the relative complexity of the sciences was based on a combination of evolutionary theory and a hierarchy of the sciences that emphasized the fundamental importance of the physical sciences. The Rockefeller Foundation officers adopted this understanding of the hierarchy of scientific disciplines, as they explained:

Mathematics as a method and tool comes first. Then follows physics which rests upon mathematics. After physics, and dependent upon it, chemistry appears. Until physics and chemistry have reached a certain stage of development the general science of life, biology, is hampered in its growth.\textsuperscript{659}

It seems highly likely that Foundation officers embraced this understanding of the hierarchy of the sciences after corresponding regularly with Millikan and Noyes.

Caltech scientists built on this understanding of the hierarchy of the sciences to add the other key dimension of their research philosophy: progress and discovery depended upon having the best instrumentation. Caltech researchers asserted that they could excel in all branches of science if they had the optimal tools: spectrosopes, x-ray crystallography machines, ultracentrifuges, instruments to measure the diffraction of electrons in gases. Again, the scientists informed the foundations about what they needed to succeed, and foundation officers responded by adjusting their programs based on the latest information.

\textsuperscript{658} "A Joint Attack on the Constitution of Matter," 1921, folder 15, box 20, series IV, Robert Andrews Millikan Papers, California Institute of Technology, Pasadena, CA (henceforth "Caltech").

\textsuperscript{659} Rockefeller Foundation, \textit{1925 Annual Report} (New York: Rockefeller Foundation, 1926), 50.
In 1921, Hale wrote a nine page letter to Henry S. Pritchett, the chairman of the Carnegie Corporation of New York (CCNY) board of trustees, in which he communicated his reasons why foundations should help Caltech become a top research institution. “We have initiated a clearly-defined and organically sound policy, in which our plans for instruction and research in physics and chemistry play a fundamental part,” Hale asserted. Yet, he continued, “to accomplish what we seek we must have the very best men in this country and abroad, and we must give them the best of equipment and adequate financial support.” Hale, adept scientific entrepreneur that he was, utilized the “best man” approach, thereby appealing directly to the Carnegie mandate to “find and equip the exceptional man.” Unsurprisingly, the tactic worked. On November 22, 1921, Pritchett wrote both Hale and Millikan to inform them that the Carnegie Corporation had allotted $30,000 a year for five years, to be administered through the Carnegie Institution of Washington, “for the promotion of the work of research in physics and chemistry at the California Institute of Technology.”

To Millikan, the influx of foundation money proved that he had made the right decision by moving to Caltech. He wrote Pritchett on December 8, 1921 to express his excitement about the Carnegie grant allotment:

My decision to break the very strong ties which held me in Chicago . . . was finally reached largely because of the interest which yourself, Mr. [Elihu] Root, and Mr. [John C.] Merriam had expressed in the development of the project here, and because of the probability that the Corporation would wish to have a hand in this development. It is now highly gratifying to find that this probability has been

660 Hale to Pritchett, October 28, 1921, box 69, folder 1, CCNY Grant Files, CCNY Archives, Columbia.
661 Pritchett to Hale and Millikan, November 22, 1921, folder 9, box 20, Series IV, Millikan Papers, Caltech.
transferred into reality, and you may be sure that the confidence which you have reposed in me will be a great stimulus to me to do my utmost to make the investment a good one.  

As Millikan conveyed in his letter, he saw the Carnegie grant as a vindication of the plan to inaugurate a cooperative scientific research effort based on the physical sciences at Caltech. With Rockefeller and Carnegie money to supplement an endowment provided by head trustee Arthur Fleming and with the scientists firmly in charge, the Caltech experiment was underway.

Over the course of the 1920s, Millikan and Noyes led Caltech to international prominence by running top research programs while continuing to master the art of fundraising (Hale remained a trustee, but his research was tangential because he was at the Mt. Wilson Observatory). The scientists fulfilled their promises to the foundations by encouraging interdisciplinary cooperation, especially between chemistry and physics, and by utilizing tailor-made instrumentation, regardless of the expense, to reveal insights about the atomic structure of numerous inorganic substances. Further, the Caltech scientists continued their frequent correspondence and interaction with officers from both Carnegie and Rockefeller philanthropies. The foundations began to echo the Caltech theme of “cooperation in research” more forcefully as they witnessed the apparent success of the interdisciplinary programs.  

At the same time, Caltech scientists relied more and more on their close personal relationships with foundation leaders. For example, Noyes had been thrilled to learn that John C. Merriam was appointed director of

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662 Millikan to Pritchett, December 8, 1921, folder 9, box 20, Series IV, Millikan Papers, Caltech. Elihu Root was a trustee of the CCNY and chairman of the CIW board of trustees; John C. Merriam was the president of the CIW and a member of the CCNY board of trustees.

the Carnegie Institution of Washington (CIW) in 1921 because they had worked together as dear friends at the NRC. He wrote Hale that, with Merriam in charge, "you, he, and I would together largely determine the policies of the Carnegie Institution." Noyes's enthusiasm about Merriam was rewarded: he received more than $200,000 from the Carnegie Corporation of New York, administered by the CIW, over the next decade. At the same time, the CIW supported Millikan with $10,000 a year for ten years beginning in 1922 and continued to pay Hale's salary at Mt. Wilson Observatory.

Noyes and Millikan made sure that the Carnegie officials understood that they were advancing the progress of research in the United States by supporting Caltech science. In a 1924 letter to Frederick Keppel, president of the Carnegie Corporation, Noyes congratulated the foundation for helping to determine "the future of biochemical research." Millikan was even more effusive. In November 1924, he thanked Keppel for the outstanding caliber of Caltech research:

The research work could not have been kept going at all in its present rigorous form had it not been for the $30,000 yearly which Noyes and Millikan have together received from the Carnegie Corporation. . . . The cause of the scientific development of the United States owes the Corporation a debt of gratitude for what it has done.

Their enthusiasm both encouraged the Carnegie foundations to maintain their connection with Caltech and caught the attention of the Rockefeller boards.

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665 John C. Merriam to Millikan, January 7, 1922, folder 15, box 20, Series IV, Millikan Papers, Caltech.
666 Noyes to Keppel, October 16, 1924, folder 1, box 69, CCNY Grant files, CCNY Archives, Columbia.
667 Millikan to Keppel, November 29, 1924, folder 1, box 69, CCNY Grant files, CCNY Archives, Columbia.
As mentioned in earlier chapters, the General Education Board (GEB) shifted its focus from college endowments to the support of scientific research in 1923, when Wickliffe Rose took over as president. As he explained in an internal office memo, Rose saw scientific research as "the key to such dominion as men may ever exercise over his physical environment."\(^{668}\) Like his colleagues at the Carnegie Corporation, Rose turned to prestigious scientists for information about how to fulfill the Rockefeller charter of contributing to the welfare of mankind. Rose believed that "on the whole, the progress of civilization coincides with the increase in accurate knowledge and the spread of the objective and dispassionate spirit of scientific inquiry."\(^{669}\) Upon assuming his post as director of the GEB, he announced that "the General Education Board is now definitely undertaking to cooperate in improving the situation of the physical and biological sciences."\(^{670}\) Millikan and Noyes took advantage of Rose’s predilections and initiated a correspondence with the GEB about the direction of Caltech science. They learned that Rose and his fellow officers were particularly interested in taking the next step in the scientific hierarchy by adding organic chemistry and biological sciences to the research agenda. The scientists set to work, creating a program that continued to privilege the physical sciences but broadened the scope to incorporate the "border disciplines" of organic chemistry, biochemistry, and biophysics. Their conceptualization of the new program fit neatly with their promotion of cooperation in research and their dedication to physics as the basis of all sciences. Once they gained assurances from the GEB about financial support for the new program, Noyes and Millikan used the promise of

\(^{669}\) Ibid., 152.
\(^{670}\) Ibid.
Rockefeller funding to motivate Carnegie officers to contribute even more to Caltech science. They continued to go back and forth between the foundations in an effort to encourage the officers from the different organizations to cooperate and/or compete by offering grants to Caltech.

The only problem was that the Carnegie officers were not nearly as enthusiastic about the prospects for a biology program at Caltech as the GEB officers. As late as December 1924, Carnegie Institution of Washington (CIW) president John C. Merriam and Carnegie Corporation of New York (CCNY) president Frederick Keppel expressed reluctance about supporting a biology research program at Caltech. They did, however, see organic chemistry as a potentially promising avenue that could lead eventually to the biological sciences. As Merriam wrote Keppel,

I think I have convinced Millikan that, as you suggest, the biological side should be held to a minimum for the present at least. It is my view that the California Institute can do more for biology by intensive research on fundamental physics and chemistry . . . California Institute should carry its physics and chemistry up the boundary of biology. Organic chemistry should be carefully developed and the elements of biology should be handled by a man who could make contacts of the best type. 671

These exact conditions were fulfilled by the research programs developed by Thomas Hunt Morgan and Linus Pauling in the 1930s, but much transpired before their arrival. In the mid-1920s, Noyes and Millikan were still trying to outline a program that would suit

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671 Merriam to Keppel, December 6, 1924, folder 1, box 69, CCNY Grant files, CCNY Archives, Columbia.
their current facilities and researchers, as well as the expectations of both Rockefeller and Carnegie foundation officers.

Noyes and Millikan understood that the Carnegie officers believed organic chemistry was worthy of support, and they were certain that they could convince the officers to embrace the "borderline" disciplines of biophysics and biochemistry. Again, they used the promise of General Education Board (GEB) support to convince the Carnegie officers that the fields related to biology were worthwhile. Noyes wrote Henry S. Pritchett on January 30, 1925,

My only hope . . . is that we may establish here a department of biochemistry and biophysics, such as we have described in a recent application to the General Education Board . . . We have good reason to think, from our conversations with Dr. Rose, Dr. Buttrick, and Mr. Toquelson [sic], that the Board will assist our development along existing lines and in organic chemistry, biochemistry and biophysics by a substantial annual grant.\(^{672}\)

This letter and other correspondence reveal the techniques that Caltech scientists used to sustain foundation enthusiasm for their work. Without challenging their assumptions about the dependence of the biological sciences on physics and chemistry, Noyes and Millikan gently nudged the Carnegie officers in the direction that the General Education Board (GEB) officers wanted to pursue. In March 1925, the GEB announced an appropriation of $450,000 for Caltech as an endowment to support current scientific research projects and to assist the inauguration of a program in organic chemistry,

\(^{672}\) Noyes to Pritchett, January 30, 1925, folder 1, box 69, CCNY Grant files, CCNY Archives, Columbia. Noyes was referring to H.J. Thorkelson, a GEB officer and trustee who had visited Caltech on a number of occasions.
biophysics, and biochemistry.\textsuperscript{673} When Keppel of the Carnegie Corporation heard about the finalization of the GEB grant, he prompted the officers of his foundation to add a contribution of $100,000.\textsuperscript{674} The volleying between Carnegie and Rockefeller foundations had been remarkably effective.

For the most part, Millikan and Noyes used the additional funds to maintain the research programs that they had already established. Millikan continued to investigate the structure, location, and function of electrons within inorganic molecules and to conduct radiation studies in a quest to determine the mass of the electron. Noyes persisted in his attempt to find “the effect of radiation and electric discharges on the rate and equilibrium of chemical reactions” and the structure of inorganic substances through x-ray crystallography, as well as studies of ions and rare elements.\textsuperscript{675} They did expand the chemistry and physics departments and began the process of recruiting researchers in the biological sciences. Nonetheless, the notable achievements of Caltech scientists remained in the fields of physics and chemistry for several years after the GEB and Carnegie grants of 1925. To succeed in the effort to shift resources and energy toward the biological sciences, Millikan needed to recruit top scientists.

Hale had tried to recruit famous geneticist Thomas Hunt Morgan from Columbia to Caltech in 1925, but Morgan refused due to the lack of a well-equipped biological laboratory and endowment fund. Hale, Noyes, and Millikan knew Morgan well through the NRC and NAS, and they hoped that he would be the one to launch biology at Caltech. The triumvirate felt that Morgan would fit in perfectly with their cooperative research

\textsuperscript{673} GEB to Millikan, March 10, 1925, folder 6476, box 612, subseries IV, Series I, General Education Board Archives, RAC.
\textsuperscript{674} Keppel to Millikan, April 7, 1925, folder 1, box 69, CCNY Grant files, CCNY Archives, Columbia.
\textsuperscript{675} Report, “Investigations concerning the structure of matter,” December 1, 1925, folder 2, box 69, CCNY Grant files, CCNY Archives, Columbia.
agenda because he had consistently argued that the biological sciences should be based on an understanding of the "fundamental processes" elucidated by physics and chemistry. Still, Morgan was very content at Columbia and it would take an impressive incentive package to draw him away from the "fly lab" where he had completed his groundbreaking genetics research.

Largely due to his desire to recruit T. H. Morgan and to build a biology department that was comparable to the physics and chemistry departments at Caltech, Millikan launched a major fundraising campaign in 1926. Since he was already receiving substantial support from the foundations, Millikan turned to potential donors from the Pasadena area. In his letters to prominent Pasadena residents and business owners, Millikan repeatedly stressed that philanthropic foundations had shown tremendous interest in Caltech programs. "Both the Rockefeller Foundation and the Carnegie Institution have shown their confidence in this undertaking by contributing to its initiation and support," he explained. Like he had the previous year when soliciting funds from the Carnegie and Rockefeller officers, Millikan used support from one organization to justify asking for support from another. After he made the case to local elites that Caltech was a worthy investment opportunity, Millikan then used instances of local support to bolster his applications to the philanthropic foundations for supplementary funding. The strategy was highly effective: money poured in and the donors reinforced each other.

Throughout 1926, Millikan maintained the practice of using grant allotments from Rockefeller boards to solicit funds from Carnegie foundations, and vice versa. He and

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677 Memo from Millikan to potential donors, drafted February 10, 1926, folder 8, box 19, series IV, Millikan Papers, Caltech.
his fellow triumvirate members were so entrenched in the foundation interlocking
directorate by this point that there was a continuous correspondence and exchange among
officers from the Carnegie and Rockefeller boards and representatives from the Caltech
Executive Council. For example, Carnegie Corporation of New York (CCNY) president
Frederick Keppel met with officers from the General Education Board (GEB) in October
1926, and then immediately wrote Millikan to inform him that prospects for future
Caltech funding from Rockefeller boards looked promising. "Being less discreet than
they are," Keppel wrote, referring to the GEB officers, "I don't mind writing to say that I
shouldn't worry if I were you." 678 In part, Keppel was seeking reassurance that the GEB
would offer Caltech ample support because he was considering reducing the CCNY
allotment for "fundamental researches in physics and chemistry" because the Carnegie
philanthropies were experiencing some financial problems. 679 Yet, if Keppel believed
that his efforts to placate Millikan by telling him that GEB funding was virtually
guaranteed would discourage further Caltech applications, he was mistaken. In
November 1926, only a few weeks after Keppel communicated to Millikan that the
CCNY was tightening its policies and limiting new appropriations, Hale applied for a
$600,000 grant on behalf of the Caltech Executive Council. 680 In a letter that he sent to
the top five officials from the various Carnegie philanthropic boards, Hale noted that
Caltech needed additional funds to fulfill a conditional grant stipulation from the GEB.
Using the tried and true argument that Caltech was the best possible investment that
philanthropic foundations could make in the future of American science, Hale wrote, "I

678 Keppel to Millikan, October 27, 1926, folder 2, box 69, CCNY Grant files, CCNY Archives, Columbia.
679 Meeting notes between Thorkelson and Keppel, October 26, 1926, folder 6467, box 611, subseries 4,
series I, General Education Board Archives, RAC.
680 Hale to Keppel, Elihu Root, Henry S. Pritchett, John C. Merriam, J.J. Carty, November 18, 1926, folder
2, box 69, CCNY Grant files, CCNY Archives, Columbia.
doubt whether any other institution with the same means in the same time has made such progress in advanced work as has been accomplished here since Millikan joined us five years ago." "The sole difficulty," he explained, "has been financial."\textsuperscript{681} If the CCNY could muster a $600,000 contribution, Caltech would "bring in a gift of $450,000 from the General Education Board." Moreover, a sizeable Carnegie investment would "insure [sic] another large gift from them [the GEB] immediately."\textsuperscript{682}

Keppel found Hale's letter unsettling. He had been under the impression that the GEB was going to take on more of the financial responsibility for Caltech, without strings attached. The day after he received the letter from Hale, Keppel returned to the GEB offices to negotiate further. A few days after that, he wrote Hale that he went "to see the Rockefeller people," who had claimed that they were receptive to supplying Caltech with the $450,000 if the CCNY simply renewed its current grants, which totaled $30,000 a year.\textsuperscript{683} Disappointed, Hale turned to Millikan for ideas.

In a series of meetings in January and February 1927, Hale and Millikan determined that the best way to guarantee that both Carnegie and Rockefeller philanthropic boards would contribute as much as possible to Caltech science was to divide the disciplines into two categories. According to their plan, the Carnegie Corporation of New York (CCNY) would be responsible for the maintenance and endowment of physics and chemistry, while Caltech would turn to the Rockefeller philanthropies to help launch a program in the biological sciences. On March 12, 1927, Millikan gathered the members of the Executive Council for a meeting to discuss how to impress Wickliffe Rose and H.J. Thorkelson of the GEB, who were coming for a campus

\textsuperscript{681} Ibid.
\textsuperscript{682} Ibid.
\textsuperscript{683} Keppel to Hale, November 23, 1926, folder 2, box 69, CCNY Grant files, CCNY Archives, Columbia.
visit in two weeks. The strategy that Millikan outlined essentially became the interdisciplinary Caltech science program that has been celebrated ever since.

Millikan pointed out that Caltech had an excellent physical sciences program, but that the biological sciences were virtually nonexistent. One problem was Noyes’s inability to find someone to run an organic chemistry lab. Without a functioning organic chemistry lab, the Caltech scientists were having trouble convincing foundation executives that they could build biological sciences on the base of physics and chemistry. Still, it was imperative that Caltech focus on biology, Millikan argued, both for the sake of science and because officers from both the GEB and Rockefeller Foundation seemed to believe that “the big advances of the next generation are probably going to be made in the biological sciences.”

Princeton University and the University of Chicago had just secured $1 million in endowment funds from the GEB for their biology programs. For Caltech to compete with other top universities, the Executive Council needed to offer more opportunities for scientists to expand and diversify their programs. “It is well nigh certain that we can get a million dollars from them for the purposes of endowment under similar conditions” to Princeton and Chicago, Millikan added, “How can we do it?”

At the close of his remarks, Millikan suggested that, when Rose and Thorkelson arrived on March 24th, Caltech should have met the conditions of all prior GEB grants and should be prepared to propose the “intensification of our present departments and the beginning of the addition of fundamental biology.” The Executive Council members agreed wholeheartedly and Millikan’s plan went into effect immediately. Millikan had a

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684 Memo by Millikan to Caltech Trustees, March 12, 1927, folder 22, box 32, series IV, Millikan Papers, Caltech.
685 Ibid.
686 Ibid.
well-deserved reputation as a capable leader who maintained amiable relationships with
his fellow scientists in every department at Caltech. Once his plan was officially
approved, he visited each of the professors to inform them that Caltech was in the process
of coordinating scientific activities in the hopes of a financial windfall. The professors
responded enthusiastically by putting together ambitious prospectuses for their future
research and organizing demonstrations in their laboratories in anticipation of the GEB
officers' visit. The scientists' efforts worked well: Thorkelson and Rose returned home
from their March 24th visit in awe. When Thorkelson drafted a memo to describe the
visit to his fellow GEB officers and trustees, he basically repeated the arguments that
Millikan had presented to them:

The Institute cannot develop researches in the fundamental sciences without
including biology for the fields of both bio-physics and bio-chemistry are now
particularly open to fruitful research, and these fields hold the greatest promise for
productive work when associated with strong departments in the physical
sciences, physics, chemistry and mathematics.687

The GEB was therefore willing to contribute to the inauguration of biological sciences at
Caltech based on the plans that Millikan had outlined during March 1927. The money
was available. Millikan and Noyes took charge of the next steps: designing a laboratory
and recruiting a top professor to lead the new program.

As noted earlier, to attract their ideal candidate in biology, Thomas Hunt Morgan,
the Caltech leaders knew that they would have to provide outstanding laboratory facilities
and a sizeable endowment. After some negotiations with Millikan, the GEB officers

687 Memo re: Caltech visit by Thorkelson, March 24, 1927, folder 6476, box 612, subseries 4, series I,
General Education Board Archives, RAC.
agreed that Caltech would need $1.5 million to help build and equip a biological laboratory, as well as an annual budget of $100,000 to recruit Morgan’s research team.\footnote{Ibid.} In keeping with foundation policy, the GEB expected Caltech to find local donors to foot the bill for some of the facilities. Within a few months, Millikan found a local philanthropist, William G. Kerckhoff, to supply funds for the laboratory. Soon after, the construction of the Kerckhoff Biological Laboratory began.\footnote{Lily Kay, The Molecular Vision of Life, 80.} Meanwhile, the Caltech Executive Council received terrific news: the Carnegie Corporation of New York had approved a new grant of $200,000 toward the endowment of “fundamental researches in physics and chemistry” in addition to their annual $30,000 grant.\footnote{Keppel to Hale, April 6, 1927, folder 2, box 69, CCNY Grant files, CCNY Archives, Columbia} Coupled with a new GEB endowment grant of $1,050,000 “for enlarging the resources and facilities for graduate instruction and research in the physical sciences and initiating similar activities in the biological field,” the Caltech science programs were flush with the fruits of philanthropic beneficence.\footnote{Brierley (GEB secretary) to Millikan, June 9, 1927, folder 6476, box 612, subseries 4, series I, General Education Board Archives, RAC.} The Carnegie Corporation continued to support the physical sciences with two more $200,000 endowment contributions in 1928, enabling the Caltech leaders to focus their full attention on building the program in biological sciences.\footnote{Resolutions B-441 for $200,000 (May 19, 1927), B-485 for $200,000 (May 5, 1928), B-540 for $200,000 (October 26, 1928), folder 2, box 69, CCNY Grant files, CCNY Archives, Columbia.} By setting up the Kerckhoff Lab and finding the resources to attract a top biologist, Millikan, Hale, and Noyes had done everything necessary to set the stage for their goal of cooperation in research between the physical and biological sciences. They had presented compelling rationales for their research programs to philanthropic foundation officers and had mobilized the resources necessary to bring their plans to
fruition. The only thing left was to convince Thomas Hunt Morgan that he should move to Pasadena.

A close examination of Thomas Hunt Morgan’s professional life not only provides insights into the fascinating background of a Nobel Prize-winning geneticist, but it also reveals his role in transforming the field of biology in the early twentieth century. Morgan was no ordinary scientist. Over the course of his storied career, he influenced the organization, methodology, and politics of the study of biology. Morgan had worked with the top scientists in his field at each stage of his scientific training. He had earned his Ph.D. in zoology from the Johns Hopkins University in 1891, where he had wrestled with evolutionary theory under the guidance of William Keith Brooks (1848–1908). At that time, Johns Hopkins was the most prominent American university center for experimental biology, largely due to Brooks’s excellent reputation. Brooks was a proponent of Wilhelm Roux’s theory of *Entwicklungsmechanik* (developmental mechanics). Roux saw the body as a machine that could be dissected through physico-chemical analysis of the functioning parts. To Roux, physical and chemical processes were directly responsible for the mechanisms of embryonic development and cell differentiation. As Donna Haraway argues, Roux’s program involved a “full causal, physical, and mathematical explanation for embryology” that was based on “both his confidence in the machine paradigm and his sense of the inferiority of the previous approaches of biology.”

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formulations of a physico-chemical methodology for biology and the definition of the organism as a machine that can be understood through the laws of mechanics, hardly went uncontested. Yet, for the purposes of understanding the development of the biological sciences in the American universities, and particularly the theoretical influences in T. H. Morgan's career, Roux's developmental mechanics was critical.

The scientists who adopted Roux's theory of developmental mechanics were inspired to conduct myriad tests on embryos, usually culled from marine invertebrates, to determine the physical and chemical causes of cell differentiation. They bathed embryos in saline or mineral solutions, spun them in centrifuges, pressed them between glass plates, and spliced them into two parts. Many of these experiments took place at the Marine Biological Laboratory at Woods Hole. While he was working with Brooks in the 1880s, Morgan collected specimens at Woods Hole and brought them back to the laboratory at Johns Hopkins. Morgan prepared fertilized eggs for microscopic analysis by encasing them in paraffin and staining them to reveal the structures within the cells. The eggs would be in various stages of cell division, so that "it was much like looking at a randomly selected set of frames from a motion picture." He studied the earliest stages of embryonic development because he believed, following Brooks, that they represented the earliest stages of evolutionary development. In an analogous fashion to the assumptions of psychologists who believed that the study of "apes, idiots, savages, and children" represented lower levels on the evolutionary ladder, late nineteenth century embryologists and physiologists who subscribed to developmental mechanics assumed

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696 Ibid., 26.
698 Garland Allen, Thomas Hunt Morgan, 46–47.
that early stages of embryonic development corresponded to lower (less evolved) forms of life.

In 1891, Morgan had become a professor of biology at Bryn Mawr, where he worked in a two-man department with Jacques Loeb. Loeb, who was later immortalized as the fictional scientist Max Gottlieb in Sinclair Lewis’s *Arrowsmith*, was the most outspoken proponent of developmental mechanics in the United States. He became famous over the next few decades for his theory of artificial parthenogenesis, which posited that fertilization could be induced without the presence of germ cells through the proper introduction of physical and chemical agents, as well as for his groundbreaking treatise on the body as machine, *The Mechanistic Conception of Life* (1912). Working side-by-side with Loeb had surely influenced Morgan’s decision to remain wedded to developmental mechanics. Morgan had utilized the mechanistic approach throughout his tenure at Bryn Mawr, where he conducted studies of regeneration, evolution and heredity. The results of these researches were published in his first monograph, *Regeneration* (1901).

In 1904, Morgan had moved from Bryn Mawr to Columbia University. Inspired by the challenges presented by the reintroduction of Mendel’s laws, he had begun to focus more on questions of evolutionary theory, including sex determination and natural selection. As a devoted experimentalist, he found elements of Darwinian and Mendelian theories frustrating because they were difficult to test in a laboratory. The desire to test Mendelian heredity experimentally led Morgan to begin studies of the *Drosophila* fruit fly in his Columbia lab in 1908. The result was the initiation of what became revered

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throughout the world of science as the Columbia “fly lab.” In 1910, Morgan made a breakthrough after discovering a white-eyed male offspring from red-eyed parents. After repeated cross-breeding, he determined that the white-eyed flies not only confirmed the Mendelian law of recessive traits but also revealed that certain traits were inherited directly through the sex chromosomes. Morgan’s “chromosome theory of inheritance” launched a scientific revolution in genetics.

The chromosome theory of inheritance included two main theoretical claims. First, Mendelian genes could be mapped on chromosomes. Second, mutations, “lethal genes,” and other modifying factors complicated the Mendelian picture of heredity. Heredity was not simply about dominant and recessive traits. Chromosomes became crossed, linked, and otherwise transformed during the process of meiosis, or embryonic cell division. The chromosome theory raised the stakes for geneticists. From the announcement of the theory in 1911 onward, genetic researchers had a new project to engage their research interests: the mapping of genes on chromosomes. From 1911 to 1915, Morgan refined his chromosome theory of inheritance with the help of Columbia graduate student H. J. Muller, and undergraduates A.H. Sturtevant and Calvin Bridges. The research group published their results in *The Mechanism of Mendelian Heredity* (1915), which became the standard textbook for genetics courses throughout the U.S. for over a decade.

When the “fly lab” members published their findings, they had furthered the schism that had been developing between experimental biologists and biometricians, which was also becoming a battle between scientists who wanted to separate the study of

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701 Ibid.
genetics from the eugenics movement and those who remained loyal to eugenics (see previous chapter). The eugenics movement was at the height of its popularity around 1915, even as the scientific bases for eugenic claims were coming into serious question. Foundation officers were still very devoted to eugenics, and the Eugenics Record Office and affiliated Carnegie Institution of Washington laboratories at Cold Spring Harbor were lauded as centers of innovation by the wealthy elites who sponsored them. Yet, the relationship between members of the professional scientific community and the eugenics movement was becoming strained. In the 1920s, many genetics researchers continued to promote what they called “pure” eugenics, the improvement of the human race through knowledge of the genetic sources of problems and diseases, while they distanced themselves from the overtly political or social motives attached to “applied” eugenics, the reduction of the “unfit” through sterilization, immigration restriction, marriage laws, etc. 702

As it became clear to him in the mid-1920s that eugenicists were undermining the research claims of geneticists by manipulating data to suit their political agenda, Morgan had begun to speak out against the movement. While Morgan remained sympathetic to some eugenicist ideals, like the need to use rational planning to improve the human race, he expressed concern that the movement misrepresented genetic research by applying animal or plant breeding practices to humans. Morgan ultimately asserted that eugenicists were promoting an unscientific agenda:

Geneticists can now produce, by suitable breeding, strains of populations of animals and plants that are free from certain hereditary defects . . . In man it is not

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desirable, in practice, to attempt to do this, except in so far as here and there a
hereditary defective may be discouraged from breeding. . . . The claims of a few
enthusiasts that the human race can be entirely purified or renovated at this later
date, by proper breeding, have I think been greatly exaggerated. 703

In general, by the time he moved to Caltech, Morgan had dissociated himself from any
scientists who supported the eugenics movement, including former close associates like
Raymond Pearl.

Although Morgan had expressed his reservations about eugenics to members of
the scientific community, he minimized his concerns when communicating with
foundation officers, especially representatives from the Carnegie boards. By the time he
renounced the movement, Morgan had already received Carnegie grants by utilizing the
language of eugenics. He was aware that the Carnegie Institution of Washington (CIW),
which was heavily influenced by Eugenics Record Office director Charles B. Davenport,
would be more sympathetic to his grant proposals if he couched his research rationales in
eugenics terminology. When he had first applied for a grant in 1914, Morgan had written
Robert S. Woodward (the director of the CIW), “we have an opportunity to attack one of
the most important problems of biology, viz., the constitution (in the sense of the material
configuration) of the hereditary germ plasm in relation to heredity.” 704 In his reference to
“the hereditary germ plasm,” Morgan was using to the eugenicist description of
inheritance rather than offering his own more nuanced chromosomal theory. The “germ
plasm” was an undifferentiated mass of material within sperm and egg cells. Eugenicists

believed that traits were mixed into the germ plasm, which would yield offspring that had "blended" combinations of "factors" contributed by the parents. Morgan and his fly lab researchers had already proven otherwise. "Germ plasm" was inadequate. Hereditary characteristics—Mendel’s "factors"—were actually located on chromosomes that could be observed, counted, manipulated, and analyzed. Nonetheless, Morgan elected to speak to the Carnegie officers in a language they could easily understand, and he continued to do so for many years.

The Carnegie Institution of Washington (CIW) officers supported Morgan’s work partially because they were biased toward Columbia University (see previous chapter), but also because they believed it would bring "capital results" in the form of improved human stock. Beginning in 1915, the CIW paid the salaries for A. H. Sturtevant and Calvin Bridges, Morgan’s students who worked with him from their undergraduate years until he retired. For the next twenty-five years, Morgan returned annually for additional funds, and the CIW never refused. As Morgan collected over $250,000 from the CIW, he became a friend and confidante to the foundation officers. Throughout, the Carnegie officers’ understanding of Morgan’s research remained wedded to eugenics, as they demonstrated in their annual report for 1928:

In the Eugenics Record Office on the hill above us, Dr. [Harry] Laughlin’s unique genetical [sic] researches on the inheritance of physical and psychological characters of thoroughbred horses make direct contributions toward the study of inheritance of physical and mental characteristics of man. But the background,

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705 Grant approvals for $3600, December 29, 1917; $7750, April 16, 1919; $11,000, December 30, 1919; $12,500, December 20, 1920; Folder 3: $11,200, December 16, 1921; $11,385, December 18, 1922; $11,485, December 29, 1923; $11,475, December 12, 1924; $12,475, December 23, 1925; $12,575, December 15, 1926; $12,575, December 13, 1927, folder 4, Thomas Hunt Morgan (1905–1946), Carnegie Institution of Washington Grant files, CIW.
against which these interesting accumulations of experience in observation of the
horse must be projected, is formed by such researches as those on the fruit fly
under the hand of Morgan. . . . The special studies on human inheritance and
development, constituting Dr. Davenport's culminating problems of the Eugenics
Record Office, rest not only upon the broad foundations of researches extending
from genetics through all biological and physical sciences, but connect us in other
directions with a great field of inquiry on specifically human questions.\(^{706}\)

Although Morgan by this time surely would have denied that the study of animal
behavior and genetics could relate directly to an analysis of humans, he never disabused
the Carnegie officers of their assumptions. In fact, he employed eugenics terminology
even as he prepared to move to Caltech. He reassured Carnegie officers that the move
would not inhibit or redirect his research in any way: "the original grant was intended to
give an opportunity to study intensively the constitution of the germinal material . . . to
dig deep into selected problems of heredity" and the project to "locate genes in the germ
plasm" would continue unabated.\(^{707}\) Therefore, while he was preparing to act as the
director of a state-of-the-art laboratory at Caltech, Morgan was adjusting his language to
suit the preconceptions of Carnegie officials.\(^{708}\)

When communicating with the leaders of Caltech or, later, the officers of the
Rockefeller Foundation, Morgan downplayed any correlation between his work and
eugenics, and instead concentrated exclusively on the mechanics of human life as
revealed through physics and chemistry. According to Garland Allen, Morgan was an

\(^{707}\) Morgan to Merriam, December 14, 1928, folder 2, Thomas Hunt Morgan (1905–1946), CIW.
\(^{708}\) Grants for $11,775, December 19, 1929; $11,775, December 15, 1930; $11,775, December 31, 1931;
$10,000 annually from 1932 to 1937, folder 2, Thomas Hunt Morgan (1905–1946), CIW.
ideal fit for Caltech because "his view that the future of biological research rested on the utilization of physical and chemical methodology ... corresponded perfectly with Millikan's belief in the integration of the sciences."\textsuperscript{709} There is much truth to this claim. By the time Millikan finally recruited Morgan to Caltech in 1928, he had established a reputation as a world-renowned genetic researcher who was famous for analyzing complex biological systems using methods inspired by physics and chemistry, particularly efforts to dissect and quantify the impact of physico-chemical processes on development. Morgan's mechanistic understanding of biology was associated with human engineering, as it was motivated by the "seductive empowerment" of a "scientific ideology in which the complexities of the highest levels can be fully controlled by mastering the simplicity of the lowest."\textsuperscript{710}

Developmental mechanics and its associated applications were an integral part of the program that Morgan was hired to build at Caltech. As Hale explained when he wrote to congratulate Morgan about the decision to take over biology at Caltech in 1928, "the very essence of our plan, of course, is to induce" researchers "under your general direction" to conduct research "in general physiology, development mechanics, biophysics, and biochemistry."\textsuperscript{711} Accordingly, Morgan designed the program to address "functional problems" through genetics, embryology, physiology, biophysics, and biochemistry. The program rested on assumptions that both informed human engineering efforts and influenced the policies of the Rockefeller Foundation. Like Weaver would several years later, Morgan emphasized the unity among the biological disciplines:

\textsuperscript{709} Garland E. Allen, \textit{Thomas Hunt Morgan}, 338.
\textsuperscript{710} Lily Kay, \textit{The Molecular Vision of Life}, 17.
\textsuperscript{711} Hale to Morgan, August 8, 1927, folder 9, box 18, series IV, Millikan Papers, Caltech.
It is with a desire to lay emphasis on the fundamental principles underlying the life processes in animals and plants that an effort will be made to bring together, in a single group, men whose common interests are in the discovery of the unity of the phenomena of living organisms rather than in the investigation of their manifold diversity.\textsuperscript{712}

In his laboratory at Caltech, Morgan pledged that his research would reveal fundamental truths about living beings that would ostensibly enable scientists to find the tools to improve mankind. By investigating "life processes" (a term strikingly similar to what Max Mason and Warren Weaver would call "vital processes"), he could unveil the mechanisms that determined how beings functioned.

While the triumvirate was certainly attracted to Morgan due to his experimental approaches, they were equally impressed with Morgan's prowess as a fundraiser and manager of science. Like Millikan, Hale, and Noyes before him, Morgan had proven remarkably adept at operating within the professional science organizations and at building lasting relationships with prominent foundation officers. By the time he accepted the Caltech position, Morgan had been a member of the National Academy of Sciences (NAS) since 1909 and a member of the National Research Council (NRC) since its inception.\textsuperscript{713} For many years, Morgan and Millikan had served as co-vice-chairmen on the executive board of the NAS and, with outspoken support from the triumvirate, Morgan had been elected president of the NAS in 1927.\textsuperscript{714} Throughout the 1920s, Morgan had also become close friends with several foundation officers from Rockefeller boards, including Simon and Abraham Flexner. He was equally tight with Carnegie

\textsuperscript{712} As quoted in Garland Allen, \textit{Thomas Hunt Morgan}, 345.
\textsuperscript{713} Rexmond Cochrane, \textit{The National Academy of Sciences}, 181.
\textsuperscript{714} Garland Allen, \textit{Thomas Hunt Morgan}, 338–339.
officers, who had supported twenty of Morgan's doctoral students for the duration of their graduate educations. Many of his students and research assistants had also received fellowships from the Rockefeller Foundation or NRC.\footnote{Lily Kay, \textit{The Molecular Vision of Life}, 77–80.} Morgan was justifiably heralded as an exemplar of the ideals that both Caltech scientists and foundation officers promoted.

Soon after Morgan arrived at Caltech in 1928, the Rockefeller boards completed the reorganization process that culminated in a new Rockefeller Foundation. Beginning in 1929, the Caltech research programs that had been sponsored by the General Education Board were administered by the newly formed Rockefeller Foundation Division of Natural Sciences. As explained in the previous chapter, Max Mason ran a loosely organized Division of Natural Sciences division until he was elected Foundation president in 1930, at which point he turned the division over to organic chemist Herman Spoehr. By this point, the Foundation had shifted its focus more clearly onto pure research, particularly by promoting interdisciplinary projects in the general fields of "vital processes" and "psychobiology."\footnote{"Rockefeller Foundation Activities" report by Foundation officers for John D. Rockefeller III, June 1930, folder 166, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC; Program and Policy Report PRO-22, 10/29/30, folder 167, box 22, series 900, Record Group 3.1, Rockefeller Foundation Archives, RAC.} As Mason and Spoehr developed policies for the Division of Natural Sciences, they stayed in regular contact with the scientist-administrators at Caltech.\footnote{Folder 66, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.} The Foundation officers made no secret of the fact that they relied heavily on advice from Hale, Noyes, Millikan and Morgan. The phrase "cooperation in research" was repeated frequently, and the idea of linking physical scientists with biological scientists was becoming increasingly attractive. It was no
coincidence that, beginning in 1930, Caltech received millions of dollars from the Rockefeller Foundation.\textsuperscript{718}

Rockefeller funding for Caltech biological sciences began in earnest in 1930, when Mason announced a $1,140,000 (equivalent to $12,300,000 in 2005) grant for Morgan's biology program and $1,700,000 (equivalent to $18,300,000 in 2005) to assist the development of organic chemistry, biophysics, and biochemistry.\textsuperscript{719} The Rockefeller Foundation was just beginning to feel the impact of the stock market crash, but the officers were still confident that Caltech was worth the risk. The grants were conditional: Caltech had five years to raise $3.5 million in matching funds. To fulfill the grant conditions, Millikan turned to local business leaders and philanthropists. In his letters to potential donors, he emphasized several points: Caltech scholarship and research were of the highest caliber, Southern California needed an elite university to fulfill its potential to become a vital, prosperous region, and privately sponsored institutions like his were necessary for the advancement of Anglo-Saxon civilization. Millikan incorporated beliefs about white Anglo-Saxon Protestant superiority that remained pervasive among educated elites of the time.\textsuperscript{720} He also tied the advance of civilization to the development of biological science, thus tacitly acknowledging the eugenics movement as well as the connection between biology research and human engineering.

According to Millikan, the best way for Caltech to contribute to the future of the race and the nation was to design a biology program. In a letter to the wife of William G. Kerckhoff, the donor of the biological laboratories, Millikan wrote,

\textsuperscript{718} Lily Kay, \textit{The Molecular Vision of Life}, 13.
\textsuperscript{719} Grant summary for RF 30080, 12/10/30, folder 66, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Biology at the Institution presents an immense and altogether unique opportunity for the subject is much bigger than physics, less well worked out, indeed almost untouched as yet in the matter of our fundamental understanding of living processes, and yet of universal interest, and of transcending importance to man's life.\(^{721}\)

Millikan continued by asserting that Caltech was uniquely position to contribute to national, social, and racial progress through its biology program:

> Each generation must . . . pass on the accumulated knowledge of the past, and in general make also some contribution of its own to that knowledge; and both of these things are most effectively done, certainly in Anglo Saxon countries, in privately endowed, nongovernmental combined research and educational centers which are obviously more permanent, i.e. less susceptible to political change and influence than are governmental institutions.\(^{722}\)

By linking the future of the biology program at Caltech to a racially-charged conception of national progress, Millikan engaged the hopes and fears of both local donors and foundation officers, for whom he repeated his arguments. His message that privately funded biology research could contribute to the "welfare of mankind" certainly resonated with Rockefeller Foundation officers.

While Millikan worked on his endless fundraising campaign, T. H. Morgan began to build his biological sciences program. Morgan tried to create a cooperative research environment that would satisfy the expectations the Caltech leadership as well as the foundation officers who were supporting his work. In his hiring decisions and project

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\(^{721}\) Millikan to Mrs. William G. Kerckhoff, May 29, 1929, folder 9, box 19, Series IV, Millikan Papers, Caltech.

\(^{722}\) Ibid.
proposals, Morgan relied on two paradigms: the theory of proteins as the basis of all life and the mechanistic interpretation of physiological functions. He began by combing Europe for researchers who would embrace a biology program based on the methodology of physics and chemistry. Using Carnegie funds, Morgan recruited rising star Theodosius Dobzhansky, as well as Calvin Bridges and A. H. Sturtevant from his former laboratory at Columbia, to continue the genetics research on *Drosophila*. Over the next several years, Morgan brought biophysicist Henry Borsook from the University of Toronto, biophysicist Robert Emerson from the University of Berlin, biochemist Kenneth Thimann from the University of London, and physiologists Hermann Dolk, Fritz Went, and Cornelius A. G. Wiersma from the University of Utrecht.\(^{723}\) In 1932, Morgan outlined an ambitious research agenda designed to investigate “the physicochemical nature of replication and mutation in relation to cellular growth and organismic development.”\(^{724}\) Although his plan called for interdisciplinary cooperation, the various lab groups housed in Kerckhoff operated rather independently of one another. Nonetheless, the ostensible lack of true cooperative work did not dissuade Morgan from claiming, in correspondence with foundations, that his group was moving rapidly toward a comprehensive, interdisciplinary understanding of vital life processes.

Morgan was framing his descriptions of the biology program partially in response to feedback that he received from Warren Weaver, who had taken over as director of the Rockefeller Foundation Division of Natural Sciences as Morgan’s program was getting underway. Like the other leading scientists at Caltech, Morgan corresponded frequently with Weaver. Weaver had a deep and abiding affection for Caltech, where he had

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\(^{724}\) Lily Kay, *The Molecular Vision of Life*, 106.
worked briefly as a professor (from 1917 to 1920) before Max Mason had convinced him to return to the University of Wisconsin. He was incredibly fond of Millikan. As late as 1961, Weaver continued to marvel at Millikan’s remarkable abilities as a scientist, administrator and fundraiser:

Dr. Millikan had everything. He was not only a great scientist, but he was so darned handsome, and such a lovely speaker, that when the women’s clubs asked him to give talks to them, when he got done all they wanted to know what, what did Millikan want? Because Dr. Millikan was obviously going to have it. And the wealthy old ladies just began shelling out for him. He was extremely successful in getting wealth out of southern California . . . Robert Millikan did a magnificent piece of development there . . . California Institute of Technology was technically run by a Council . . . but if there was ever an institution that had a single top administrative officer it was California Institute of Technology under Robert A. Millikan. . . . He was so far up you couldn’t touch him. He did a marvelous job. Of course, what has grown out of it is a very, very wonderful thing, a thing in which I have very deep interest. I still hold a professorship at California Institute of Technology. . . . It’s a wonderful place, a perfectly wonderful place.\footnote{Warren Weaver, Columbia University Oral History Project, Volume One, March 19, 1961, pp. 66–68, Columbia.}

Weaver was definitely enamored with Caltech and he wanted to find ways to channel even more Rockefeller funds toward the research programs there.

Weaver’s philosophy of science fit seamlessly with the approach that Millikan, Noyes and Morgan had adopted at Caltech. When Weaver suggested that the Rockefeller
Foundation should design a biology program based on physics and chemistry, he surely had the Caltech model in mind. The “vital processes” program that Weaver constructed during his first years as director of the Division of Natural Sciences easily justified the tremendous amount of financial support that the Rockefeller Foundation provided for Caltech over the next decade.\textsuperscript{726} The degree of overlap between the language of Rockefeller Foundation Natural Sciences policies and the descriptions of the Caltech biology and chemistry programs continued to increase. His comprehension of the function of biology was based largely on the same assumptions that guided Caltech scientists and leaders.

Over the course of 1933, Morgan constructed his rationale for his biology program in close consultation with Rockefeller officers, who in turn revised their policies based on the promises that Caltech scientists made. A focal point of Weaver’s “vital processes” program was the idea that biology needed to “catch up” to the physical sciences: “Biology is today in a position in some ways analogous to that occupied by physics and chemistry many years ago. It has, that is, advanced . . . into the stage of detailed quantitative analysis.”\textsuperscript{727} He and Mason consistently communicated to Caltech scientists that the “vital processes” program would be strictly followed when allotting grants due to tightening budgets at the Rockefeller Foundation in the wake of the Great Depression.\textsuperscript{728} Morgan was anxious to satisfy the “vital processes” agenda in his proposals because, as he explained to the Rockefeller officers in a series of letters in the

\textsuperscript{726} “The Benefits from Science” by Warren Weaver, January 27, 1933, p. 9, folder 6, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{727} “The Natural Sciences—Program and Policy” presentation by Weaver to Trustees, April 11, 1933, p. 77, folder 6, box 1, series 915, Record Group 3.1, Rockefeller Foundation Archives, RAC.

\textsuperscript{728} Mason diary, April 20, 1933 re: conversation with Millikan, folder 67, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
spring of 1933, his biology program was in serious financial trouble. The Great Depression was hurting every organization that relied heavily on endowment funds. The Arthur Fleming Trust, which supported Morgan’s laboratory, had collapsed. His research group was kept afloat exclusively through foundation grants, including installments from the CIW that he used to pay his loyal research group members, Sturtevant and Bridges.\(^\text{729}\)

Weaver was alarmed to hear that Morgan’s lab was in financial jeopardy. Over the next few months, Weaver ensured that the Foundation responded to Morgan’s request for supplemental funds. As he saw it, the Caltech situation was an “emergency” that required instant attention to rescue genetics, biophysics and biochemistry.\(^\text{730}\) In October 1933, Weaver went to Caltech to help Morgan devise a research plan that dovetailed more obviously with the “vital processes” policy.\(^\text{731}\) After the visit, Morgan repeatedly sought Weaver’s advice about restructuring the biology program to create a more cooperative, interdisciplinary environment that could serve as a model of “vital processes.”\(^\text{732}\) In his official grant application, Morgan assured the Rockefeller Foundation officers that Caltech would create a bold, innovative research program that would incorporate chemists, geneticists, biochemists, and physiologists in an effort to investigate vital processes from multiple perspectives.\(^\text{733}\) The Foundation responded with a grant for $50,000 a year for three years beginning in 1933 for Morgan’s laboratory,

\(^\text{729}\) Morgan to Mason, May 15, 1933, folder 24, box 32, series IV, Millikan Papers, Caltech.
\(^\text{730}\) Weaver diary, September 8–10, 1933, folder 71, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\(^\text{731}\) Morgan to Millikan, September 9, 1933, folder 14, box 18, series IV, Millikan Papers, Caltech.
\(^\text{732}\) Weaver diary, October 23–25, 1933 re: visit to Caltech and meeting with Morgan, folder 71, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\(^\text{733}\) Morgan to Weaver, November 9, 1933, folder 71, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
which would supplement the conditional millions that had already been set aside as a conditional grant for the endowment of Caltech biology and biochemistry.\footnote{734}

By the end of 1933, Weaver was satisfied that Morgan’s program of investigating genetic mutations, mapping genes on chromosomes, and connecting genes to expressed traits defined the cutting edge of research.\footnote{735} Although the cooperative aspects of Morgan’s research were never fully realized, the fact that he won the 1933 Nobel Prize in Physiology for chromosome gene mapping bought him some time; he was hardly going to be under scrutiny when he was in the midst of celebrating such an achievement. Weaver looked past the difficulties that Morgan was having and maintained his enthusiasm about the prospects for interdisciplinary biological research. He described Morgan’s program as a highly sophisticated application of developmental mechanics:

General physiology, as exemplified by Jacques Loeb, is especially concerned with the treatment of basic problems by chemical and physical techniques. . . . Modern physiology is often concerned with cells, single nerve fibres, and tissues rather than with whole organs. The refined modern techniques are permitting a breaking up of impossibly complicated problems into simpler component parts . . . The program at the California Institute of Technology is primarily concerned with studies designed to bridge the gap between the gene-chromosome theory of genetics and the developed characteristics of the mature organism.\footnote{736}

Thus, in the mid-1930s, Morgan was running a research program that looked very similar to the one that he had begun years earlier. Despite his numerous references to his desire

\footnote{734} Thompson to Millikan, December 19, 1933, folder 24, box 32, series IV, Millikan Papers, Caltech.
\footnote{735} Raymond Fosdick, \textit{The Story of the Rockefeller Foundation}, 163.
\footnote{736} RF 35047, Grant for $20,000 a year from 1935 to 1937, General Statement by T.H. Morgan, 4/17/35, folder 70, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
to encourage cooperative research, Morgan was more comfortable operating within the boundaries of his own discipline. Throughout the early 1930s, the Rockefeller officers were pleased enough with Morgan to downplay his lackluster attempts to organize cooperative programs. To achieve their hopes for interdisciplinary research programs, the foundations turned to a brilliant, young chemist who was skilled in chemistry, physics and mathematics, and also showed great promise in biology: Linus Pauling.

Linus Pauling (1901–1994) first came to Caltech as a graduate student in 1922 after completing an undergraduate degree in chemical engineering at Oregon Agricultural College (OAC). He had attended OAC, which gave him a scholarship for his first year of college when he was sixteen, because it was the only institution of higher education that he could afford. Within a few months of Pauling’s arrival on the OAC campus, his professors had realized that he was a prodigy and had given him the opportunity to pay for the remainder of his education by teaching chemistry courses. By the time he reached the age of eighteen, Pauling had already embraced the life of a professor; his free time was spent reading journals, conducting experiments, and tutoring students. As he was perusing science journals, he had come across Irving Langmuir’s paper, “The Arrangement of Electrons in Atoms and Molecules.” Langmuir’s paper was a “sixty-six page tour de force” that built on the work of University of California chemist Gilbert Lewis. Lewis and Langmuir were attempting “to bring chemistry in line with some of the baffling things physicists were discovering about the structure of atoms.”

Atomic structure was of prime interest to physicists and chemists at the time, but they were not communicating across disciplinary boundaries. As Langmuir wrote in the paper that sparked Pauling’s interest in the field of physical chemistry:

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The problem of the structure of atoms has been attacked mainly by physicists . . . who have given little consideration to the chemical properties which must ultimately be explained by a theory of atomic structure. The vast store of knowledge of chemical properties and relationships, such as is summarized by the Periodic Table, should serve as a better foundation for a theory of atomic structure than the relatively meager experimental data along purely physical lines.\textsuperscript{738}

By utilizing their knowledge of chemical behavior based on the Periodic Table, Lewis and Langmuir had determined that molecules were formed through the sharing of electrons. Their cubical model of the atom was later overthrown, but their focus on the importance of electrons in bond formation was not. More importantly, they had inspired a generation of physical chemists, including Pauling. In his application for the graduate program in chemistry at Caltech, Pauling had referred to Langmuir and Lewis, and had described his future plans as research into the location of electrons in atoms.\textsuperscript{739} When he read Pauling’s application, Noyes decided that the project had tremendous potential.

Noyes had assigned Pauling to Roscoe Dickinson’s x-ray crystallography lab, where the young chemist became an expert at inorganic molecular structural analysis. Pauling was such a fast learner and hard worker that Noyes became determined to keep him at Caltech after he completed his Ph.D. After a brief flirtation with Stanford and a trip to Europe under the Guggenheim fellowship, Pauling submitted to Noyes’s exhortations and came to Caltech as Assistant Professor of Theoretical Chemistry in 1927. That very year, Pauling published a paper that established his national reputation as a chemist, “The Theoretical Prediction of the Physical Properties of Many-Electron

\textsuperscript{738} As quoted in Hager, Force of Nature, 60.
\textsuperscript{739} Thomas Hager, Force of Nature, 47–58.
Atoms and Ions, Mole Refraction, Diamagnetic Susceptibility, and Extension in Space.”

In essence, he argued that ionic size was determined by the outermost layer of electrons and that electron layers’ distance from the nucleus was measurably affected by magnetic repulsion that drove electrons apart. Using the same concept of magnetic repulsion, Pauling also contended that the distance between the nuclei of atoms within crystal formations was largely determined by the interactions between protons. Much of his inspiration for the 1927 papers came from discussions with German quantum physicists that he met while in Europe, and he wanted to test his theories in the lab. When he returned to Caltech, Pauling leapt at the opportunity to use the world-class x-ray crystallography equipment that Noyes had assembled at Gates Laboratory.⁷⁴⁰

Over the next two years, Pauling analyzed hundreds of inorganic substances using x-ray crystallography. He made another breakthrough in 1929 when he posited a set of rules governing ionic crystal formation using a combination of his experimental knowledge of crystal structure and his theoretical understanding of quantum mechanics. “Pauling’s Rules,” as they came to be known, were published in the Journal of the American Chemical Society, and the article wowed chemists throughout the world. That same year, Pauling combined quantum mechanics and crystal analysis to reveal the heat capacity of solids and the molecular structure of silicates, prompting Harvard, MIT, and Michigan to offer him professorships. To keep Pauling, Noyes offered a promotion, a substantial pay raise, a lab assistant, and travel money. He also used Carnegie and Rockefeller funds to finance the construction an electron-diffraction machine, which Pauling ultimately used to determine the structure of 225 molecules. In 1931, Pauling won the American Chemical Society’s Langmuir Prize (named for the aforementioned

⁷⁴⁰ Ibid., 108–131.
author of the electron theory) for best young chemist in the United States, was promoted to full professor at Caltech, and published a series of acclaimed papers on the nature of covalent chemical bonds in crystals. He was thirty years old.\textsuperscript{741}

Up to this point in his Caltech career, Pauling had paid little or no attention to biology or organic chemistry. Yet, Noyes needed someone to inaugurate the organic chemistry and biochemistry program that he had proposed to the Rockefeller Foundation in order to get a $500,000 grant. In his application to the Rockefeller Foundation in 1930, Noyes had presented organic chemistry and biochemistry as necessary for achieving a fully realized research program at Caltech. He had asserted that organic chemistry should be encouraged “in directions germane to the research developments at the Institute in physical chemistry and subatomic physics, and in biochemistry and biophysics.”\textsuperscript{742} There was a sense of urgency in his proposal:

Lack of funds and of laboratory space . . . has . . . prevented any adequate permanent provision for organic chemical work; but recent growth in related directions now makes it important that such provision be made as soon as possible. . . . One of these is theoretical in a fundamental sense, in that it concerns itself with the interpretation of the molecular structure of carbon compounds and of the occurrence and rate of organic reactions, from the viewpoints of modern physics and subatomic chemistry. The other is biological in its bearing, in that it concerns itself with the study of those substances and reactions that are of special interest to . . . physiologists.\textsuperscript{743}

\textsuperscript{741} Ibid., 159–171.
\textsuperscript{742} Noyes proposal to Rockefeller Foundation, November 8, 1930, folder 66, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\textsuperscript{743} Ibid.
While Noyes explicitly referred to a cooperative research plan that would build on his physical chemistry program to incorporate organic chemistry and the biological sciences, he made no mention of the personnel who would coordinate the enterprise.

In 1932, soon after he took over as director of the Rockefeller Foundation Division of Natural Science, Weaver began to communicate with Noyes about using the grant to support organic and biochemistry research under Pauling's direction. Weaver was too impressed with Pauling to be dissuaded by the young chemist's lack of experience or interest in organic chemistry or biochemistry. Using an understanding of the direction of biology that he had learned from T. H. Morgan, Weaver encouraged Pauling to shift his emphasis to research into proteins and the molecular structure of organic molecules. When Pauling applied for his own Rockefeller grant in April 1932, Weaver specifically instructed him to include a sentence or two on the potential biological applications of his research.\footnote{Raymond Fosdick, \textit{The Story of the Rockefeller Foundation}, 158--159; Thomas Hager, \textit{Force of Nature}, 186--187.} Despite some initial resistance on Pauling's part, he did include a line on his application stating that his use of x-ray crystallography and electron diffraction to analyze inorganic molecules could be broadened and thus "may be of great importance to biochemistry, resulting in the determination of the structures of proteins, haemoglobin [sic], and other complicated organic substances."\footnote{"A Program of Research in Structural Chemistry" by Linus Pauling, submitted to Warren Weaver April 1932, folder 70, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.} Weaver ensured that Pauling received a grant of $20,000 a year for the next two years. From that point on, Weaver frequently advised Pauling on how to apply effectively for additional funds. After some initial resistance, Pauling began to accept that he would
have the greatest likelihood of long-term financial opportunity if he concentrated increasingly on the study of organic molecules and biochemistry.

When he reported to the Rockefeller Foundation on the progress of his research in 1933, Pauling wrote, “Our methods have been found to be particularly valuable in the treatment of the complicated problems of organic chemistry, and we are now devoting the major part of our effort to these problems.” He went on to describe that his research into molecular structure was revealing much about “purpurine, chlorophyll, hemoglobin, and other substances of biological importance.” Pauling composed his report while Weaver was visiting Caltech and the two worked together to frame the information in language that Foundation officers would find suitable. At the time, due to the collapse of Fleming Trust in the wake of the stock market crash, other Caltech professors were struggling to maintain their research facilities and to pay their assistants. Most of the professors were also accepting lower salaries. Due to his close relationship with Weaver, Pauling was able to depend on Rockefeller funds for assistant salaries, equipment, and other needs. Yet, the relationship was contingent on Pauling pursuing research with biological applications because his program had to fit with the strict “vital processes” policy.

In 1934, Pauling’s $10,000 grant was renewed for only one year because Foundation officers doubted the connections between Pauling’s research and the Rockefeller “vital processes” program. Pauling realized that he needed to listen to Weaver if he wanted Foundation support to continue. As Pauling noted, “It seemed

746 “A Brief Account of Research in Chemistry Support by Grant from the Rockefeller Foundation” by Linus Pauling, submitted to Warren Weaver October 24, 1933, folder 71, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
747 Weaver diary, October 23, 1933 re: visit to Caltech, folder 71, box 5, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
pretty clear to me that I would have difficulty in getting further support from the
Rockefeller Foundation unless I became interested in chemistry in relation to biology. 748

In November 1934, Pauling applied for a Rockefeller grant to investigate exclusively
organic, biologically relevant substances, especially proteins, enzymes, and hemoglobin.
He promised to make major progress “on difficult structural problems such as arise in
biochemistry” and received a grant for $30,000 a year over three years. 749

It was through this grant that Pauling began to forge links between his research
and the work Morgan was doing at the Kerckhoff lab. The Rockefeller officers credited
themselves for bringing the two research groups together at Caltech and contributing to
the effort to engender cooperation in research. 750 Weaver visited Pauling regularly and
reported to Foundation officers that his prized chemist was making rapid progress in
biologically relevant research. 751 On March 6, 1936, Weaver visited Pauling, Noyes, and
Morgan. His comments on the visit reveal much about how the Foundation was
influencing connections between chemistry and biology at Caltech. In reference to
Pauling, Weaver wrote,

Within one or two years P[auling] plans to write a review monograph on their
haemoglobin [sic] work, with particular emphasis on . . . results as will make
these understandable and useful to biochemists, physiologists, etc. P’s interests
turn more and more to the biologically important compounds. 752

748 As quoted in Thomas Hager, Force of Nature, 188.
749 Pauling to Weaver, November 26, 1934, folder 73, box 5, series 205D, Record Group 1.1, Rockefeller
Foundation Archives, RAC.
751 Weaver diary 4/5/35, folder 81, box 6, series 205D, Record Group 1.1, Rockefeller Foundation
Archives, RAC.
752 Weaver diary, March 6, 1936, folder 75, box 6, series 205D, Record Group 1.1, Rockefeller Foundation
Archives, RAC.
At the same time, Weaver reported that Noyes was still planning to join chemistry with biology, but he needed additional funds to complete the process:

Noyes reviews financial situation in chemistry dept. and his concern to build, equip and staff as soon as possible the connecting link between the present chemistry building and the biology laboratory. This would house Pauling’s work and, presumably, a bio-organic chemist of front rank. M[ax] M[ason] and W[arren] W[eaver] express their real interest in such a development.\footnote{Ibid.}

It would appear that Weaver and his Rockefeller funds were driving Pauling and Morgan together toward a cooperative attack on the structure and function of proteins and other organic molecules. Weaver certainly believed that, with Pauling’s help, he could facilitate the cooperation in research that Caltech had championed in applications for so many years.

While Weaver encouraged the Caltech program to move toward cooperation between biology and chemistry, the Caltech scientists also influenced the Rockefeller Foundation officers’ understanding of the implementation of the “vital processes” policy. In her critique of Rockefeller Foundation policy in the 1930s, Pnina Abir-Am argues that Weaver felt he needed Pauling to provide coherence for the “vital processes” program. He also needed Pauling to demonstrate that applying the instruments and methods of chemistry and physics to research in the biological sciences would yield important results.\footnote{Pnina Abir-Am, “The Discourse of Physical Power and Biological Knowledge in the 1930s,” 359.} Although I disagree with other aspects of her argument, Abir-Am makes an excellent point in this regard: Weaver saw Pauling as a genius whose almost certain
future success, especially in collaboration with Morgan, would reflect beautifully on the Division of Natural Sciences policy. As Weaver remembered his feelings on the subject:

Cal Tech was so obviously a place in which imaginative basic science was going to prosper, and Linus Pauling was so obviously a person who combined superb command of technique with real fundamental imagination. . . . . It was so clear that Linus had it that we would have been very stupid indeed if we hadn’t latched onto him. 755

Thus, it should come as no surprise that Pauling’s approach to organic chemistry and the biochemical analysis of proteins both justified and transformed Weaver’s Natural Science policies.

By the late 1930s, Weaver had shifted his focus from “vital processes,” “psychobiology,” and an interdisciplinary “new science of man” to a concentration on the investigation of the structure and function of the microscopic components of living beings. As Robert Kohler has noted, the shift in Division of Natural Sciences policy signified “a decline in the invocation of utility, a reduction in the support of those aspects of endocrinology, sex research, and nutrition associated with clinical application; and an increasing reference to the physical sciences, particularly organic chemistry.” 756 Kohler describes what I have defined as a move from one phase of the human engineering effort to another, a turn from the “human biology” and psychobiology ideas that informed Weaver’s early program toward a fully mechanistic, quantitative understanding of biology and chemistry. While Kohler does not mention the role of Caltech in his

analysis, I would argue that Weaver’s policy changes were related to his intimate knowledge of and devotion to the Caltech research program. Through Pauling’s efforts to work with the biology group that Morgan had put together at the Kerckhoff laboratory, Weaver was witnessing the realization of an enduring hope: that the Rockefeller Foundation could guide and finance collaborative research that would unravel the mysteries of living beings. The implication was still that the results of the collaboration would yield results that could improve mankind, and Pauling helped Weaver to achieve that dream as well.

Beginning in 1936, several events conspired to bring the process of unifying the biological and physical sciences to full realization, and any remaining obstacles to the “cooperation in research” fell away. First, retired steel magnate Edward W. Crellin donated the necessary funds to build an organic chemistry laboratory. Then, Arthur A. Noyes died and Warren Weaver became involved in an effort to reconfigure the chemistry department with Pauling in charge. Finally, the Rockefeller Foundation offered a grant that was specifically designated for a joint biology-chemistry project directed by Pauling and Morgan, in which the two lab groups worked together to conduct a thorough investigation into the structure and function of genes, hormones, proteins, and enzymes.757

Arthur Noyes had spent the last years of his life trying to solicit funds to build an organic chemistry lab that Pauling could use as a bridge to the biology department. When Noyes secured $350,000 toward the lab project from Edward Crellin in 1936, he immediately began to work with Pauling to get additional support from the Rockefeller Foundation.

Foundation. After Noyes died in June, Pauling assumed that he would become the
cchair of the Department of Chemistry and, upon its completion, the director of the new
Crellin Laboratory. His fellow chemistry professors had other ideas. Much to Pauling’s
surprise and chagrin, most of the professors in the chemistry department believed that he
had hastened Noyes’s death by being too demanding about the organic chemistry lab
project. The other chemistry professors ostracized Pauling, whom they saw as too young
and aggressive to be a suitable replacement for the revered Noyes. In the most overt snub
and expression of “anti-Pauling feeling,” every member of the Caltech Executive Council
and Department of Chemistry was asked to be an honorary pall-bearer at Noyes’s funeral
except Pauling.759

Later that year, the Caltech Executive Council asked Richard C. Tolman to be
chairman of the Department of Chemistry even though everyone knew that he was
planning to retire, thus insulting Pauling again. As expected, Tolman turned down the
job. Millikan then offered the chairmanship to Pauling, who responded by writing a
scathing letter to the Executive Council refusing the position.760 A combination of
Pauling’s hurt feelings and righteous indignation had placed the entire organic chemistry
project in jeopardy. Enter Warren Weaver. After hearing from Millikan about the
problems with the chemistry department, Weaver came to Caltech in January 1937 to
visit Pauling. According to Weaver’s diary, he became an intermediary between Millikan
and Pauling to resolve the contentious situation:

758 Lily Kay, The Molecular Vision of Life, 152.
760 Weaver diary, January 30, 1937 re: visit to Caltech, folder 76, box 6, series 205D, Record Group 1.1,
Rockefeller Foundation Archives, RAC.
Pauling seemed sincerely apologetic, and Weaver suggested writing a new letter, since Millikan had not submitted the “impudent” version out of embarrassment. Then, Weaver and Mason smoothed things over with Millikan, promising that Pauling would become a gracious chair of the department if given a second chance. Pauling was soon installed as chairman of the Department of Chemistry and director of both the Crellin and Gates labs. His first act as chair was to put together a large grant proposal to the Rockefeller Foundation.

When Pauling drafted his proposal to the Foundation in 1937, he already knew what Weaver and the other officers wanted to see happen. In October 1936, Rockefeller Foundation officers had composed a memo outlining their hopes for the future of Caltech research:

The development of biology under Professor T. H. Morgan has been, to date, concentrated in the fields of genetics, physiology, and biochemistry. In the Chemistry Department, on the other hand, the researches of Linus Pauling have been turned, during the last few years of RF assistance, towards the problems of structure of the large and complicated molecules of biologically important substances. Professor Pauling has keenly felt the need of co-operation with an active research department of organic chemistry. This field has not been

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Weaver diary, January 31, 1937 re: visit to Caltech to meet with Pauling, folder 76, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
developed at Pasadena, but would furnish an essential link between the more theoretical researches of Professor Pauling and the biochemistry, physiology, etc., under Professor Morgan.\footnote{762}

When Weaver visited Caltech in January 1937 and acted as a negotiator for Pauling, he was actually on campus to work out the details of the interdisciplinary program that the Foundation officers had drafted in the October 1936 memo.\footnote{763} Once Pauling’s situation with the Department of Chemistry and the Caltech Executive Council had been handled, Weaver was able to return to the business at hand. Throughout the spring of 1937, Millikan and Weaver corresponded regularly and informally (they addressed each other as “Rob” and “Warren,” respectively) about how to organize what they began to call the “bio-organic chemistry program.” Millikan asked Weaver for advice on personnel, equipment, and research, always with the underlying message that Rockefeller support was necessary to complete any plans.\footnote{764}

Weaver was excited to be a part of the interdisciplinary “bio-organic chemistry” project and took it upon himself to ensure that the Caltech application would be successful.\footnote{765} He wrote to Pauling to suggest that the proposal include more details about the specific projects that the research groups would complete using the funds. According to the final proposal, Pauling would direct research projects that included separating polypeptide chains, studying enzymatic reactions, analyzing the structure of hemoglobin and other natural substances like vitamins and hormones, and completing

\footnote{762} RF Officer Memo re: Caltech Natural Science, October 5, 1936, folder 75, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\footnote{763} Weaver diary, January 29, 1937, folder 76, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
\footnote{764} Weaver to Millikan, March 11, 1937; Millikan to Weaver, March, 1937, folder 4, box 18, series IV, Millikan Papers, Caltech.
\footnote{765} Millikan to the officers of the Rockefeller Foundation, March 27, 1937, folder 76, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
spectroscopic analysis of hydrogen bonds, while Morgan, Sturtevant, and Bridges would conduct "a chemical attack on the structure of chromosomes," all for the bargain price of $300,000 over five years. While Weaver was an enthusiastic supporter of the project and had tremendous faith in both Pauling and Morgan, he wanted to justify such a substantial grant to his fellow officers. Weaver corresponded with a number of experts in chemistry, mostly Rockefeller grant recipients, to assess the general opinion on Caltech's prospects. He received an overwhelmingly positive response. Then, he visited Caltech again to interview Pauling and left with a much better understanding of the current debates in protein research. In particular, he learned about Pauling's and Morgan's intentions to test Dorothy Wrinch’s cyclol theory, a mathematically elegant formulation that posited a protein structure consisting of flexible rings of molecules. Weaver had done his homework and he convinced the other Rockefeller officers to support the Caltech application. On December 4, 1937, the Foundation officially notified Millikan of a grant for $300,000 for a six year period beginning on July 1, 1938 "towards support of developments in chemistry in its relationship to biological problems" to be directed by Pauling and Morgan.

In many respects, Morgan and Pauling were perfectly matched. They both believed that molecules, especially proteins, held the key to understanding the fundamental mechanisms governing living beings, and they both trusted in the capacity for well-calibrated instrumentation to reveal the true nature of organic molecules.

766 Millikan to Weaver, August 7, 1937, folder 77, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
767 Weaver diary, November 1, 1937 re: visit to Caltech, folder 78, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
768 Thompson to Millikan, December 4, 1937, folder 78, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC.
Though outside forces initially pushed Morgan and Pauling into a cooperative project, the two scientists quickly realized that combining their research groups provided an outstanding opportunity to investigate proteins and molecules from multiple disciplinary perspectives. The result of the cooperative effort was the development of a new field of study, molecular biology, a term that Warren Weaver coined after he observed the research projects at Caltech.

The success of the partnership was exhilarating for everyone involved. Millikan was especially thrilled about the influx of funds coupled with the opening of the Crellin Laboratory. In the June 24, 1938 issue of *Science*, Millikan announced his excitement to the scientific community at large:

The facilities for a frontal attack on the most pressing problems of bio-organic and structural chemistry—that is on the problems of life itself—are now provided through the joint interest and generosity of Mr. and Mrs. E.W. Crellin and the Rockefeller Foundation. With oarsmen like these and those found in the group of institute chemists “pulling at the bars,” the chemical bark of the California Institute of Technology can scarcely fail to win in the race for human betterment through chemical and biological advances.\(^{769}\)

These were lofty claims, but they were in keeping with both the Caltech and Rockefeller Foundation philosophy of science. The hope remained that analyzing living beings on a structural, molecular level would provide the tools necessary to guide human progress. The human engineering project was alive and well in the ambitions of the administrators and patrons of Caltech “bio-organic chemistry.”

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Guided by these same assumptions, Weaver celebrated the Caltech project in the Rockefeller Foundation Annual Report for 1938 as the culmination of years of work toward utilizing the methodology and instruments of the physical sciences to improve the efficacy of biological research. It was in this report that Weaver coined the term “molecular biology,” a field of science effectively inaugurated at Caltech:

In the twentieth century, the physical sciences have probed inside the atom . . . In that progress toward greater refinement of detail, the biological sciences have also shared. Using many of the delicate and exact instruments which the physicist devised in his attack on inorganic material, they are beginning the description and analysis of biological phenomena, not in terms of cells as units, but in terms of genes and other important subdivisions of cells; and recently even in terms of molecular structure and forces . . . A new biology—molecular biology—has begun . . . it is by means of the new tools and techniques developed . . . by the physical sciences that the door to a biology of molecules has recently been opened. 770

With his continued emphasis on the precedence of physics and the importance of technology, Weaver layered the mechanistic conception of life onto the latest developments in biology. Pauling and Morgan were perfectly happy with that approach.

Before he retired in 1939, Morgan secured Carnegie funding for Sturtevant and Jack Schultz, a promising researcher who had earned his Ph.D. with Morgan in 1929, to help them take over the work with the researchers at the Gates Laboratory. 771 After

retirement, Morgan stayed informed about the research at Gates and served as the primary liaison for the foundations. His research group also made great strides in clarifying the relationship between genes and cell biochemistry, as well as discoveries in molecular genetics about gene structure, chromosome mapping, gene replication, and mutations. In terms of the cooperative efforts supported by the Rockefeller grant, Morgan contributed his expertise to two research projects that Pauling directed: investigations into protein structure and into the formation of antibodies.

The years leading up to the United States entry into World War II were extraordinarily productive for Pauling, who began to spend more and more time working with the biologists at Caltech to link the researchers from the Crellin and Gates laboratories with the researchers from the Kerckhoff lab. In 1939, using a detailed analysis of hydrogen bond formation and of restricted rotations around covalent bonds in polypeptide chains, Pauling dismantled Dorothy Wrinch’s cyclol theory. His article, “The Structure of Proteins,” established Pauling as the leader in the field of protein chemistry.\textsuperscript{772} That year, he also published The Nature of the Chemical Bond and the Structure of Molecules and Crystals: An Introduction to Modern Structural Chemistry, which became the standard chemistry text in major universities, went through three editions, and was translated into seven languages.\textsuperscript{773}

In a report to Weaver in 1940, Pauling explained that he had assembled an exceptionally productive, interdisciplinary research group including organic chemists Carl Niemann, E. R. Buchman and J. B. Koepfli, as well as A. J. Haagen-Smit from the

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\textsuperscript{772} Lily Kay, \textit{The Molecular Vision of Life}, 160.

Biology Department. In terms of protein research, Pauling boasted, "our laboratory remains the only one in which any complete structure determinations of amino acids and peptides have been made." He also reported, "I have developed a theory of the structure of antibodies and the nature of the process of their formation." Within a year, Pauling published "A Theory of the Structure and Process of Formation of Antibodies" in the Journal of the American Chemical Society and reinvigorated the field of immunology. His model posited that physical energy forces were responsible for the attraction between antibody and antigen. The antibody would denature the antigen by molding to it through electrostatic attraction, a kind of "hand-in-glove" relationship between T-cell and bacteriophage. Pauling's model, which was a digestion of and improvement on the work of many predecessors, inspired other researchers to explore the physical and chemical aspects of the relationship between biological molecules. Pauling was exceeding every expectation that Weaver had. The Rockefeller officers threw money at him. In May 1941, Pauling received an additional $33,000 grant for three years to aid his "general program of development of the field of structural chemistry and . . . its application to the more and more complex problems of biology."

The entry of the United States into World War II severely disrupted the research programs in all divisions at Caltech. The structure of funding also changed radically due to increased involvement by the federal government during and after the war. Rockefeller support continued and Pauling's research proceeded apace after the war, though Weaver was less intimately connected than he had been in the pre-war years.

774 Pauling to Weaver, April 6, 1940, folder 83, box 6, series 205D, Record Group 1.1, Rockefeller Foundation Archives, RAC. 775 Ibid. 776 Pauling to Hanson, March 18, 1941; Thompson to Millikan, May 29, 1941, folder 28, box 32, series IV, Millikan Papers, Caltech.
Still, building on the earlier work that he had done with Rockefeller support, Pauling made numerous other revolutionary achievements related to the biological sciences in the 1940s. His most notable accomplishment was his model of DNA, called the “alpha-helix.” The alpha-helix was a spiral of amino-acid chains held together by hydrogen bonds that was revolutionary because it posited that DNA must be modeled in three dimensions (rather than as a planar structure). The “helix” model was “a sensation” that prompted other researchers, including the famous pair of James Watson and Francis Crick, to explore the structure of DNA in three dimensions.\textsuperscript{777}

In 1954, Pauling was awarded the Nobel Prize for Chemistry. The prize was “given in recognition of everything he had done with the chemical bond from 1928 to the alpha helix.”\textsuperscript{778} In a highly unusual move, the Nobel officials had awarded Pauling for a lifetime of achievement as a “bio-organic” chemist rather than isolating a specific project. He had clearly become a pioneer in the field. My question is, would Pauling have begun to apply his expertise in physical chemistry to the study of organic molecules if Weaver and the Rockefeller Foundation had never become involved? Would he have been able to continue the research that led to his groundbreaking discoveries, especially during the 1930s, without the support from his Foundation sponsors? While it is difficult to determine the extent to which Weaver helped to shape Pauling’s career specifically, there is no doubt that the remarkably successful Caltech experiment in “cooperation in research” would not have been fully achieved without foundation intervention.

Caltech represented the epitome of “cooperation in research,” however defined. The most adept scientific entrepreneurs in the United States—Hale, Millikan, and

\textsuperscript{777} Thomas Hager, \textit{Force of Nature}, 373–375.
\textsuperscript{778} Ibid., 450.
Noyes—had cooperated to organize and build Caltech into a prestigious research center in the physical sciences. Hale was the original champion of "cooperation in research," and Caltech was his proudest achievement after the National Research Council. The efforts of the Hale-Millikan-Noyes triumvirate established an intellectual and administrative framework based on the ideal of cooperation, a framework that formed the bases for the many innovations that Caltech scientists made in the 1920s and 1930s. At Caltech scientists cooperated with trustees and acted as administrators. They also cooperated with foundation officers to an extraordinary extent. Ultimately, and most importantly, the scientists cooperated with each other to define new fields of specialty research, including molecular biology.

Weaver deserves credit for his exertions on behalf of Pauling, and the Rockefeller Foundation clearly played a significant role in the remarkable development of molecular biology research at Caltech. The Rockefeller Foundation was the most important component within a matrix of individuals and institutions that molded Caltech during the interwar period. In the 1920s, Noyes and Millikan set the stage for the cooperative program that Morgan and Pauling brought to fruition by establishing a university devoted to the physical sciences, equipped with the latest laboratories and instrumentation, and run by scientists. The triumvirate was successful in raising the funds to build outstanding laboratories filled with renowned scientists because they had ample support not only from the Rockefeller Foundation, but also from Carnegie philanthropies and local donors. Pauling and his research partners used the resources provided by their predecessors to make their award-winning discoveries. "Cooperation in research" was in evidence at Caltech between the world wars in every respect and the results were truly phenomenal.
Where Weaver made his most significant impact was in his negotiations with Pauling about the type of research the young physical chemist would undertake, especially the idea that he should join forces with T. H. Morgan and the biologists at the Kerckhoff laboratory. Weaver was intent on fulfilling his mission to create “mixed marriages” between physical and biological scientists because, after working with biologists at Stanford and the University of Chicago and learning about the developmental mechanics approach espoused by Morgan, he had come to believe that the best way to achieve human engineering goals was to dissect living beings based on their physico-chemical components. The emphasis on controlling human behavior was transformed into an emphasis on mapping the “molecular labyrinth of the human soma and psyche in order to control biological destiny.”\textsuperscript{779} The endorsement of developmental mechanics in biology was the truest embrace of human engineering; the body as machine was rendered comprehensible, malleable, and eminently fixable. Support for Pauling’s research, especially the research that he conducted with Morgan, was thus infused with hopes tied to the reinvention of the “science of man” in terms of the investigation and control of the structure and function of living beings on a molecular level.

Between the turn of the twentieth century and World War II, what had begun as an ill-defined and indeterminate human engineering effort by philanthropic foundations had been gradually transformed into a justification for defining and funding molecular biology research. Beginning in the late nineteenth century, when the belief that scientific innovation could improve mankind had captured the imaginations of Andrew Carnegie and John D. Rockefeller, philanthropists and scientists had worked together to apply scientific research to the goal of improving mankind. The belief that scientific research

\textsuperscript{779} Lily Kay, \textit{The Molecular Vision of Life}, 17.
could improve mankind was vague and astonishingly optimistic, but it was also immensely powerful, especially when it was coupled with millions of dollars. This hope for improving mankind that had motivated the creators of the major philanthropic foundations continued to encourage foundation officers as they attempted to engineer a better world by engineering better humans.

In the early phases of the tacit human engineering effort, the foundations supported what would be regarded now as pseudo-science, projects that were even questionable in their own time and were heavily freighted with the specter of eugenics. Before and during World War I, the human engineering effort seemed both clear and eminently realizable. The ideas behind social hygiene and eugenics were relatively simple: fix dysfunction through the regulation of sexual behavior and society will benefit. In the 1920s, this simple prescription became more difficult to implement. The impetus behind the human engineering effort remained salient, however, even as the original projects in social hygiene and eugenics were adjusted to accommodate changes in prevailing scientific opinion. Investigating and controlling behavior, particularly sexual behavior, remained the goal, but the tactics changed to incorporate the research projects that were introduced by scientists who had gained the trust of the foundation officers, largely through the National Research Council. James R. Angell, Robert M. Yerkes, Raymond Pearl, Frank R. Lillie: these scientists were friends and advisors to foundation officers who interpreted the human engineering effort in terms that suited their research interests. Foundation officers of the 1920s, few of whom had any scientific training, relied on these scientific entrepreneurs to point them in the right direction to fulfill the goals of human betterment. The policies of the 1920s were a jumble of popular
assumptions about sexuality and evolution, mixed with ideas about human behavior and biology that were gleaned from the scientific entrepreneurs who successfully applied for grants.

The Carnegie foundations remained stuck in the policy morass of the 1920s, but the Rockefellers escaped in the early 1930s. The reorganization of the Rockefeller philanthropic boards and the decision to have trained scientists take over as directors of divisions that were explicitly devoted to the promotion of select research policies was a major turning point. The other major turning point was the arrival of Warren Weaver. Weaver was a determined and idealistic foundation professional who brought previously blurry ideals and trends into sharp relief. During his tenure as director of the Division of Natural Sciences, Weaver furthered all of the foundation objectives that had been established in the preceding decades. He maintained Rockefeller Foundation relationships with leading scientific entrepreneurs, avidly pursued the goal of "cooperation in research," and clarified the human engineering effort into a set of policy initiatives.

Although the implementation of his "vital processes" program with its "new science of man" objectives was difficult, Weaver was tenacious. His efforts yielded promising results in several universities that received major grants, including University of Chicago and Stanford University. The goals of cooperation in research and the development of a new science of man might not have been achieved, but that does not diminish the crucial part that Weaver played in helping biology programs at these universities thrive during the Great Depression. Still, without a doubt, the greatest triumph for Weaver was at Caltech, where the real approached the ideal.
There are numerous justifiable criticisms of the choices that foundation officers made in the interwar period, especially regarding the neglect of universities and researchers that were outside the “top tier” in the academic hierarchy, but a counterfactual analysis of what was not accomplished seems futile. What were the tangible effects of the work that the foundation officers did? For all of its potentially iniquitous implications, the foundation human engineering effort made a lasting impact on the production of knowledge in the United States, both through “successes” and “failures.” The National Research Council Committee on Research in Problems of Sex led to the Kinsey Report, a project that had a negligible effect on science but a sizeable impact on American culture and society. Yerkes’s primate laboratories continue to prosper today, albeit in a much changed form, and his obsession with chimpanzees inspired future generations of primatologists. Primatology and the study of animal behavior remain highly controversial, but they are very much alive. Even Charles Davenport’s Station for Experimental Evolution was transformed into the prestigious Department of Genetics at the Carnegie Institution of Washington, while generations of biologists have built on the work done at laboratories supported by foundation dollars.

Developments in the biological sciences in particular show the real work that faith in the ability of research to improve mankind did and continues to do. We only need to look at the human genome project or stem cell research to see cogent examples of how investment into biology research—public or private—is still premised on the idea that unlocking the secrets of the human body will improve lives. This faith in the awesome transformative power of scientific research was inaugurated in the late nineteenth century, formalized in the early twentieth century, and sustained and strengthened by
philanthropic foundations in the 1920s and 1930s. It remains a principal factor in the support that university scientists continue to rely upon today for their research.
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### Appendix I:
Research Expenditures by Carnegie and Rockefeller Philanthropies, 1902–1941

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<th>Organization</th>
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<th>Amount (in thousands)</th>
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<td>Other research expenditures</td>
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Appendix II:
Officers and Trustees of the Rockefeller Foundation (RF)
1913–1941

Presidents
John D. Rockefeller, Jr. 1913–1917
George E. Vincent 1917–1929
Max Mason 1929–1936
Raymond B. Fosdick 1936–
*Presidents of the Rockefeller Foundation were members of the GEB Board of Trustees.

Vice Presidents
Roger S. Greene 1918–1929
Selskar M. Gunn 1927–1941
Edwin R. Embree 1927
Thomas B. Appleget 1929–

Secretaries
Jerome D. Greene 1913–1916 RIMR and GEB trustee
Edwin R. Embree 1917–1924
Norma S. Thompson 1925–

Directors of the Division of Medical Sciences
Richard M. Pearce, M. D. 1919–1930
Alan Gregg, M. D. 1930–

Directors of the Division of Natural Sciences
Max Mason 1928–1929
Hermann A. Spoehr 1930–1931
Warren Weaver 1932–

Directors of the Division of Social Sciences
Edmund E. Day 1928–1937
Sydnor H. Walker (acting) 1937–1938
Joseph H. Willits 1939–

Directors of the Division of the Humanities
Edward Capps 1929–1930
David H. Stevens 1932–

Chairmen of the Board of Trustees
John D. Rockefeller, Jr. 1917–1939
Walter W. Stewart 1939– GEB trustee

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<th>Years</th>
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<tr>
<td>Winthrop W. Aldrich</td>
<td>1935–</td>
<td>GEB trustee</td>
</tr>
<tr>
<td>James R. Angell</td>
<td>1928–1936</td>
<td>President of Yale; GEB trustee</td>
</tr>
<tr>
<td>Trevor Arnett</td>
<td>1928–1936</td>
<td>RIMR and GEB trustee</td>
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<tr>
<td>Wallace Buttrick</td>
<td>1917–1926</td>
<td>President of the GEB; Fmr. GEB Secretary</td>
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<td>John W. Davis</td>
<td>1922–1939</td>
<td>GEB trustee</td>
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<td>Harold W. Dodds</td>
<td>1937–</td>
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<td>John Foster Dulles</td>
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<td>David L. Edsall</td>
<td>1927–1936</td>
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<td>Charles W. Eliot</td>
<td>1914–1917</td>
<td>GEB trustee; Fmr. President of Harvard</td>
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<td>Simon Flexner</td>
<td>1913–1930</td>
<td>Director of the RIMR</td>
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<td>Henry E. Fosdick</td>
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<td>President; GEB trustee</td>
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<td>Douglas Freeman</td>
<td>1937–</td>
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<td>Herbert Gasser</td>
<td>1937–</td>
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<td>Frederick T. Gates</td>
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<td>Walter S. Gifford</td>
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<tr>
<td>Jerome Greene</td>
<td>1913–1917</td>
<td>Secretary and Acting Director of RF</td>
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<td></td>
<td>1928–1939</td>
<td>GEB and RIMR trustee</td>
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<td>Barton A. Hepburn</td>
<td>1914–1922</td>
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<tr>
<td>Ernest M. Hopkins</td>
<td>1928–</td>
<td>GEB Chairman and trustee</td>
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<td>Charles P. Howland</td>
<td>1928–1932</td>
<td>GEB trustee</td>
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<td>Charles E. Hughes</td>
<td>1917–1928</td>
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<td>Harry Pratt Judson</td>
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<td>GEB trustee; President, Univ. of Chicago</td>
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<td>Vernon L. Kellogg</td>
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<td>Max Mason</td>
<td>1930–1936</td>
<td>GEB trustee; President of RF</td>
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<td>Thomas I. Parkinson</td>
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<td>John D. Rockefeller</td>
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<td>Founder</td>
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<td>1913–1940</td>
<td>Chair, President; GEB and RIMR trustee</td>
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<td>John D. Rockefeller, III</td>
<td>1931–</td>
<td>GEB and RIMR trustee</td>
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<td>Wickliffe Rose</td>
<td>1913–1928</td>
<td>GEB trustee; President of the GEB</td>
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<tr>
<td>Julius Rosenwald</td>
<td>1913–1931</td>
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<tr>
<td>Martin A. Ryerson</td>
<td>1916–1928</td>
<td>CIW trustee</td>
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<td>Walter W. Stewart</td>
<td>1931–</td>
<td>Chairman; GEB trustee</td>
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<td>Anson Phelps Stokes</td>
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<td>1916–1931</td>
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<td>Augustus Trowbridge</td>
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<td>William Allen White</td>
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Appendix III:
Officers and Trustees of the General Education Board (GEB)
1902–1941

Presidents
Wallace Buttrick 1917–1923*
Wickliffe Rose 1923–1928
Trevor Arnett 1928–1936
Raymond B. Fosdick 1936–
*Rose was the “Secretary and Chief Executive Officer from 1902–1917, when his title was changed to “President.” All GEB presidents were on the RF Board of Trustees.

Vice Presidents
David H. Stevens 1930–1938 RF Director of Humanities

Secretaries
Abraham Flexner 1917–1925 Assistant Secretary, 1913–1917
Trevor Arnett 1920–1924 RF and RIMR trustee
William W. Brierley 1925–

Chairmen of the Board
William H. Baldwin, Jr. 1902–1905
Robert C. Ogden 1905–1907
Frederick T. Gates 1907–1917 RF and RIMR trustee
Wallace Buttrick 1923–1926 Fmr. GEB President; RF trustee
Raymond B. Fosdick 1932–1936 RF trustee; RF and GEB President beg. 1936
John D. Rockefeller, Jr. 1936–1939 Chairman of RF Board; RIMR trustee
Ernest M. Hopkins 1939– RF trustee

Trustees
Edwin A. Alderman 1906–1928
Winthrop W. Aldrich 1935– RF trustee
E. Benjamin Andrews 1905–1912
James R. Angell 1922–1934 RF trustee; President of Yale
Trevor Arnett 1920–1936 RF and RIMR trustee
William H. Baldwin, Jr. 1902–1905 Chairman
Wallace Buttrick 1902–1926 President and Chairman; RF trustee
Andrew Carnegie 1908–1918 Founder of CCNY and CIW
Henry Woodburn Chase 1930–1936
J. L. M. Curry 1902–1903
John W. Davis 1935–1938 RF trustee
James H. Dillard 1918–1929
Harold W. Dodds 1937– RF trustee
Charles W. Eliot 1908–1917 RF trustee; President of Harvard

782 General Education Board, Review and Final Report, 1902–1964 (New York: General Education Board, 1964), 93–97. If their tenure went beyond 1941, the second date is left blank.
<table>
<thead>
<tr>
<th>Name</th>
<th>Dates</th>
<th>Role</th>
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<tbody>
<tr>
<td>Abraham Flexner</td>
<td>1914–1928</td>
<td>Secretary</td>
</tr>
<tr>
<td>Raymond B. Fosdick</td>
<td>1922–</td>
<td>President and Chairman</td>
</tr>
<tr>
<td>Douglas R. Freeman</td>
<td>1937–</td>
<td>RF trustee</td>
</tr>
<tr>
<td>Hollis B. Frissell</td>
<td>1906–1917</td>
<td></td>
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<tr>
<td>Frederick T. Gates</td>
<td>1902–1928</td>
<td>Chairman; RF and RIMR trustee</td>
</tr>
<tr>
<td>Walter S. Gifford</td>
<td>1935–</td>
<td>RF trustee</td>
</tr>
<tr>
<td>Daniel Coit Gilman</td>
<td>1902–1908</td>
<td>President of CIW</td>
</tr>
<tr>
<td>Jerome D. Greene</td>
<td>1912–1939</td>
<td>RF Secretary and trustee; RIMR trustee</td>
</tr>
<tr>
<td>Hugh H. Hanna</td>
<td>1905–1912</td>
<td></td>
</tr>
<tr>
<td>Ernest M. Hopkins</td>
<td>1930–</td>
<td>RF trustee</td>
</tr>
<tr>
<td>Charles P. Howland</td>
<td>1919–1932</td>
<td>RF trustee</td>
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<tr>
<td>Morris K. Jesup</td>
<td>1902–1908</td>
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<tr>
<td>Harry Pratt Judson</td>
<td>1906–1927</td>
<td>RF trustee; President, University of Chicago</td>
</tr>
<tr>
<td>Edgar L. Marston</td>
<td>1909–1918</td>
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<tr>
<td>Max Mason</td>
<td>1930–1936</td>
<td>RF President and trustee</td>
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<tr>
<td>Edwin Mims</td>
<td>1931–1936</td>
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<td>Starr J. Murphy</td>
<td>1904–1921</td>
<td>RF and RIMR trustee</td>
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<tr>
<td>Robert C. Ogden</td>
<td>1902–1913</td>
<td>Chairman</td>
</tr>
<tr>
<td>Walter Hines Page</td>
<td>1902–1918</td>
<td></td>
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<tr>
<td>Thomas I. Parkinson</td>
<td>1935–</td>
<td>RF trustee</td>
</tr>
<tr>
<td>George Foster Peabody</td>
<td>1902–1912</td>
<td>Founder of Peabody Fund</td>
</tr>
<tr>
<td>John D. Rockefeller, Jr.</td>
<td>1903–1939</td>
<td>Chair; RF President, Chair; RIMR trustee</td>
</tr>
<tr>
<td>John D. Rockefeller, III</td>
<td>1932–</td>
<td>RF and RIMR trustee</td>
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<tr>
<td>Wickliffe Rose</td>
<td>1910–1928</td>
<td>President; RF trustee</td>
</tr>
<tr>
<td>Albert Shaw</td>
<td>1902–1929</td>
<td></td>
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<tr>
<td>Frank E. Spaulding</td>
<td>1918–1920</td>
<td></td>
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<tr>
<td>Walter W. Stewart</td>
<td>1932–</td>
<td>RF trustee</td>
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<tr>
<td>Anson Phelps Stokes</td>
<td>1912–1932</td>
<td>RF trustee</td>
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<tr>
<td>Harold H. Swift</td>
<td>1931–</td>
<td>RF trustee</td>
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<tr>
<td>Augustus Trowbridge</td>
<td>1930–1934</td>
<td>RF trustee</td>
</tr>
<tr>
<td>George E. Vincent</td>
<td>1914–1929</td>
<td>RF President and trustee</td>
</tr>
<tr>
<td>George H. Whipple</td>
<td>1936–</td>
<td>RF and RIMR trustee</td>
</tr>
<tr>
<td>Ray Lyman Wilbur</td>
<td>1931–1940</td>
<td>RF trustee; President of Stanford</td>
</tr>
<tr>
<td>Arthur Woods</td>
<td>1930–1934</td>
<td>RF trustee</td>
</tr>
<tr>
<td>Owen D. Young</td>
<td>1925–1939</td>
<td>RF trustee</td>
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</table>
Appendix IV:  
Presidents and Trustees of the Carnegie Institution of Washington (CIW)  
1902–1941

<table>
<thead>
<tr>
<th>Presidents</th>
<th>Years</th>
<th>Note</th>
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<tbody>
<tr>
<td>Daniel Coit Gilman</td>
<td>1902–1904</td>
<td>Fmr. Johns Hopkins President; GEB trustee</td>
</tr>
<tr>
<td>Robert Simpson Woodward</td>
<td>1904–1920</td>
<td></td>
</tr>
<tr>
<td>John C. Merriam</td>
<td>1921–1938</td>
<td></td>
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<tr>
<td>Vannevar Bush</td>
<td>1938–</td>
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<th>Trustees</th>
<th>Years</th>
<th>Note</th>
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<tr>
<td>Alexander Agassiz</td>
<td>1904–1905</td>
<td></td>
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<tr>
<td>George J. Baldwin</td>
<td>1925–1927</td>
<td></td>
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<tr>
<td>John S. Billings</td>
<td>1902–1913*</td>
<td></td>
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<tr>
<td>Robert S. Brookings</td>
<td>1910–1929</td>
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<tr>
<td>John L. Cadwalader</td>
<td>1903–1914</td>
<td></td>
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<tr>
<td>William W. Campbell</td>
<td>1929–1938</td>
<td></td>
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<tr>
<td>John J. Carty</td>
<td>1916–1932</td>
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<tr>
<td>Whitefoord R. Cole</td>
<td>1925–1934</td>
<td></td>
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<tr>
<td>Frederic A. Delano</td>
<td>1927–</td>
<td></td>
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<tr>
<td>Cleveland H. Dodge</td>
<td>1903–1923</td>
<td></td>
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<tr>
<td>Charles P. Fenner</td>
<td>1914–1924</td>
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<tr>
<td>Homer L. Ferguson</td>
<td>1927–</td>
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<tr>
<td>Simon Flexner</td>
<td>1910–1914</td>
<td>Director and trustee of the RIMR</td>
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<tr>
<td>W. Cameron Forbes</td>
<td>1920–</td>
<td></td>
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<tr>
<td>Chairman</td>
<td>1937–</td>
<td></td>
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<tr>
<td>William N. Frew</td>
<td>1902–1915*</td>
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<td>Lyman J. Gage</td>
<td>1902–1912*</td>
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<td>Cass Gilbert</td>
<td>1924–1934</td>
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<td>Frederick H. Gillett</td>
<td>1924–1935</td>
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<tr>
<td>Daniel C. Gilman</td>
<td>1902–1908*</td>
<td>President of CIW; GEB trustee</td>
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<td>John Hay</td>
<td>1902–1905*</td>
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<td>Myron T. Herrick</td>
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<td>Abram S. Hewitt</td>
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<td>Henry L. Higginson</td>
<td>1902–1919*</td>
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<td>Ethan A. Hitchcock</td>
<td>1902–1909*</td>
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<tr>
<td>Herbert Hoover</td>
<td>1920–</td>
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<td>William Wirt Howe</td>
<td>1903–1909</td>
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<tr>
<td>Charles L. Hutchinson</td>
<td>1902–1904*</td>
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<tr>
<td>Frank B. Jewett</td>
<td>1933–</td>
<td></td>
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<tr>
<td>Samuel P. Langley</td>
<td>1904–1906</td>
<td></td>
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<tr>
<td>Charles A. Lindbergh</td>
<td>1934–1939</td>
<td></td>
</tr>
<tr>
<td>William Lindsay</td>
<td>1902–1909*</td>
<td></td>
</tr>
</tbody>
</table>

* Carnegie Institution of Washington, *Annual Reports*, 1902–1941. If their tenure went beyond 1941, the second date is left blank.
Henry Cabot Lodge 1914–1924
Seth Low 1902–1916* President of Columbia University
Wayne MacVeigh 1902–1907*
Andrew J. Mellon 1924–1937
Darius O. Mills 1902–1909*
S. Weir Mitchell 1902–1914*
Andrew J. Montague 1907–1935
William W. Morrow 1902–1929*
William Church Osborn 1927–1934
James Parmelee 1917–1931
William Barclay Parsons 1907–1932
Stewart Paton 1915–
George W. Pepper 1914–1919
John J. Pershing 1929–
Henry S. Pritchett 1906–1936 President of Carnegie Foundation
Elihu Root 1902–1937* Chairman of the Board
Martin A. Ryerson 1908–1928 RF trustee
Theobald Smith 1914–1934
William Benson Storey 1924–1939
William H. Taft 1906–1915
Charles D. Walcott 1902–1927*
Frederic C. Walcott 1931–
Henry P. Walcott 1910–1924
William H. Welch 1906–1934 RIMR trustee
Henry White 1913–1927
George W. Wickersham 1909–1936
Robert S. Woodward 1905–1924 President of the CIW
Carroll D. Wright 1902–1908*
*Original trustee appointed by Andrew Carnegie