Published in final edited form as:

Bull Hist Med. 2012 ; 86(1): 94–121. doi:10.1353/bhm.2012.0017.

Methods and Management: NIH Administrators, Federal Oversight, and the Framingham Heart Study

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Summary

This article explores the 1965 controversy over the Framingham Heart Study in the midst of growing oversight into the management of science at the National Institutes of Health (NIH). It describes how, beginning in the early 1960s, federal overseers demanded that NIH administrators adopt particular management styles in administering programs and how these growing pressures led administrators to favor investigative pursuits that allowed for easy prospective accounting of program payoffs, especially those based on experimental methods designed to examine discrete interventions or outcomes of interest. In light of this changing managerial culture within the NIH, the Framingham study and other population laboratories—with their bases in observation and in open-ended study designs—became harder for NIH administrators to justify and defend.

Keywords

National Institutes of Health; science administration; federal oversight; public administration; Framingham Heart Study; biostatistics; research methods; Wooldridge Review

The Framingham Heart Study has been among the most scientifically productive and influential studies of the post–World War II period.¹ It has helped investigators identify the major causes of heart disease and has facilitated the development of the risk factor concept along with essential methods and techniques for evaluating multiple variables in population-based studies of chronic diseases.² Through continuing to provide an effective basis for the primary prevention of chronic disease, the study has allowed epidemiology to remain a premier health science into the twenty-first century.³

The history of the Framingham study is not, however, a grand story of the progress of medical science. National Institutes of Health (NIH) administrators surprised its investigators in 1965 by attempting to discontinue the study during its most scientifically productive years. That year, James Shannon, the director of the NIH, began exploring the possibility of ending the Framingham study five years prior to its promised twenty-year funding period—a line of inquiry that presaged what contemporaries would call “the Pause” in funding for further field studies that occurred between 1968 and 1972.⁴ During 1965, Shannon requested the review of several large-scale intramural endeavors of the NIH to discern if they demonstrated adequate likelihoods of program payoff. Upon receiving this review, he concluded that the Framingham study had met its original objectives and that, due to the degree of ambiguity surrounding its future findings, it did not merit continued funding. As a direct result of Shannon’s 1965 conclusions, NIH administrators temporarily eliminated the study as a directly funded intramural program of the National Heart Institute in 1970—an action that is as poorly understood by historians now as it was by investigators then.

Shannon’s 1965 proposal met with fierce opposition from the proponents of the Framingham study who adamantly defended its contributions and potential for future scientific returns to the surgeon general, the secretary of the Department of Health,

Education, and Welfare, and even the president in their efforts to secure continued funding.⁵ For them, Shannon's disregard of their entreaties to continue the study undermined the NIH's proclaimed commitment to keep science in the hands of scientists and away from intrusive external oversight and direction. Ending the study by administrative fiat also challenged the principles for vetting the scientific merit of projects that had been in place at the NIH since the end of the Second World War. As the head of the Heart Institute warned, "[A]n administrative decision to close the Framingham program would be tantamount to the negation of confidence in the advice of a large body of scientific opinion and would also negate the traditional responsibility for each institute to direct its own scientific programs."⁶ Moreover, the ambiguous administrative rationale behind attempts to end the study along with "the Pause" in funding for field trials that followed left epidemiologists questioning whether NIH administrators fully recognized and appreciated the value of their scientific contributions during this period. The "apparent indifferent interest in the efforts and accomplishments of Framingham" on the part of NIH administrators prompted Thomas Dawber, the study's principal investigator (PI), to leave the Heart Institute's intramural program and other leading heart disease epidemiologists to undertake an international crusade to garner support for heart disease field trials.⁷

As we now know, the efforts of proponents were ultimately effective and the Framingham study continues to this day. However, the 1965 controversy provides an opportunity to better understand the administration of science within the NIH during the 1960s. What contributed to the administrative decision to end the Framingham study? Did field studies simply "lack glamor," "strain our resources and patience," and "protract the moment of truth to excruciating limits," as one Heart Institute director proposed?⁸ Or, did Shannon's conclusion rest on a matter of "management style, not scientific substance," as a later Framingham study investigator has suggested?⁹

This article provides insight into the 1965 controversy over the Framingham study. It describes how federal overseers demanded NIH administrators to adopt particular management styles in administering programs and how these growing pressures led Shannon to favor investigative pursuits that allowed for easy prospective accounting of program payoff, especially those based on experimental methods designed to examine discrete interventions or outcomes of interest. In light of this changing managerial culture within the NIH, the Framingham study and other population laboratories—with their bases in observation and in open-ended study designs—became harder for NIH administrators to justify and defend. In exploring this controversial episode in the history of the Framingham study, this article encourages a critical examination of how various scientific practices lend themselves to management styles and of how federal demands for accountability have shaped the biomedical and health sciences of the late twentieth century.

The Framingham Heart Study, 1965

In February 1965, Thomas Dawber, the lead investigator on the Framingham Heart Study, prepared and submitted a "Report of Progress in the Framingham Study" to William Zukel, the interim director of the Heart Institute.¹⁰ Zukel had asked Dawber to do so at the request of James Shannon, the director of the NIH, who wanted to be briefed on all endeavors taking place through each institute's Collaborative Studies Program "from the standpoint of the mechanics by which they have been developed, the review groups which gave advice, [and the] manner in which approval was given."¹¹ Shannon had correctly anticipated that the NIH Collaborative Studies Programs (CSPs) would garner close scrutiny from an upcoming Office of Science and Technology review of NIH spending.¹² By the 1960s, the budget for CSPs had grown significantly, often exceeding funding for investigator-initiated research conducted through the NIH's intramural program. Both Shannon and Zukel expected the

Framingham study, as a keystone study of the Heart Institute's CSP, to receive especial attention.

According to Dawber, the Framingham study had been designed as an observational study of a natural population to elucidate the patterns, frequency, and possible etiology of heart disease. Beginning officially at the Heart Institute in 1949, the study enrolled 5,209 disease-free individuals who had enrolled between the ages of approximately thirty and fifty-nine and followed them through time to provide real-time (as opposed to retrospective) insights into the natural history of heart disease. The ability to follow a cohort of individuals free of disease through time was what made the study so promising to Dawber and his colleagues. "Since the Framingham Study is capable of following all cases of coronary heart disease developing in the population including those dying suddenly ... as well as those too 'mild' to appear in hospital statistics," the Framingham study gave investigators data that were "not obtainable from any other source," they wrote.¹³ These investigators were enthusiastic about Framingham largely because of the difficulties in obtaining funding to conduct long-term prospective studies of chronic disease.¹⁴ "A living study in which a group of people would be followed throughout life would be ideal," Harold Dorn, a leader in health surveys, had earlier explained with respect to cancer studies. "However," he added frankly, "such a project is a very large undertaking."¹⁵ Investigators valued the Framingham study, which had been initiated eight years after Dorn's comments, for meeting this ideal.

The Framingham study would not have been possible without the postwar expansion of funding for health research. "The expense of such a study and the necessity of guaranteeing its operation for a span of many years puts it beyond the capabilities of the individual investigator," Dawber explained.¹⁶ With guarantees of long-term financial support from the Bureau of State Services of the U.S. Public Health Service and, later, the Heart Institute, program directors were able to design a study that, in effect, covered the cost of conducting biennial examinations on approximately six thousand individuals for twenty years. By the time of Dawber's report, the study's then fifteen-year time frame had allowed investigators to identify 441 new cases of coronary heart disease, 60 new cases of cerebral vascular disease, and 132 new cases of hypertension that had developed in the Framingham population. From the data collected on these cases, Dawber and his colleagues found that men who smoked, were middle aged and obese, and had elevated serum cholesterol and high blood pressure had a higher risk of developing coronary heart disease. Moreover, they found that individuals who fit this profile had a thirtyfold higher chance of developing heart disease than those who did not.¹⁷

Because very little was understood about the natural history, etiology, and diagnosis of heart disease, Dawber and other investigators especially valued the Framingham study's function as what they called a "population laboratory." Applying lessons from Joseph Mountin and Harold Dorn, Public Health Service officials who were leaders in conducting health surveys and demonstration programs, Framingham investigators regarded the development of heart disease screening procedures and the collection of accurate statistics about incidence and prevalence as important contributions of the study itself.¹⁸ As Dawber explained, the overall undertaking was intended to "study the efficiency of various diagnostic procedures in finding heart disease or as indicators of the subsequent development of heart disease" as well as the provision of "data on prevalence and incidence of cardiovascular disease."¹⁹

As one of the few prospective population-based investigations of heart disease, the Framingham study had become the de facto site for learning *how to study* heart disease (and chronic diseases more generally) in populations. The study occupied "a rather unusual position," Dawber wrote in his report, to "investigate the possibility and validity of various simpler methods of epidemiologic study using the complete prospective study as a standard

of comparison.”²⁰ Biometry experts including Jerome Cornfield and Tavia Gordon of the Heart Institute’s Biometrics Research Branch used the Framingham study in this capacity.²¹ For example, Cornfield and colleagues developed and tested the use of multivariate analysis, which dramatically reduced the number of new cases of heart disease needed for evaluating multiple variables and formed the methodological basis of the risk factor approach.²²

As a population laboratory for observational research, the Framingham project designed its inquiries to be open-ended about its potential findings and the length of time needed for meaningful insights to emerge. The Framingham program was not designed to elucidate a narrowly defined set of variables as was the case with experimental studies. Moreover, its investigators cast a wide net with respect to population size, “which might,” they hoped, “be expected to provide the data necessary to test these hypotheses.”²³ They sought a two-thirds sample of the town’s population following “considerable thought” and “deliberation.”²⁴ Similarly, its twenty-year time horizon was not based on calculations of the likelihood of establishing the role of specific variables in the development of heart disease. Rather, twenty years seemed an adequate length of time to begin accumulating cases with, at the very least, the early symptoms of coronary heart disease for the purpose of describing the disease’s natural history and its incidence and prevalence rates. In turn, the early study designers selected individuals who were at least thirty years old at the start of the study because they assumed that, given a twenty-year period of observation, the prevalence of coronary heart disease among this group would be fairly high.

The Framingham investigators intended to add areas of study to the investigation as they saw fit; they believed that thoroughly collected and consistently arrayed data from such a stable populations ought to be used for more than just the elucidation of the role of blood pressure and serum cholesterol on cardiovascular disease. For them, not tapping this resource to its fullest extent would be to “waste a tremendous investment already made.”²⁵ Accordingly, Framingham investigators had decided to expand the scope of their study in 1964 to follow the development of stroke and other cerebrovascular abnormalities. Dawber wrote that “an attempt has been made to incorporate as many possible other disease entities into the Framingham program,” recognizing that the cost of this program “has been relatively high and that to obtain population laboratories of this type is a very time consuming, laborious, and expensive procedure.”²⁶

The methodology for conducting epidemiological studies with better estimates of the population size and length of follow-up was not fully developed at the time of the Framingham study’s onset in 1949; the bio-statistical enhancements for which the Framingham study is now known were not developed until the mid- to late-1950s, and multivariate analysis was not applied until the early 1960s.²⁷ Perhaps more importantly, Dawber and his colleagues felt that carefully following approximately six thousand individuals for a period of at least twenty years through detailed interviews and clinical exams would give them a fairly comprehensive picture of the patterns and manifestations of heart disease. For them, tracking the natural history of heart disease—again, an illness with multiple causes and, at the time, an ambiguous onset—was a descriptive endeavor that required lengthy and careful observation as well as an openness to nuance and a willingness to follow leads as they emerged. Throughout his report, Dawber reaffirmed this logic through making connections between scientific clarity and the length of observation. “Evidence accumulated at Framingham over more than a decade of observation of this general population sample has given an increasingly clear picture of the natural history of this disease,” he wrote in one instance.²⁸ The investigators also implied that in well-conducted observational studies, the time horizon should be established as the study progressed and not from the outset of the period of observation. As Dawber recommended in his progress report, the population should be followed “indefinitely ... as long as follow-up

remains satisfactory and useful information is being obtained.”²⁹ In other words, whether to continue, expand, or curtail a population laboratory depended on how consistently and accurately data had been collected and how useful they were in conducting scientific evaluations. As was characteristic of scientific field stations in general, flexibility in study design and an acceptance of the inherent ambiguity about what would be found through observation were central scientific tenets of the Framingham study circa 1965.³⁰

So critical was its function as a population laboratory and as a testing ground for technique and method that Heart Institute administrators positioned the Framingham study as a key initiative of their CSP. In 1956, Shannon gave NIH institute directors the option of establishing CSPs within each of their intramural programs.³¹ Unlike smaller, investigator-initiated projects that characterized the bulk of research then taking place through the NIH intramural program, the CSPs were intended to secure funding for projects that required steady, long-term support and centralized coordination and for those projects that institute administrators and national advisory council members considered critical to a research area but unlikely to be carried out independently by investigators in the extramural community. Recognizing the value of larger collaborative or coordinated endeavors (similar to his malaria research during the war), Shannon authorized the use of a line item in the federal budgets of each institute for collaborative studies.³² As Shannon described, these CSPs served as “ways in which the NIH engages in the active programming of scientific research”—that is, opportunities for institute directors to undertake projects considered essential to critical research areas.³³ The amount of funding distributed to any institute’s CSP and the type of work encompassed by these programs were left to the discretion of NIH institute directors and the members of their national advisory councils. Once they were agreed upon, Shannon would back CSP funding requests at congressional appropriations hearings.

In the case of the Heart Institute, institute administrators and National Advisory Heart Council (NAHC) members decided that studies in “epidemiology,” “geographical disease studies,” “biometrics research,” and “clinical trials” should take place through funding from the Heart Institute’s CSP.³⁴ These administrators and advisors found such studies critically important but too difficult for extramural investigators to conduct without adequate funding and coordination.³⁵ Furthermore, the basic methodological approach for conducting population studies on chronic disease was far from established—a situation that made extramural investigators less likely to undertake such endeavors. If anything, the developments needed in these areas “emphasize[d] the importance of having a highly competent central group to provide consultation and assistance to [extramural] grantees in carrying out such studies.”³⁶ The director of the Heart Institute’s CSP wrote how he hoped the personnel from these programs would provide “direct participation in certain studies, and also serve as a resource for advice and consultation to [extramural] investigators involved in such projects.”³⁷

As a population laboratory, the Framingham study served well the broader functions and services proposed for the Heart Institute’s CSP. Personnel working on the Framingham study took the lead in organizing precedent-setting national and international meetings on the study of chronic diseases. Investigators used Framingham data as a long-standing resource with which to explore hypotheses, develop methods, and acquire basic data such as the incidence and prevalence of particular diseases and risk factors. With these incidence and prevalence data (then considered the most accurate available on heart disease), they could assess whether other geographical areas and populations were demonstrating unusual, and hence scientifically noteworthy, patterns of disease formation.³⁸ In turn, biostatisticians at the Heart Institute’s CSP would assist investigators in designing their studies and interpreting their results. A Heart Institute CSP report listed twenty-two published methodological contributions from personnel of the Biometrics Research Branch. In

addition, staff of the Biometrics Research Branch had served as consultants on over fifty extramural research projects in chronic disease epidemiology.³⁹

In 1965, Framingham investigators considered these applied- and service-oriented functions as essential deliverables of the study itself. Alongside publications and presented papers, they listed “physician and medical student training,” “extended consultations to other studies,” “other consultations to investigators,” “health education,” and “service to outside investigators” among the reportable products of the study.⁴⁰ Dawber and his colleagues oriented several of their publications toward the more general aspects of conducting epidemiological studies of chronic disease. They devoted one article to describing their personal experiences in carrying out such an undertaking, classifying the problems encountered as professional, administrative, organizational, and technical.⁴¹ The Framingham study also became the site of an informal training rotation for Public Health Service physicians with interests in control and demonstration programs and in epidemiological research.⁴² As Dawber reported, “[M]edical personnel have been deliberately assigned to the Framingham study both by the Heart Institute and by the Heart Disease Control Program to receive training in cardiovascular epidemiology and to obtain additional instruction at the Harvard School of Public Health” with these physicians going on to “excellent careers in teaching, research, and medical practice.”⁴³ Finally, Dawber and other Framingham investigators were in demand around the country and internationally to consult on the design and development of chronic disease studies. It was through these service-oriented functions of the Framingham study that it came to influence and direct the overall development of population-based studies of chronic disease.⁴⁴

In sum, investigators valued the Framingham study in 1965 for its role as a population laboratory. Like laboratories in general, it was not established to undertake one experimental study but to serve investigators as a longstanding resource in which they could develop techniques, train investigators, and explore hypotheses. As Dawber’s “Report of Progress” demonstrated, investigators of several varieties used this institutional resource to evaluate methods, identify and test the strength of various risk factors, and refine diagnostic techniques, among many other tasks. In accordance with the broader mandate of the Heart Institute’s CSP, Framingham investigators provided applied insights to the research community on how to conduct these types of studies. What made these objectives possible, in turn, was the study’s nature as a long-term observational study and population laboratory devoted to understanding and improving the study of chronic disease in natural populations.

“In the Wake of Wooldridge ...”

Beginning in the late 1950s,⁴⁵ the NIH received increasing calls to demonstrate the efficiency of their operations and the accountability of their grantees.⁴⁶ Congressman Lawrence H. Fountain, a fiscally conservative Democrat from North Carolina who chaired a subcommittee of the House Committee on Government Operations, began reviewing the NIH in the late 1950s and almost annually thereafter.⁴⁷ These reviews culminated in 1965 with the president’s Office of Science and Technology’s decision to “study how NIH spends its approximately billion dollar budget, to judge whether the American people are getting their money’s worth from the expenditure, and to recommend any changes in organization or procedure that would ... increase the effectiveness of the program.”⁴⁸ The Wooldridge Review, as the Office of Science and Technology’s review would come to be called, was among the most thorough investigations of the NIH in its history. In addition to six of the agency’s collaborative programs, its members visited the labs of 52 intramural scientists and evaluated the work of over 600 NIH scientists and 150 administrators through reviewing 240 traditional grants, 125 unsuccessful applications, 75 research career grant awards, 30 training grants, 10 general research support grants, 6 special research centers, 5 general

clinical research centers, 4 primate centers, 3 international centers, and 10 program projects.⁴⁹

The Fountain and Wooldridge Reviews were characterized by their focus on the accountability of scientists funded through the NIH and on the efficient design of programs taking place at the NIH. Fountain, for instance, called on the agency to “formulate grant principles which [would] clarify the moral obligations of the scientist as a trustee of public funds.”⁵⁰ In the case of the Wooldridge committee, accountability during this period became implemented through a growing preoccupation with establishing the likeliness of program payoff.⁵¹ For the members of the Wooldridge committee, a majority of whom had backgrounds in the physical and engineering sciences (“to bring the highest degree of objectivity” to the review, as Shannon dryly observed), such accountability could be ensured only through the more rigorously quantitative evaluation of studies achieved through the “increased participation of physical scientists and mathematicians in all aspects of NIH operations.”⁵² Wooldridge committee members also demanded that NIH administrators assess all projects for program payoff *prior to* the commitment of funding, especially for projects of larger scale, longer length, and higher cost—like those taking place through the NIH’s CSPs. “An extraordinary justification should be required ... for the support of any collaborative program involving the expenditure of more than a very few million dollars a year,” read the report.⁵³ According to the members of the Wooldridge committee, the NIH was more obligated to implement management guarantees for expensive projects since the risk of financial mismanagement and waste was higher. “There should be a strongly negative weight factor applied to the size of the proposed project,” they wrote. “With increasing size, there should be a rapidly increasing requirement on the estimated probability of pay-off.”⁵⁴

The impact of these reviews on the managerial culture of the NIH was significant. Prior to the late 1950s, Public Health Service administrators like Joseph Mountin had focused their managerial attention on agency organization and the distribution of personnel and resources in accordance with the needs of the public’s health.⁵⁵ The oversight introduced by Fountain, on the other hand, involved an orientation around the activities of scientists themselves.⁵⁶ From Fountain’s point of view, meeting the moral obligations of public administration involved implementing safeguards to ensure accountability and effective oversight or, as NIH administrators understood it, “the establishment of highly specific and detailed procedural and accounting requirements by Federal agencies, together with constant and close ‘policing’ of grantees.”⁵⁷

As noted by critical commentators, these calls catalyzed a managerial sea change within the NIH during the 1960s.⁵⁸ For example, Fountain saw to it that NIH grantees could no longer use their funding for travel or toward research not delineated in their original grant application. The administrative paperwork and accounting requirements of scientists grew as well; grantees had to document clearly all pieces of equipment needed for research (along with whether or not their institution already owned this equipment) and were required to log the hours spent on a given research project, to name a couple examples.⁵⁹ If Fountain focused on the behaviors of extramurally funded scientists, the members of the Wooldridge committee directed their attention to the function and responsibilities of NIH administrators, particularly with respect to the intramural program’s CSPs. From the perspective of oversight, they noted, investigations taking place through CSPs were not subjected to the same scientific review as extramural projects—a state of affairs that mandated the close oversight of all CSP investigations by NIH administrators.⁶⁰ With Shannon leaving funding decisions to the discretion of NIH institute administrators and advisors, the agency’s various CSPs were not required to undergo any formal, external evaluation.⁶¹

Having some of the largest NIH programs take place without careful fiscal oversight did not sit well with the members of the Wooldridge committee—a reason why its members proposed establishing a Policy and Planning Council at the NIH.⁶² From the perspective of members of the Wooldridge committee, a Policy and Planning Council was especially mandated given what they considered the gross mismanagement of the NIH's CSPs. Members were shocked by the degree to which the Cancer Chemotherapy Program—the collaborative study with the largest budget—lacked strong central supervision, for example.⁶³ As a result, they felt that the overall “benefits from the Cancer Chemotherapy Program had not fulfilled the predictions made at the time of its initiation despite the nearly two hundred million dollars that have gone into it since its start in 1956.”⁶⁴ From their point of view, the program was wracked with scientific inefficiencies, overall slow progress, and inadequate results. But, they maintained, had “very extensive attempts, involving either pilot tests or analyses” to determine the “relative or probable effectiveness of these studies” been carried out (i.e., the function of the proposed Policy and Planning Council), the Cancer Institute might have saved a significant amount of money for the NIH.⁶⁵

The Wooldridge committee members also insisted that the occasional internal reviews assembled at the request of institute directors and the members of their advisory councils were inadequate. “The judgment that must be made as to the current state of scientific knowledge, the probable trend of future discoveries, how long it is likely to take and how much it is likely to cost to achieve a hoped-for result, are often such as to tax the abilities of the wisest and most experienced scientists and science managers,” they wrote.⁶⁶ Given this, they believed thorough feasibility reviews conducted by outside consultants was the only “sound procedure” with respect to program oversight and ought to be put into effect immediately. Their proposed Policy and Planning Council would function in this way as a permanent research management office, servicing the agency through funding “optimization” and through ensuring that its scientific products were “commensurate with their costs.”⁶⁷ In making the assessment of all large-scaled endeavors mandatory, the committee members believed that “the great scientific and management strengths ... can be brought to bear on a final determination of the soundness of the case that has been made for each major proposed project and can, when required, judge the relative merits of proposals coming from various parts of [the] NIH.”⁶⁸

Though NIH administrators denied having “loose administrative practices” when it came to the intramural programs' CSPs, they immediately felt the consequences of these accusations.⁶⁹ For the first time in over a decade, both House and Senate appropriations committees chose not to increase the NIH budget over the president's and secretary's initial request. NIH administrators correctly attributed this, along with the Bureau of the Budget's reluctance to release funds for a new Heart Institute drug study and for the existing Cancer Chemotherapy Program, to rising concerns over the poor management and oversight of NIH spending.⁷⁰ In response, NIH administrators undertook unprecedented efforts to centralize the oversight and management of NIH-funded research, particularly with respect to large intramural programs whose funding requests they directly supported.⁷¹ In April 1965, Shannon instructed institute directors to provide him with a list of all projects requiring central management and a financial commitment of over a number of years as well as those projects not subjected to the normal grant review process. “Neither the use of a contract nor the use of the collaborative budget item as the source of all or part of the funds should be regarded as a condition for identifying a project as collaborative,” he explained.⁷² With this list, he identified projects that, by virtue of their size or organizational complexity, warranted strong management, making CSPs a more functional category that included all administratively and managerially troublesome research endeavors.⁷³

In an effort to co-opt the functions of the proposed Policy and Planning Council, Shannon instructed institute directors to assemble reviews on all large programs that addressed questions of managerial pertinence including whether the project was scientifically productive, whether it had reached its original objectives, and whether its progress indicated a change in direction or objectives—points of focus that had begun defining the agency's overall funding policies by the late 1960s. At a director's meeting in 1968, for instance, Robert Marston, who became NIH director following Shannon's retirement, explained matter-of-factly to institute directors that the oversight environment at the NIH had changed. "The emphasis of the appropriation hearings this year will not be on the identification of unmet needs but on the more traditional exercise of defending all elements of the budget request," he explained. No longer could a charismatic NIH administrator or scientist dazzle congressmen with the promises of science, he suggested. Marston requested institute directors to prepare program overviews for these hearings, explaining that "testimony for each appropriation—and the justification for every activity—must be clear, concrete, and convincing." Anything less would jeopardize the entire enterprise—" *Verbum sat sapienti*" or "a word to the wise," Marston concluded.⁷⁴

Managing the Framingham Study

The new managerial preoccupation around program payoff lay behind the 1965 controversy over continuing the Framingham study. The decision to initiate an extensive internal review of the Framingham study, as one of the six collaborative programs mentioned in the Wooldridge committee's report, was largely a foregone conclusion. In August 1965, Shannon assembled an ad hoc committee to review the Framingham study, with Abraham Lilienfeld, an epidemiologist at Johns Hopkins and long-standing member of the NAHC, serving as chair. Other members of the committee included George James, former commissioner of health of New York City and a member of the Wooldridge committee; George Burch, chair of Tulane's Department of Medicine; and Alan Gittelsohn, professor of biostatistics at Johns Hopkins University. With Dawber's February 1965 "Report of Progress" in hand, Lilienfeld and his fellow committee members spent two days in Framingham, Massachusetts, meeting with the study's directors and staff and observing its clinics and daily operating procedures.⁷⁵

Overall, the members of the Lilienfeld committee found the Framingham study to be an exemplary field study. Lilienfeld stressed throughout his report that the study was considered a productive enterprise—an opinion that was unequivocally held by all members of the committee and supported by all of their professional contacts in the epidemiology, public health, and cardiology communities.⁷⁶ Like Dawber, the members of the review committee valued the Framingham study for being a rare example of a well-designed prospective study of a natural population. "Longitudinal prospective study represents a long-term investment. Up to the present time the major activity of the Framingham study has been the systematic collection of high quality data," they wrote.⁷⁷ Faith that such a population laboratory would undoubtedly yield important insights into health and disease guided their evaluation. Correspondingly, they explained how "an insufficient period of follow-up has elapsed to allow adequate evaluation of the risk of coronary heart disease or other manifestations of atherosclerosis and hypertension associated with these factors."⁷⁸ Moreover, they recommended that the study be continued "indefinitely ... as long as follow up is satisfactory and useful information is being obtained."⁷⁹

The Lilienfeld committee clearly supported the Framingham study's function as a population laboratory. Its members, many of whom had public health backgrounds and had worked on public health surveys and demonstration programs, valued the study for providing reliable incidence and prevalence statistics and for serving as a site to observe

patterns and trends in disease development. William Zukel, a long-time supporter of field epidemiology and acting director of the Heart Institute, read the report as a clear justification for continuing the study for its remaining five years, if not longer.⁸⁰ From his perspective, the review clearly indicated that the Framingham study was living up to its applied- and service-oriented expectations as a collaborative study. In his opinion, “the important function of the Framingham resource both in its generation of additional knowledge on the epidemiology of cardiovascular disease and its key role in relation to other epidemiological studies in which Framingham acts as the standard reference population” was in good working order.⁸¹ For him, the prospect of future research gains was a certainty, as was the study’s continued use and function as a service for epidemiological investigators. “There are additional broad program objectives affecting the total epidemiology program of the Heart Institute which would be severely compromised if this key resource is not maintained further,” he warned.⁸² Zukel also brought up the Framingham study’s use and function as a resource for investigators in which to train and develop methods. The “development of research personnel and resources is still inadequate,” he explained, a state of affairs that further justified the Framingham study.⁸³ “The Framingham Program became one cornerstone of these coordinated efforts and has been responsible for providing the operational epidemiological training and practical organizational supervision for the clinical, electrocardiographic, laboratory, records keeping, autopsy, and follow-up procedures,” he wrote.⁸⁴ Moreover, Zukel saw Framingham’s basic and applied scientific products along with its service-oriented functions as dependent on one another. “The present inter-related functions cannot be separated without jeopardizing the accomplishment of the original objectives both at Framingham and in the coordinated studies of more unusual populations,” he concluded.⁸⁵

However, given a new preoccupation with clear projections of program payoff, Shannon found this defense of the Framingham study simplistic and functionally inadequate. Zukel’s support of the Framingham study’s open-ended observational approach did little to meet Shannon’s request for “highly specific and detailed procedural accounting” (ideally arrived at through the “participation of physical scientists and mathematicians”) needed to address new management standards.⁸⁶ When Lilienfeld and his fellow committee members listed several possible areas of further study, such as the role of coagulation as a factor in heart disease, the relationship among stroke and genetic factors and heart disease, and further subgroup analyses of the population, they did so along the scientific tenets on which the Framingham study was originally founded: a comfort with flexible and open-ended study designs. Lilienfeld and Zukel also did not provide details on how these areas would be studied, how long they would take, or (as Shannon really needed to know) how likely these new areas of inquiry would pay off. Rather, in explaining why more analyses had not yet been conducted on subgroups, Lilienfeld wrote how “certain methodological problems of the statistical analysis have to be solved in order to analyze the collected data to the fullest extent”—an unproblematic observation given how the development of such methods was regarded as a central function of a population laboratory.⁸⁷

Shannon’s response, however, reflected his growing preoccupation with demonstrable likelihoods of program payoff as well as his inclination to evaluate the Framingham study as an experiment rather than an observational field trial and population laboratory. How could he defend a study when its basic methods had not yet been worked out? “This is one of the things that bothers me,” Shannon wrote in the margins beside Lilienfeld’s comment on methodological problems, “massive collection of data with no means or at the very least no attempt to utilize longitudinal change in factors: too much emphasis on initial take-off.”⁸⁸ Shannon’s orientation around program payoff became evident again in Lilienfeld’s discussion of further subgroup analysis. A small number of new cases of heart disease had developed in the population, Lilienfeld explained, making an analysis of subgroups

“manifestly impossible” since “the numbers of cases rapidly disappears as the number of cells increase.” However, added Lilienfeld reassuringly, “because the study group is now entering age groups that have an accelerating incidence of coronary disease, the next five years will provide a sufficient number of cases.”⁸⁹ Shannon balked at such loose speculations, writing, “[W]hat are [the] figures? Expectations?”⁹⁰ He added later how “problems here also relate to a) size of population studied b) specific objectives that can be reached within limitations of study c) recommend [that] biometrical assessment be required,” suggesting that the investigators did not have a large enough population to begin with, which would in turn limit their study’s proposed objectives and future potential.⁹¹

Overall, the Lilienfeld committee did not provide hard figures about what the Framingham study would produce over the next five years or assurances of how its investigators might reduce inefficiencies. Shannon wrote to Donald Fredrickson, the director of the Heart Institute following Zukel’s brief term, about how he was “not convinced one way or the other of the net utility of the study” and that, in his opinion, the site visits to the Framingham program “had not been handled intelligently.”⁹² Without rigorous calculations conducted by biometrists, how could he defend the study to overseers? Fredrickson and Shannon both agreed that the Framingham study’s review committee had fallen short of providing them with a more operational sense of the study’s scientific productivity. “The survival of the Framingham [study] cannot be decided without more intensive analysis,” Frederickson concluded.⁹³ Fredrickson noted how “if Framingham is to continue there must be new project leadership, added biometric competence, revamped program design, and, above all, convincing projection of valid and useful information to be gained by continued investment.”⁹⁴

To acquire the information he sought concerning the exact payoff in continuing the Framingham study, Shannon turned to Biometrics Research Branch statisticians Jerome Cornfield and Tavia Gordon. With instructions from Shannon to evaluate whether the program had established the link between heart disease and high blood pressure and high serum cholesterol (and if not, how long before it did), Cornfield calculated the number of additional coronary cases that would likely develop in the next five years and the degree of certainty these additional cases would provide about the role of cholesterol, high blood pressure, and other hypothesized risk factors for heart disease. Based on these calculations, Cornfield wrote that while these additional cases may strengthen existing findings, “nevertheless, the major single factor effects ... are already reasonably well established and additional cases can be expected to make only somewhat more precise findings which are already precise enough.”⁹⁵ After discussing the reductions in the range of uncertainty for specific variables, Cornfield ended his memo writing, “[E]ven after these reductions, unfortunately, the range of uncertainty ... will still be far from trivial.”⁹⁶ Tavia Gordon also agreed that most of the original hypotheses had been tested adequately and that continuing data collection for an additional five years would do little to refine them.

Even as they acknowledged the unlikely payoff of an additional five years with respect to the study’s original objectives, Cornfield and Gordon nonetheless supported the continuation of the Framingham study as a population laboratory (along the lines of Zukel’s and Lilienfeld’s formulation) in their reports. In providing them with the resource that facilitated their development of methodological techniques in population-based research, these two statisticians valued the Framingham study for far more than a set of narrowly defined objectives. “Framingham carries sufficient authority that there is a tendency to copy [its] procedures. This is a good deal more effective a route to standardization than argument,” wrote Gordon with respect to the study’s use in setting methodological precedents and its service as a flagship for the cardiovascular disease research community. Among the uses of the Framingham study, Gordon saw it as “a proving ground for new techniques,” “a testing

area for new hypotheses,” “a school for investigators,” and finally a “consultative resource.”⁹⁷ With respect to the study’s function in method development, Cornfield concurred. “We have been aware for some time that we are in need of improved mathematical methods,” he wrote. “It is expected that, if [Framingham] continued, it will bear fruit during the next five years.” Cornfield was optimistic that his work on multivariate frequency distributions and on statistical decision theory would yield “order of magnitude improvements in our methods of analyzing not only joint effects for Framingham, but indeed for all the large-scale epidemiological studies now under way.” In effect, Cornfield and Gordon had explained to Shannon how essential the Framingham study was for the advancement of biostatistical method and technique. “Although our participation in ... these activities could, in theory, continue if observation in Framingham were terminated,” Cornfield wrote, “it seems not improbable that the termination of observation would lead to the termination of analyses as well.”⁹⁸

Shannon, however, remained resolved to evaluate the study along the lines of an experiment rather than a population laboratory. Accordingly, Cornfield’s and Gordon’s defense of the study, along with those of Zukel, Lilienfeld, and Dawber, did not justify the study’s continuation, especially given the managerial environment imposed on the NIH “in the wake of Wooldridge.” Relying on Cornfield’s and Gordon’s statistical assessments of the Framingham study’s payoff with respect to its original objectives, Shannon concluded that its continuation for an additional five years would have little impact on the validity of already-identified risk factors for heart disease.

Conclusion

Experimental and laboratory-based styles of research have long dominated the scientific portfolio of the NIH. To explain this, observers point to a faith in the bench-to-bedside model of biomedical advancement, to the distillation of applied and basic research with the growth of the NIH, and to the scientific predispositions of influential NIH administrators like James Shannon.⁹⁹ The 1965 controversy over the Framingham study provides an additional perspective on why laboratory-based science and rigorous experimental study design came to define research supported by the NIH during the 1960s. As this article shows, the 1965 attempt to end the Framingham study had less to do with its failure to meet scientific standards and more to do with its inability to meet emerging managerial values, especially with regard to demonstrating high probabilities of future program payoff, in the political context of the 1960s. In short, the 1965 controversy over the Framingham study resulted from a proclivity on the part of NIH administrators to conflate scientific merit with an ability to demonstrate accountability in a prospective and highly circumscribed manner.

The tension between science and management illustrated in the 1965 controversy over the Framingham study lay at the heart of subsequent controversies over the support of field trials at the NIH. While the Framingham study was allowed to continue, most proposals to assess the impact of risk factor interventions in natural populations in the years that followed were either denied or “administered to death,” as was the case with the Diet-Heart study.¹⁰⁰ This refusal to fund additional population laboratories and field trials led their advocates to refer to the period between 1968 and 1972 as “the Pause”—one that was broken only through the determined advocacy of cardiovascular disease investigators for the continued funding of preventive trials in natural populations. Not surprisingly, this was also a period in which NIH administrators devised new standards for population-based research. As Frederickson described in 1968, the top two ingredients for a successful field trial included the proper definition of the problem and an element of cost-benefit analysis in its study design.¹⁰¹ On a related note, he chastised investigators for “neglecting randomization and the double-blind when they are feasible, because of inconvenience or the interposition of

false issues, of failing to standardize methods or, worst of all, of having misguided faith *in end points that perniciously soften with time.*"¹⁰² Implicit in his comments was his preference for the experimental approach of a well-designed randomized controlled trial over that of observational research conducted via population laboratories. "Clearly the most important first step in scientific investigations is to ask, 'What is the question?'" Frederickson continued, somewhat condescendingly. "In the field, as in the laboratory, the better circumscribed is the question, the more likely is one to obtain an unequivocal answer."¹⁰³

The administrative rationale to end the Framingham study was no match for the counter-response of its advocates; the defense of the Framingham study by investigators like Dawber, Lilienfeld, and Zukel made its termination far too risky an undertaking for Shannon—especially given the NIH's long-held commitments to scientists with respect to administrative intrusion. Regardless of this outcome, the story of the 1965 controversy over the Framingham study gives historians, investigators, and administrators a clearer sense of the managerial stakes in funding population laboratories. Population laboratories pose challenges to administrators in certain managerial contexts because of their inability to be accounted for in certain prescribed ways. The Framingham study did not become problematic due to a tight budgetary climate or to administrators' reluctance to threaten extramural investigators by funding definitive, large-scale studies of risk factors. In the particular oversight environment created by Lawrence Fountain and the Wooldridge committee and the management culture of the federal government that developed during the 1960s, prospective accounting of the likelihood of program payoff reigned supreme—a reason why other considerably more expensive endeavors were able to continue unchallenged while the Framingham study, though requiring a modest budget (both then and now), required "more intensive analysis."¹⁰⁴

As this article illustrates, the 1965 controversy over the Framingham study is a clear and illustrative instance of the drawbacks of equating scientific merit with particular forms of accountability demanded by overseers and can thus serve as a guide to navigating the perennial tensions that arise in managing federally funded science. In running a public institution, NIH administrators are responsible for demonstrating returns on investment—a charge that, depending on the management values in vogue at the time, is likely to affect various scientific endeavors in different ways. During the 1960s, projects like population laboratories and field stations (ones that would ideally continue "indefinitely" so long as they "produced useful results") were considered managerially high risk and became harder to justify. But from the perspective of epidemiologists and heart disease investigators, Shannon simply asked the wrong questions in his evaluation of the Framingham study. Rather than focusing his assessment on whether particular objectives had been met, Shannon might have asked if the collected data were of high quality and if their continued collection and analysis remained essential for addressing pressing questions regarding heart disease, how the agency could facilitate and expand access to other investigators who might most benefit from the data, and whether auxiliary technologies would help increase the scientific value of the data and how to facilitate their development, to name a few.

Since 1970, NIH administrators have funded the Framingham study at levels that have allowed it to continue as a long-term population laboratory. Over the course of its now sixty-two-year history, the study has expanded to include the second and third generation of Framingham study participants, has become a major resource for genome-wide association studies, among many other lines of inquiry, and has produced well over one thousand publications to date. Moreover, its contribution to our understanding of health and disease has recommended the continued support of population studies in addition to laboratory- and clinic-based research at the NIH.¹⁰⁵ By 1972, the Heart Institute actualized such a balanced

strategy by issuing requests for and honoring proposals on preventive trials on populations, through which the community-based Hypertension Detection and Follow-up Program, the Lipid Research Centers, and the Multiple Risk Factor Intervention Trial, among others, were funded. Today, field trials, including the Framingham study, continue through the National Heart, Lung and Blood Institute. The 1965 controversy over the Framingham study and its resolution over the course of the late 1960s, therefore, have served to secure a permanent, albeit less prominent, place for population studies in the overall research portfolio of the National Institutes of Health.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Acknowledgments

I would like to thank Robert Aronowitz, Dan Fox, Toby Jorin, Sheryl Magzamen, Richard Mandel, Robert Martensen, Gerald Oppenheimer, Mark Parascandola, Carl Roth, Alan Schechter, Leo Slater, Paul Sorlie, and Doogab Yi for taking the time to read and comment on this article. Special thanks to Paul Sorlie for providing access to the NHLBI Division of Prevention and Population Sciences's institutional archives and to Richard Mandel for access to the NIH Office of the Director residual files.

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2. See Oppenheimer, Gerald M. Profiling Risk: The Emergence of Coronary Heart Disease Epidemiology in the United States (1947–70). *Internat J Epidemiol*. 2006; 35(3):720–30.
3. For methodological histories of epidemiology and the role of the Framingham study in this history, see Susser, Mervyn. *Epidemiology in the United States after World War II: The Evolution of Technique*. *Epidemiol Rev*. 1985; 7:147–77. [PubMed: 3902493] and Rothstein, William G. *Public Health and the Risk Factor: A History of an Uneven Medical Revolution*. Rochester, N.Y: University of Rochester Press; 2003.
4. See Blackburn's, Henry. A Transition from the Early to the Modern Era of CVD Epidemiology: 'The Pause,' 1968 to 1972. *Preventing Heart Disease and Stroke: A History of Cardiovascular Disease Epidemiology*. accessed July 20, 2011 <http://www.epi.umn.edu/cvdepi/essay.asp?id=66>
5. See chapter 8, titled "A Near-Death Experience" in Levy, Brink. *Change of Heart*. (1):107–26., for a discussion of the response to Framingham's termination.
6. "Memo from Zukel to Shannon titled 'Review of the Framingham Program,'" October 29, 1965, Framingham Study folder, box 99, Office of the Director, NIH Central Files, 1960–82, record group 443, National Archives and Records Center, College Park, Md. (hereinafter OD Central Files).
7. See Blackburn's. *Transition from the Early*. (4). For quote, see "Memo from Zukel" (n. 6).
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14. An early but unsuccessful attempt was Sir James Mackenzie's prospective community study of heart disease in the early 1920s. See Mackenzie, Sir James. The Basis of Vital Activity; Being a Review of Five Years' Work at the St Andrews Institute for Clinical Research. London: Faber and Gwyer; 1926. For Dawber's discussion of this early attempt, see Dawber, Thomas; Meadors, Gilcin; Moore, Felix. Epidemiological Approaches to Heart Disease: The Framingham Study. Amer J Pub Health. 1951; 41:279–86., here 280.
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18. The Framingham study was originally designed as a heart disease demonstration program through the Bureau of State Services. See Oppenheimer, Gerald. Becoming the Framingham Study, 1947–1950. Amer J Pub Health. 2005; 95(4):602–10. [PubMed: 15798116] . For an overview of Harold Dorn's work with the National Cancer Survey, see Lilienfeld, David. Harold Fred Dorn and the First National Cancer Survey (1937–1939): The Founding of Modern Cancer Epidemiology. Amer J Pub Health. 2008; 98(12):2150–58. [PubMed: 18556615] . For an overview of Joseph Mountin's work at the Public Health Service, see Roemer, Milton I. Joseph W. Mountin, Architect of Modern Public Health. Pub Health Rep. 1993; 108(6):727–35. [PubMed: 8265757]
19. Dawber; Meadors; Moore. Epidemiological Approaches to Heart Disease. p. 280
20. Dawber. Report of Progress. p. 46sec. B
21. See Dorn, Harold. Some Applications of Biometry in the Collection and Evaluation of Medical Data. J Chron Dis. 1955; 1(6):638–64. [PubMed: 14381500]
22. See Truett J, Cornfield Jerome, Kannel William. A Multivariate Analysis of the Risk of Coronary Heart Disease in Framingham. J Chron Dis. 1967; 20:511–24. [PubMed: 6028270]
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25. Dawber. Report of Progress. p. 21
26. Ibid., 23.
27. See Oppenheimer. Profiling Risk. (2) and Susser. Epidemiology in the United States. (3)
28. Dawber. Report of Progress. p. 38sec. C
29. Ibid., sec. C, 38.
30. On the field sciences, see Kuklick, Henrika; Kohler, Robert E. Science in the Field. Osiris. 1996; 11:1–265.. For a detailed look at the diverging values of field- and lab-based biology, see Kohler, Robert E. Landscapes & Labscapes: Exploring the Lab-Field Border in Biology. Chicago: University of Chicago Press; 2002
31. For information on its origins and history, see National Institutes of Health. Background Information on the Collaborative Programs of the NIH for the Wooldridge Committee. Comm 4-2 Wooldridge Committee NIH Study April–Dec 1964 folder, box 19, OD Central Files.
32. For an overview of the organization and management of Shannon's wartime malaria research, see Slater's, Leo. War and Disease: Biomedical Research on Malaria in the Twentieth Century. New Brunswick, NJ: Rutgers University Press; 2009
33. Memo from Shannon to Wooldridge committee members, March 19, 1964, found accompanying National Institutes of Health. Background Information on the Collaborative Programs. (31)
34. See the National Heart Institute's “Report of Collaborative Studies” in National Institutes of Health. Background Information on the Collaborative Programs. (31):89–150.
35. Zukel, William. personal communication. McLean, Va: May 19. 2011

36. U.S. NIH Study Committee. Biomedical Science and Its Administration: A Study of the National Institutes of Health. Washington, D.C: Government Printing Office; 1965. p. 126
37. National Institutes of Health. Background Information on the Collaborative Programs. p. 126-27.
38. In addition to providing investigators with coronary heart disease incidence rates, the Framingham population laboratory helped refine oscillographic techniques and equipment for diagnosing peripheral artery disease, to give a very different example. See Dawber. Report of Progress. (10): 22.sec. A
39. National Institutes of Health. Background Information on the Collaborative Programs. (31):90.. The Heart Institute CSP staff and personnel were involved in assisting extramural scientists in cooperative studies on the treatment of acute rheumatic fever, studies on the risk of lipoproteins in atherosclerosis development, drugs in hypertension, feasibility studies of dietary impacts on CHD, among several others. Ibid., 96.
40. Dawber. Report of Progress. p. 47sec. B
41. See Dawber, Meadors, Moore. Epidemiological Approaches to Heart Disease. (14)
42. Dawber. Report of Progress. p. 26sec. A
43. Ibid., sec. A, 26.
44. See Levy and Brink's "The Launch of a Gold Standard," in Levy, Brink. Change of Heart. (1):79–92.
45. The phrase selected for the heading was used as a section title of the directors' meeting minutes, April 22, 1965, Mtgs 1–3 1965 folder, box 39, OD Central Files.
46. Institutional efficiency—increasingly defined by a preoccupation with high probabilities of program payoff—had grown into a more prominent approach in public administration during the 1960s. Paul C. Light, who describes trends in major reforms in public administration, broadly characterizes the dominant reforms occurring during this period as "the watchful eye" for their heavy use of oversight bodies oriented toward managerial efficiency. Light, Paul C. The Tides of Reform: Making Government Work, 1945–1995. New Haven, Conn Yale University Press 1997
47. The House Committee on Government Operations—a congressional oversight body established in 1952 to evaluate "the operations of Government activities at all levels with a view to determining their economy and efficiency"—had incorporated a decidedly operational focus to improve spending efficiency and guarantee investment returns. Public Law 601, Rule XI (H). See also U.S. House Committee on Government Operations. Notes on the Jurisdiction of the House Committee on Government Operations. Washington, D.C Government Printing Office 1959. For more information about this committee, see Henderson, Thomas A. Congressional Oversight of Executive Agencies: A Study of the House Committee on Government Operations. Gainesville University of Florida Press 1970. For reports of these reviews, see U.S. Congress, House, Committee on Government Operations. Health Research and Training: The Administration of Grants and Awards by the NIH. Washington, D.C Government Printing Office 1961 and its *Administration of Grants*. For congressional hearings, see U.S. Congress, House. The Administration of Grants by the National Institutes of Health: Hearings. Washington, D.C Government Printing Office 1962. As many commentators have pointed out, Fountain's reviews were consequential for holding NIH-funded scientists—both intramural and extramural—to the same standards as other public servants with respect to economy and efficiency. For overviews, see Spingarn's, Natalie Davis. Heartbeat: The Politics of Health Research. Washington, D.C Robert B Luce, Inc 1976, Strickland's, Stephen P. Politics, Science and Dread Disease: A Short History of United States Medical Research Policy. Cambridge, Mass Harvard University Press 1972, and Greenberg's, Daniel. The Politics of Pure Science. New York New American Library 1967
48. For Fountain's influence on later reviews, see his testimony regarding the Wooldridge Report in *Congressional Record*, June 21, 1965. Also see C. Miller's report titled "Relationship between Wooldridge Committee Report to Fountain Committee Recommendations," January 6, 1965, in Comm 4-2 Wooldridge Committee, NIH Study, Jan–April 1965 folder, box 19, OD Central Files. For quote, see U.S. NIH Study Committee. Biomedical Science and Its Administration. (36):xv.
49. U.S. NIH Study Committee. Biomedical Science and Its Administration. p. xv

50. Quote from U.S. Congress, House, Committee on Government Operations, . Administration of Grants. (47):25. For general discussion of the repercussions of the Fountain reviews, see Greenberg's, Daniel. The New Politics of Science. Pure Science. (47):270–94.
51. Demonstrating high probabilities of program payoff and variations on this theme had become a widespread management strategy throughout public administration during this period. See Light's. Tides of Reform. (46). Fountain's decision to focus his attention on the NIH was somewhat arbitrary, as described in Greenberg's. Pure Science. (47), but his critiques gained momentum nonetheless and prompted the Wooldridge Review.
52. Quotes from memo from Shannon to Institute and Center directors titled "Wooldridge Committee Survey of the NIH," November 20, 1963, Comm 4-2 Wooldridge Committee Study, 1963 folder, box 19, OD Central Files, 3 and U.S. NIH Study Committee. Biomedical Science and Its Administration. (36):47.. For discussions of the use of physical scientists and mathematicians in matters of procedural efficiency, see Rau's, Erik. Hughes, Agatha C.; Hughes, Thomas P. The Adoption of Operations Research in the United States during World War II. Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After. Cambridge, Mass MIT Press 2000:57–92. and Fortun, Michael; Schweber, SS. Scientists and the Legacy of World War II: The Case of Operations Research (OR). Soc Stud Sci. 1993; 23:595–642.. Dean Wooldridge—himself a trained physicist—ran an R&D company that was considered one of the largest military contractors in history. His role in building aircraft for the military exposed him to Robert McNamara's programming–planning–budgeting–executing offices and their PPBE approaches. For a discussion of McNamara's influence on Dean Wooldridge, see Wildenberg, Tom. A Visionary Ahead of His Time: Howard Hughes and the U.S. Air Force—Part III: The Falcon Missile and Airborne Fire Control. Air Power Hist. Summer;2008 55:5–13.
53. U.S. NIH Study Committee. Biomedical Science and Its Administration. p. 44
54. Ibid., 44.
55. Mountin's managerial approach revolved around the distribution of resources and personnel in accordance with the health needs and priorities of a community. See Mountin, Joseph. Distribution of Health Services in the Structure of State Government. Washington, D.C Government Printing Office 1943. For an overview of Mountin's administrative and managerial career, see Roemer. Joseph W Mountin. (18)
56. For an overview of management styles following the Second World War, see Light's. Tides of Reform. (46). According to Light's periodization, Mountin's approach would be along the lines of "Scientific Management," Fountain's along the lines of "Watchful Eye."
57. "NIH Directors Meeting Minutes," March 25, 1965, Comm 4-2 Wooldridge Committee, NIH Study, Jan–Apr 1965 folder, box 19, OD Central Files, 3.
58. Three prominent commentators of health research policy noted this important transition at the NIH. See Spingarn's. Heartbeat. (47), Strickland's. Politics, Science and Dread Disease. (47), and Greenberg's. Pure Science. (47)
59. See Greenberg's, Daniel. The New Politics of Science. Pure Science. (47):270–94.
60. For an overview of the NIH peer-review system for its extramural program, see Mandel's, Richard. A Half Century of Peer Review, 1946–1996. Bethesda, Md Division of Research Grants, NIH 1996. In 1956, each institute established a Board of Scientific Counselors to review intramural research, but Wooldridge committee members considered this system an internal review process.
61. For more information, see National Institutes of Health. Background Information on the Collaborative Programs. (31)
62. U.S. NIH Study Committee. Biomedical Science and Its Administration. (36):87.. Such offices had become widespread throughout the federal government during the 1960s and were more formally established by President Johnson, who requested each federal agency to "set up a very special staff of experts who, using the most modern methods of program analysis," would "define the goals of their department for the upcoming year." As quoted in Novick, David. Current Practice in Program Budgeting (PPBS): Analysis and Case Studies Covering Government and Business. New York RAND 1973:22.. Overall, program budgeting practices such as those proposed for the Policy and Planning Council were intended to make executive decisions more rational through combining output planning with budgetary considerations. For an overview of the history of PPBE, see *ibid*.

63. See the section on the Cancer Chemotherapy Program in U.S. NIH Study Committee. Biomedical Science and Its Administration. (36):39–41.
64. U.S. NIH Study Committee. Biomedical Science and Its Administration. p. 40
65. Ibid., 40.
66. Ibid., 42.
67. Ibid., 37.
68. Ibid., 43. Again, the incorporation of such scientific and management strengths was occurring throughout program evaluations throughout federal agencies. See note 62.
69. U.S. Congress, House, Committee on Government Operations. Administration of Grants. p. 26
70. “NIH Directors Meeting Minutes,” March 25, 1965, Mtgs 1–3 1965 folder, box 39, OD Central Files, 4.
71. Shannon and top administrators sought to incorporate these new managerial functions into the existing NIH Office of Program Planning. According to a history of this office, the Wooldridge Report was responsible for the dramatic staff increase that occurred in the late 1960s. See Carson’s, Bruce F. A Short History of the Office of Program Planning and Evaluation (OPPE). History of OPPE folder, O&M 3 file, residual holdings, NIH Directors’ Files. NIDB Bethesda, Md
72. “Directors’ Meeting Minutes,” May, 13, 1965, Mtgs 1–3 1965 folder, box 39, OD Central Files, 2.
73. “Collaborative Research FY 1964–1968 A Preliminary Analysis,” prepared on July 2, 1965, Comm 4-2-A file, NIH Directors Meeting Minutes folder, residual holdings, NIH Directors’ Files, NIH, Bethesda, Md.
74. “NIH Directors Meeting Minutes,” January 18, 1968, quotes from pp. 2–3, in Comm 4-2 Wooldridge Committee, NIH Study, Jan–Apr 1965 folder, box 19, OD Central Files.
75. “Report of Visit to Framingham Project by Scientific Review Committee, August 25–26, 1965,” Framingham Study folder, box 99, OD Central Files, 4.
76. Ibid., sec. C, 1.
77. Ibid., sec. C, 1.
78. Ibid., sec. C, 7.
79. Ibid., sec. C, 6.
80. Zukel credits Dr. Jim Watts, director of the NHI, with encouraging other investigators to undertake prospective follow-up studies of other population groups to validate findings from the Framingham study. Other important prospective studies such as those of Jeremiah Stamler, Oglesby Paul, Joe Doyle, and Frederick Epstein were acquiring valuable findings of new risk factors for heart disease.
81. “Memo from Zukel to Shannon,” December 30, 1965, Framingham folder, box 99, OD Central Files, 4.
82. Ibid., 2.
83. Ibid., 1.
84. Ibid., 2.
85. Ibid., 4.
86. Quotes from U.S. NIH Study Committee. Biomedical Science and Its Administration. (36):44. and NIH Directors Meeting Minutes(57):3.
87. Report of Visit. p. 4sec. C
88. Ibid., sec. C, 4.
89. Ibid., sec. C, 4.
90. Ibid., sec. C, 4.
91. “Memo from Frederickson to Shannon,” December 5, 1966, 1, Framingham Study folder, box 99, OD Central Files.
92. Ibid.
93. Ibid.
94. Ibid.
95. “Memo from Cornfield to Shannon,” December 9, 1965, Framingham Study folder, box 99, OD Central Files.

96. Ibid.
97. "Memo from Tavia Gordon to Shannon," January 24, 1966, Framingham Study folder, box 99, OD Central Files.
98. Ibid.
99. For a discussion of these, see Greenberg. *Pure Science*. (47). For Shannon's particular scientific training, see Kennedy, Thomas J. James A. Shannon and the Beginnings of the Laboratory of Kidney and Electrolyte Metabolism of the National Institutes of Health. *Kidney Internat*. 1999; 55:326–33. and Slater's. *War and Disease*. (32). For discussions of the type of research supported by the NIH, see Comroe, Julius. *Scientific Basis for the Support of Biomedical Science*. Science. 1976; 192:105–11. [PubMed: 769161] and Fredrickson, Donald. *Biomedical Research in the 1980s*. *New England J Med*. 1981; 304(9):509–17. [PubMed: 7005685]
100. Harry Marks, for instance, noted the heavy and, what he considered, unprecedented involvement of top-level administrators in the review and evaluation of this study. See his *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900–1990*. New York:Cambridge University Press 1997:187.
101. Fredrickson. *Field Trial*. (8):985.. His ideas echoed the Greenberg Report. See Heart Special Project Committee. *Organization, Review and Administration of Cooperative Studies (Greenberg Report): A Report from the Heart Special Project Committee to the National Advisory Heart Council*, May 1967. *Controlled Clin Trials*. 1988; 9:137–48. [PubMed: 3396364]
102. Fredrickson. *Field Trial*. (8):986. (emphasis added).
103. Ibid., 987.
104. In 1965, the estimated cost of continuing the Framingham study was \$350,000. See Report of Visit. (75):7.sec. C
105. See the National Heart, Lung, and Blood Institute. *Arteriosclerosis: The Report of the 1977 Working Group to Review the 1971 Report of the Task Force on Arteriosclerosis*. Washington, D.C:Government Printing Office 1977. See also International Working Group on Field Trials. *Mass Field Trials on the Prevention of Coronary Heart Disease: Perspectives and Tasks: Report of an International Working Group Meeting Makarska, Yugoslavia 19–24 September 1968* <http://www.epi.umn.edu/cvdepi/pdfs/Makarska%20doc.pdf>

Biography

Sejal S. Patel received her Ph.D. from the University of Pennsylvania's History and Sociology of Science Department. In 2007, she was accepted to the Robert Wood Johnson Foundation's Health and Society Scholars Program at the University of Wisconsin. She is currently senior research historian at the Office of History, National Institutes of Health. Her research interests include the history of twentieth-century research practices in medicine and public health. She has conducted research on community medicine and the rise of biostatistics. Her current project on the political economy of American medical research develops a broader framework for understanding postwar health research through a critical focus on the National Institutes of Health.