Taking Stock: The Past and Future of Biomedical Research Funding

Bhaven N. Sampat

1 Introduction

Biomedical research policy is perhaps the best case setting for economics and other research on the science of science funding. For decades, researchers have had access to administrative data on grants and outputs (from NIH RePORTER) and publications (PubMed). Patents and citations, workhorse outcome measures in economics, are plausibly more informative as indicators of innovation in life sciences than other fields (Mezzanotti and Simcoe 2023; Sampat 2010). Through FDA administrative data, patents can be linked to drugs (Durvasula et al. 2023), which in turn can be connected to measures of both private and social value. These features make biomedical research policy a useful strategic research site for science of science funding research. And because the social value of medical innovation can be enormous (Nordhaus 2002; Murphy and Topel 2006; Lichtenberg 2022; Cutler 2004), the returns to more effective funding policy are correspondingly high.

For all these reasons, there is a large economics literature on biomedical research funding policy. Much of this work has focused on the U.S. National Institutes of Health (NIH). For roughly 75 years, the NIH has been the world's largest public funder of research. The NIH has typically enjoyed high-level support unusual in public policy, sometimes described as the "crown jewel" of the federal government.

Despite the high-level support for medical research spending in general, controversy has been a constant throughout the history of the NIH, including around the returns to NIH funding, the peer review process, responsiveness of funding to health outcomes, targeting, organizational structure efficiency vs. equity considerations, red tape and accountability, and other issues. This chapter reviews the main policy questions and controversies that have accompanied NIH's post-World War II expansion. Drawing on the history of the NIH, it shows how many of these questions reflect strains on and reconsideration of the postwar "social contract" for science itself. It then takes stock of what the economics of science and related literature can and cannot say about these big questions, highlights major gaps, and provides suggestions for future work.

2 Historical Background

2.1 Wartime History

Although its roots extend to the nineteenth century, the NIH was transformed after World War II. As Gross and Sampat (2025) show, the wartime research effort was the first serious government funding of medical research in the U.S. This effort was led by the Office of Scientific Research and Development's Committee on Medical Research (CMR) and resulted in a range of medical advances that were not only crucial for Allied victory, but also shaped science, innovation, and drug development in the decades that followed. The advances included the mass production of penicillin, blood substitutes, malaria treatments, steroids, and many others (Andrus 1948; Gross and Sampat 2025).

The wartime model differed from what the NIH would later become. While both involved government funding for research, during the war CMR funded primarily research targeted at solving specific problems quickly, including some fundamental work but also applied research, development, clinical trials, manufacturing, scale-up and diffusion. It set priorities based on wartime needs in collaboration with the military and, in addition to funding, had an important role in coordinating research portfolios and knowledge flows across different actors involved in the

effort (Gross and Sampat 2025). This heavily managed top-down approach is in sharp contrast to the bottom-up investigator-initiated model that would dominate NIH funding in the postwar era, as discussed in more detail below.

2.2 Endless Frontier and the Social Contract for Science

The successes of the wartime effort (in medicine and beyond) prompted numerous parties to propose plans for postwar government funding of research. The most well-known of these is Vannevar Bush's *Science, the Endless Frontier*, published in 1945. In it, Bush, the OSRD director, made the case for government funding of research in peacetime. He made three major claims. First, basic research is technologically valuable, the driver of technological innovation. In doing so, he emphasized that many of the major advances during the war relied on pre-war basic research. Second, anticipating the Nelson-Arrow market failure rationale for research funding, that while industry would fund applied research, it would underinvest in basic research. Third, basic research is difficult to plan. Finally, given complementarities between teaching and research, universities are the natural home for basic research.

Mixing and stirring these gives the Bush plan for postwar science policy: significant government funding for basic research at universities in peacetime, with limited political interference. This hands-off approach to science funding differed markedly from the wartime model. The Bush Report was in large part a response to other approaches (in particular from Senator Harley Kilgore, D.–W.Va) for a different type of postwar science funding, one with more top-down planning, a focus on both basic and applied problems, and more direct political accountability. Bush was making an argument not just to taxpayers that science ought to be supported after the war, but also to scientists—many of whom resented the political control of science during the war—that government funding would not compromise the autonomy of scientists or universities.

Though he did not use the term, the Bush Report is sometimes credited with establishing the postwar social contract for science, the idea that the government would provide resources to the scientific community and provide it with freedom from top-down political management "an

unusual degree of intellectual autonomy and self-governance" as Brooks (1988) put it. In return science would contribute to increases in economic welfare, national security, and health (Brooks 1988; Guston 2000). Accountability would operate at a broad level, rather than project specific micro-management or political oversight, with the scientific community and universities in charge of what research was prioritized, rewarded, and quality-control (Guston 2000).

2.3 New Horizons in Medical Research: The Rise of the NIH

Amidst their many disagreements, Vannevar Bush and Harley Kilgore agreed there should be a single major government agency funding research after the war, the "National Science Foundation" in Kilgore's Bill and the "National Research Foundation" in the Bush plan. However, while Bush, Kilgore and their allies debated the different visions in the years after the war, something had to happen to the leftover OSRD contracts. Though it was perhaps no one's first choice to do so, through deft political maneuvering the NIH, sidelined during the war, took over the 46 CMR contracts, effectively launching the NIH's grants program (Sampat 2023). Other agencies took over other parts of the wartime research, so that by the time the National Science Foundation (NSF) Act was signed in 1950 the Foundation both Bush and Kilgore wanted was a small part of overall federal R&D funding, as it remains today.

The NIH program was expected to be modest. NIH hired Cassius Van Slyke, a Public Health Service veteran recovering from a heart attack to run it, describing it to him "just as an incidental part time, left hand, lower drawer of the desk sort of activity" and that he "positively wouldn't have to work more than two hours any day and probably not more than four or five hours a week" (Van Slyke 1963, p. 27).

In a December 1946 *Science* piece "New Horizons in Medical Research" Van Slyke advertised the new NIH extramural program to the scientific community. Like Bush, he emphasized the difference between this program and the wartime effort and the absolute commitment of the NIH to limited political interference and to scientific freedom and accountability.

Specifically, he wrote that the purpose of the grants program was to fund science that the sci-

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New Horizons in Medical Research

C. J. Van Slyke

Medical Director, U. S. Public Health Service, and *Chief, Research Grants Division*
National Institute of Health, Bethesda, Maryland

A LARGE–SCALE, NATIONWIDE, peacetime program of support for scientific research in medical and related fields, guided by more than 250 leading scientists in 21 principal areas of medical research, is now a functioning reality. The program, based on U. S. Public Health Service Research Grants financed by public funds, supports research—conducted without governmental control—by independent scientists. The purpose of these grants is to stimulate research in medical and allied fields by making available funds for such research and by actively encouraging scientific investigation of specific problems on which scientists agree that urgently needed information is lacking. Accompanying this purpose is complete acceptance of a basic tenet of the philosophy upon which the scientific method rests: The integrity and independence of the research worker and his freedom from control, direction, regimentation, and outside interference.

The U. S. Public Health Service Research Grants, in operation as a medical research program of scientists and by scientists, may have early and profound effects upon the course of medical history and the national health.

The program, both in principle and as administered, has been welcomed and approved wholeheartedly by leaders in medical research. A total of 264 research projects, supported by $3,900,000 granted from the inception of the program late in 1945 up to 15 October 1946, already have been undertaken in 77 univer-

past a large amount of potentially very important research has not been conducted because funds have not been available to pay for it. Many universities and other nonprofit institutions have extremely limited funds for research, even though their teaching staffs, graduate students, and other personnel have the talent, training, and interest necessary for scientific investigation. Although research conducted by industrial organizations does add considerably to the total fund of medical knowledge, such research quite often must be directed toward specific goals.

The great benefits from all medical research, wherever conducted, are received by the millions of people whose lives are made healthier, happier, and longer through widespread application of knowledge gained in research laboratories. Conversely, research not conducted for want of funds is very costly to the same millions. The essence of these facts, as related to the Research Grants program, has been stated by the National Health Advisory Council: "There are few purposes for which public funds could be used more appropriately than to discover ways to prevent and cure illness and to prolong useful years of life." The function of the Research Grants is to make it possible for workers in medical and allied sciences to expedite, extend, and intensify health-saving and life-saving research.

During the war it frequently was necessary to sacrifice fundamental, not immediately applicable research in order to arrive at specific objectives promptly;

Figure 1: Announcement of NIH Extramural Research Program

entific community deemed important with "complete acceptance of a basic tenet of the philosophy upon which the scientific method rests: The integrity and independence of the research worker and his freedom from control, direction, regimentation, and outside interference." (559). Furthermore, "support of research through the use of Research Grants funds does not imply in any way any degree of Federal control, supervision, or direction of the research project." (563)

Van Slyke emphasized the importance of freedom not just as a value but for instrumental purposes: "During the war it frequently was necessary to sacrifice fundamental, not immediately applicable research in order to arrive at specific objectives promptly; promising bypaths often had to be by-passed. In the normal course of scientific investigation, however, the bypaths quite often lead to more important findings than do the roads from which they branch." (559)

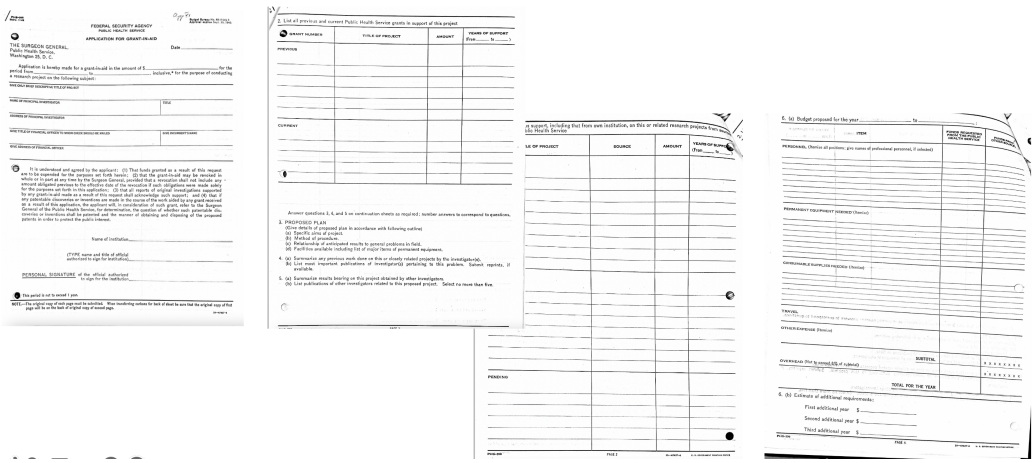


Figure 2: Early NIH Grant Application Form

Limited red tape in application and reporting was also a key characteristic "in order not to divert the researcher's time unnecessarily from the actual conduct of the research" (563). Although not explicitly referring to the Bush Report, Van Slyke tried to operationalize Bush's philosophy at NIH. In doing so he tried to do something Bush was skeptical was possible: institutionalizing freedom and autonomy at a mission-oriented agency.

2.4 Postwar Growth and Controversy

Bush was also worried about the ability of a large-scale R&D program (in medicine or elsewhere) to systematically identify high quality research (Sampat 2023), and both federal R&D in general and medical research in particular likely surpassed both his and Van Slyke's vision for the scale of the enterprise.[1]

It may be that neither anticipated support for NIH growth from disease advocates like Mary Lasker and Florence Mahoney, who frustrated with the inability to pass national health insurance, turned to increasing funding for biomedical research as a more politically feasible route to improving health (Sampat 2012; Cook-Deegan and McGeary 2006). Or legislators including John Fogarty and Lister Hill, who learned that championing medical research was politically popular (Drew 1967). Perhaps most shocking was the evolution of universities and the scientific community who by the 1970s had abandoned their 1940s reticence about receipt of federal funding and, in some cases, had become (in the words of political scientist Don Price) "addicted to federal support" (Price 1978, p. 76).

Whatever the causes, the NIH budget increased from $3.4 million to $81.1 million annually from 1946 to 1955, and to $959 million by 1965.[2] The agency also increased in scope, with the addition of disease specific institutes (modeled after the pre-war National Cancer Institute). Although the NIH had generally been a politically popular agency during the postwar decades, these increases in scale and scope raised a range of questions around the right way to allocate the funds, many of which posed challenges to the postwar social contract for science.

2.5 Postwar NIH Research Policy: The Big Questions and Debates

Since World War II at least a dozen major reports have tried to evaluate the NIH research funding program.[3] Remarkably, many of the same questions have resurfaced periodically around NIH

[1] By 1963 Bush was telling Congress the postwar research program was "overextended" and "[I]f the country pours enough money into research, it will inevitably support the trivial and mediocre" (Bush, 1963).

[2] https://www.nih.gov/about-nih/nih-almanac/appropriations-section-1

[3] I review themes from the major reports here, building on the approach in Sampat (2023), which focuses on the reports' treatment of peer review, which in turn builds on McGeary and Smith (2002), which focuses on their

extramural research funding policy. Some of the themes relate to the social compact, some echo controversies in the Bush-Kilgore debates, and other concerns relating to the growing scale of the enterprise that Bush and others anticipated. In this section, I discuss the major issues.

2.5.1 Value of NIH Research Funding

The funding of biomedical research after the war has been characterized as a "novel experiment in American medicine" (Keefer 1969, p. 62). The dramatic postwar expansion of the NIH extramural research program, a different model of government research funding, was the same.

Was it a success? Many of the early reports struggled with the basic question of whether the bet was paying off, whether returns justified the new expenditures. With the growth of NIH investment by the mid-1950s, the fiscally conservative Eisenhower administration was worried about the rapid increase in Congressional appropriations. The Secretary of Health, Education and Welfare asked the (still relatively new) National Science Foundation to initiate a study which included, among other tasks, the charge to evaluate the HEW effects on "the conduct and progress of medical research and education" with a heavy focus on the NIH. The NSF assembled a team of scientists (many of whom had been involved with CMR during the war) to conduct the evaluation, led by the Dean of the Yale School of Medicine Cyril Long.

The 1955 Report (Long, 1955) viewed NIH extramural program as successful from one perspective: preventing bureaucratic interference with science. On the actual question of value it was more circumscribed, asking whether, given the rapid growth in scale, it had expanded beyond the scale where worthwhile programs were being supported. It also raised questions about value added, asking whether NIH funding was needed in fields where philanthropies were also active. Over the years, other reports also opined on this question. Long made a number of controversial recommendations. Most prominently, the report recommended that the extramural program be severed from NIH, reflecting concerns that the categorical disease structure at NIH would hinder basic research and scientific autonomy at medical schools.

examination of NIH organizational structure.

For various reasons, including the resignation of the Health, Education and Welfare, the Long Report was shelved (Sampat 2023; McGeary and Smith 2002; Strickland 1989). Several years later, the new Secretary, together with new NIH Director James Shannon (a leader of CMR's malaria research program during the war), appointed a new committee to review the NIH, given rapid expansion and the need to ensure that the program was making "maximum contribution based on wise policies, sound administration, and competent scientific staff" (xii). This study was led by Stanhope Bayne-Jones, another former Yale Medicine Dean. The Bayne-Jones Report (1958) asserted that the value of government funded medical research was high and should not be restricted by a lack of funds, even though it stressed the importance to universities and the public of having a diverse set of funders (including industry and philanthropy) in the system.

Following several more years of budget increases, Congress created a Committee of Consultants on Medical research "to determine whether the funds provided by the Government for research in dread diseases are sufficient and efficiently spent" (1960). The Committee, chaired by Boisfeuillet "Bo" Jones was charged to consider market failures, whether the research "is of such high priority and great promise that its deferment would be likely to delay progress in medical discovery" and "that it will not bring about the substitution of federal for non-federal sources of support" (121). It echoed the call for further investment, arguing that NIH research funding has improved medical training and practice, and the returns to health improvement are high. The report concluded "if adequate support is provided, the great advances in the medical sciences which this country has witnessed in the past 60 years ... will prove only to be the prologue to the great human drama which will be played in the battle against disease and disability during the decades immediately ahead." It too opined on crowding out, asserting (based on overall trend data) that "the Federal program has served as a stimulus rather than a deterrent to non-Federal sources of support" by firms and philanthropies.

During the Kennedy Administration, another report was commissioned to judge "whether the American people are getting their money's worth from the expenditure." The Committee, chaired by Dean Wooldridge (Wooldridge 1965), concluded the answer was at high level emphatically yes,

but raised some concerns about the machinery and mechanisms through which funds were allocated that will be discussed in more detail below. In general, most of the major postwar reports on NIH were similarly positive on the overall value of NIH spending.[4]

2.5.2 Peer Review

This section draws on and synthesizes material from Sampat (2023).

Review Quality. In his 1946 *Science* article Van Slyke explained that the grants would be reviewed in a two stage process. Three National Advisory Councils (at the time for the NIH overall, but also Cancer and Mental Health) would recommend grants to the Surgeon General for awarding, based on the first-stage "advice and recommendation" of study sections "composed of groups of scientists in the major categories of medical research" (560). This "dual" review process with the scientific community reviewing for scientific merit and the Councils (eventually associated with specific NIH Institutes) making final determinations echoes the Committee on Medical Research review process (Mandel 1996; Sampat 2023), and remains the basic template today.

In the first stage of the peer review process, the study section members aim to determine the overall scientific impact of a proposed project on a field. The Bush Report itself expressed doubts about the ability to do so reliably if the scale of applications grew large (Sampat 2023), concerns which proved prescient at NIH.

By 1958, the Bayne-Jones report noted the system was designed for a small number of small grants, raised questions around whether it was fit for purpose and needed to be updated as the number of applications grew. Around the same time, a series of reports critical of NIH grants administration from Congressman Lawrence Fountain (D., North Carolina) argued the scale of applications study sections were receiving by then was straining the system's ability to adequately and expeditiously review them (Fountain 1962). The 1965 Wooldridge Report "Biomedical Science and Its Administration" provided a much stronger endorsement of the review process and NIH than

[4]The most serious criticism was from the Fountain Committee investigations, discussed in more detail below.

Fountain, but noted reviewing applications imposed major costs on the reviewers, and that the bespoke high-touch review that was once possible were by then gone:

> The cost today is considerable, in terms of the manhours of scarce scientific effort required. Unfortunately most pressures are in the direction of increased demands on the Study Sections. Almost unanimously, the extramural investigators visited expressed a desire for more contact with and advice from the highly regarded scientists who comprise the Study Sections. They would like more site visits; they would appreciate more suggestions from Study Section members about ways of improving research plans; they would like more explanation of the reasons why proposals are not approved.

Subsequent reports (Rogers 1976) echoed these concerns.

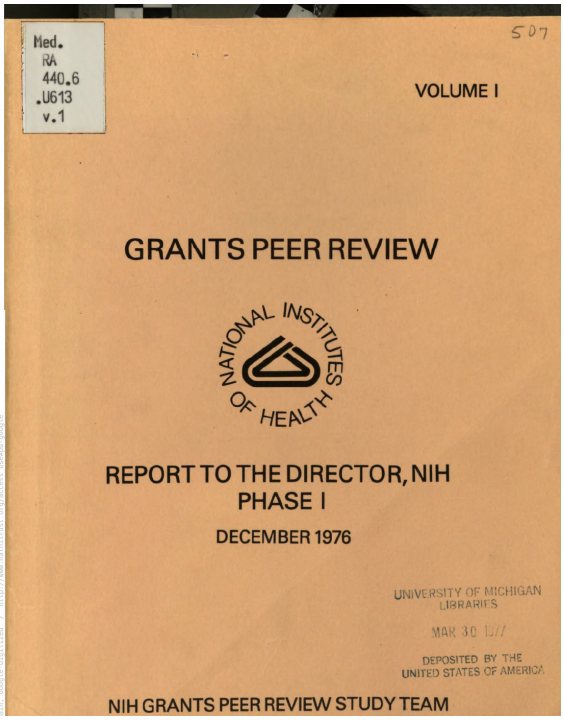


Figure 3: Grants Peer Review Study Team

Innovation. A more specific charge was that the review process itself is biased against innovation. An internal report, the 1976 Grants Peer Review Study Team (GPRST) like Wooldridge, came out

positive on the quality of peer review overall, but noted a view in the scientific community that "the peer review system favors the support of traditional, conservative, orthodox research" at the expense of "innovative, unorthodox research" noting further the latter is often done by young investigators without a track-record, who may face their own biases in the process. GPRST also noted that some observers attributed over-representation of "the establishment" representing "current state of the art" on panels (GPRST did not endorse this claim) and argued that limited funds were part of the reason the agency focused on safe bets. This idea would persist in later internal peer review working groups (Sampat 2023). As part of its response in 1997 "Innovation" was added as a review criterion for NIH applications, updated various times since. But the concern persisted, so that in 2003 the National Research Council (in a report on the organizational structure of NIH; see below) noted high-risk and innovative proposals "do not fare well in a review system that is driven toward conservatism" proposing a DARPA-like authority outside the system to support innovative research that NIH peer review would not.

Concentration and Bias. Policymakers have also raised a range of questions regarding the concentration of NIH funding, in a handful of institutions and investigators, and related questions about how well new investigators fare.

One question is around geography and institutional balance. A central theme in the Bush-Kilgore debates was around whether there should be explicit geographic formula funding for research (Kevles 1977) in part a reaction to the heavy concentration of OSRD funds during the wartime effort (Gross and Sampat 2025). NIH had no such mandate, but throughout the years this has been a cause of concern. The 1958 Bayne-Jones Report noted a tradeoff between short- and long-run goals, arguing that while in general grants should go "to the individuals and institutions best able to produce results of high quality" over the long-run the system would benefit from a "geographically dispersed system" of first-rate research institutions. Fountain (1962) noted both an institutional concentration of funding and of study section members, and "the normal tendency of advisors to favor the institutions and scientists they know best" and also called for grants to build

up research capacity. The Wooldridge Report (1965) noted similar issues.

A 1976 Congressional Investigation of NIH practices (Rogers 1976) raised concerns about geographic and institutional concentration, as well as conflicts of interest in peer review process and whether it drew sufficiently on young scientists. This was viewed as problematic since "science belongs to the young" and "has its own orthodoxy" (44). The same year a report of the President's Biomedical Research Panel (1976) noted allegations that NIH peer review fostered "mutual protection" within the scientific establishment, was elitist, even a "self-perpetuating oligarchy" (17). It rejected these accusations based on its review of data, acknowledging however that the system was elitist, but that this "gives the taxpayer more for his dollar" (18).

2.5.3 Targeting and Management

As I have argued previously (Sampat 2012; 2023) in addition to Bush-ian (and Van Slykian) concerns about whether the system is effectively identifying and supporting high quality science and supporting "innovative" research, there are also long-standing questions, more Kilgorean, about how effectively it targets specific problems or improves health. These, in turn, have prompted policy discussion about the feasibility and desirability of more targeting and political oversight in allocation, beyond what the peer review system as originally advertised by Van Slyke intended.

Early reports (Long 1955; Wooldridge 1965) celebrated that the NIH research enterprise had successfully supported basic research without undue interference. However this meant the NIH lacked levers to target specific problems. By the 1950s various institutes had started using contracts not grants for collaborative programs (including with industry, but also practitioners) to solve specific problems (including the Cancer Chemotherapy Program and Framingham Heart Study). The Wooldridge Report (1965) was critical of these, arguing they were of "considerably more variable" quality than traditional investigator-initiated grants, and difficult to review using standard peer review process, and often are not viewed by reviewers as high-quality research. A 1966 evaluation commissioned by the Secretary of Health, Education, and Welfare picked up these issues noting "the emergence in recent years of programs of directed research and development"

which were "a significant departure" from standard peer reviewed grants. It noted the lack of effective program managers at NIH for such projects and inability of peer reviewers to handle them. These issues famously came to a head during the War on Cancer, when Mary Lasker and others were so frustrated with the inability of the traditional NIH peer review process to target specific problems (and move beyond basic research into development) that they tried to sever the National Cancer Institute from NIH (Rettig 1977).

An underlying assumption in the Ruina Report was that the standard NIH peer review process was not designed to support applied work. This was echoed in the aforementioned Rogers Report (1976), which observed, among its other criticisms of peer review, that the process "has a strong bias for basic scientific inquiry and a disdain for applied research." It noted further that this together with NIH "managerial choice" for institutes to strictly follow scores from first stage review effectively limits the ability of the agency to target.

After the War on Cancer, disease lobbies continued to push the NIH towards application and targeting. Following successes of the AIDS and breast cancer lobbies in obtaining funds for research in the 1980s and 1990s, disease groups began to lobby for Congressional earmarks (generally "soft" earmarks in the form of Congressional report language) to increase funding for their diseases, or general "requests for applications" (RFAs) and "requests for proposals" on particular topics. Activists pushing these more targeted efforts argued that these were the only way to introduce health considerations into a peer review process run by scientists, designed to support science (Sampat 2012; 2023; IOM 1998).

2.5.4 The Organizational Structure of the NIH

The organizational structure of the NIH—currently 27 Institutes and Centers—is another perennial issue, closely related to priority setting.

At the end of World War II there were only two components, the National Institute of Health and the National Cancer Institute (NCI). By 1950, several new ICs had been formed: the National Institute for Mental Health (NIMH), the National Heart Institute (later NHLBI), the Dental Institute

(later NIDCR), and Diabetes (NIDDK), Neurological Diseases and Stroke (1950) had been created. The National Microbiological Institute was also created in 1948, and Congress changed its name to the National Institute of Allergy and Infectious Diseases (NIAID) in 1955, a move that both Congress and disease advocates thought would be financially beneficial.[5]

At the time the 1955 Long Report was written, there were seven Institutes. The authors acknowledged the benefits of this approach in generating public support:

> This so-called "categorical approach" to the problems of certain diseases has been justified in some quarters (a) because it is believed that the support of the Congress and the people is more easily obtained for research in diseases for which no cure is known and which all individuals in consequence fear, since they identify them in terms of themselves or their families; (b) the widely held belief, no doubt fostered by certain wartime successes, that the solution of pressing national needs is best met by drafting all available talent into the pursuit of the desired objective.

But like Bush and Van Slyke did before them, Long argued "successful research into the cause and cure of disease has frequently come from most unexpected quarters, and has often been pioneered by individuals whose work at the time was unrelated to any disease state."[6]

Bayne-Jones (1958) was more positive on the Institute structure, asserting 'Provision of research grant funds through Institutes named after diseases—the so-called categorical approach—has been called a defect in the system. ... [T]his is not inevitably the case. The system is administered to permit support of fundamental as well as applied research. The system does not lead to overemphasis upon applied research. The investigator is not required to "angle" or "slant" his research.'

Later reports too argued that with first stage study sections "the glue that holds the categorical system together" the NIH overall does a good job directing research at broad diseases and problems

[5]Drew (1967 recounts "The story goes that in the days when one of the NIH branches was called the Institute of Microbiology, one congressman asked, "Whoever died of microbiology?" The name was changed to the Institute of Allergy and Infectious Diseases."

[6]Further questioning the wartime logic the report argued "This is the essential difference between the production of a new weapon of war and a new weapon for the eradication of disease. Success in the former can come by plan and design, in the latter the accidental discovery of the solitary worker may be the decisive factor."

without heavy handed targeting or direction. Others emphasized the importance of the structure in helping Congress and the public understand what the NIH does (Rogers 1976; IOM 1984; NRC 2003).[7] Beyond questions about whether it skews research towards the applied, another concern about the proliferation of institutes has been fragmentation of the research effort across institutes, with organizational boundaries not lining up with scientific boundaries, limited coordination and cross-NIH planning, and larger overhead and administrative costs for the enterprise as a whole.

2.5.5 Red Tape

Both the Bush Report and Van Slyke sought to allay fears among scientists that government funding of research would necessarily lead to the bureaucratization of research, impede autonomy, and stifle scientific creativity. Early reports (e.g. Bayne-Jones and Bo Jones) generally complimented the program on achieving the impossible: government funding of research with limited bureaucratic interference. The Fountain Committee investigations in the 1960s represented a turning point. Fountain played the role of government watchdog, complaining about lax reporting requirements, limited review of budgets, and NIH policies "permitting the investigator unlimited freedom to change an approved research project once the grant has been awarded" as being not sufficiently subject to public oversight, arguing that NIH should be approving these changes as well if the intent was to fund a project, not a person. He noted, with disapproval, "grantees are permitted almost complete discretion in determining use of money once awarded" and "reporting requirements are at a minimum" (37).

When his initial concerns were not taken seriously by then Director Shannon, he escalated criticisms on "basic management" problems at NIH (Sampat 2023). In 1964 the NIH budget was

[7] James Shannon fought hard against new Institute formation, arguing that Institutes create artificial boundaries between fields and administration difficulties, but acknowledged (in the context of the creation of the NEI in 1968 and NIA in 1974) that new institutes "did provide greater visibility" (Shannon 1975). Over twenty-five years later a former NIH Director Harold Varmus (2001) penned a *Science* editorial even more critical of the proliferation of NIH Institutes, but acknowledged political economy benefits, observing "The NIH thrives politically and financially from the enthusiasm of its supporters [which] is enhanced when a new institute or center is founded in law, especially when the legislator-founders are prominent, the Administration gets credit for its role, and the advocacy groups feel a loyalty to 'their' unit" and bringing 27 Institutes and Center directors to the Hill to recite all that they are doing "cannot hurt during the budget process" (1904).

reduced after many years of steady increase. The NIH response was to institute new regulations, policies, and paperwork for grantees (Sampat 2023). A core element of the implicit postwar bargain—the idea that government funds science and lets scientists self-govern—was superseded by pressures for accountability of public funds. The social contract was replaced by more specific terms, stipulations, and requirements in real grants and contracts. Importantly, in his criticisms of lack of oversight and accountability for funds Fountain made little mention that the system was not delivering on its part of the bargain, rather equating more oversight with "the most prudent use of grant funds" (72).

Just three years later the Wooldridge Report (1965) would bemoan "The red tape harassments that currently annoy many scientific investigators" noting however "We have seen no evidence that the newly instituted NIH administrative procedures have significantly interfered with scientific freedom and initiative" though warned they "portend more severe restrictions in the future" and "often initiates more severe restraints [university administrators] probably from the fear that their accounting procedures will be scrutinized even more carefully in the future."

Partly in response to concerns about red tape and administrative burden, a number of the reports suggested exploration of new mechanisms as well (beyond the traditional research project grant) including institutional grants[8] and longer-term funding (Bayne Jones 1958; Bo Jones 1960; Wooldridge 1965).

Though the specific later reports surveyed here had surprisingly little to say about the topic, the growth of administrative burdens on NIH grantees has been the topic of long-running complaint since.[9]

[8] Institutional grants were also a potential tool to spread the geography of funding, and to support infrastructure.

[9] Some of the costs of growing regulations around NIH research are borne by investigators, others by universities. The latter contribute to the "indirect" costs of research, another long-standing source of controversy, also a topic in many of the reports. See Azoulay et al 2025 for a history and analysis of indirect cost recovery policy.

3 Taking Stock: What Has SSF Research Told Us?

A number of themes have recurred through the history of NIH research funding: the value of research funding (for science, scientists, private innovation), concerns around the quality and reliability of peer review, top-down targeting versus investigator-initiated research, the organizational structure of the NIH, red tape, among others. Over the past two decades economists and the broader "science of science" community have sought to bring empirical evidence to many of these questions, though not all. This section takes stock of this work, to assess where we have and have not made progress. The literature is voluminous; the assessment below discusses representative articles and questions but is not intended to be a complete literature review.

3.1 The Value of NIH Funding

Early evaluations of the NIH in the reports surveyed above generally viewed NIH as good value for money, based on their impressions or descriptive data. Economists have examined the value and effects of NIH funding more rigorously. One approach simply relates NIH funding to subsequent outcomes. This was historically difficult to do, given the lack of classification of NIH funds into useful categories that could be mapped to other economic or clinical data. To answer the questions of the impact of NIH funding on pharmaceutical R&D (and in particular whether the two are substitutes or complements) Toole (2007) used data from the NIH CRISP database (the predecessor to the current NIH RePORTER database) to create a panel of NIH funding from 1981 to 1997 to seven medical classes, for which he also obtained pharmaceutical R&D data. Comparing industry research to the distributed lag of NIH funding, the study finds evidence of complementarity. In a similar vein, Blume-Kohout (2012) mapped CRISP data to the National Library of Medicine's Medical Subject Headings (MeSH) categories associated with diseases (67 categories) from 1975 to 2006, to number of drugs entering human clinical trials for the diseases, finding strong effects on early stage trials (Phase I trials) but no robust effects on late stage (Phase III) trials. Each study acknowledges causal identification is difficult since unobserved factors could drive both NIH and

private R&D incentives.

Another approach uses patent and citation data. For years bibliometric researchers have sought to link public-sector R&D funding to private patents using citations in patents to government supported publications (Narin et al. 1997). Sampat and Lichtenberg (2011) and Li, Azoulay, and Sampat (2017) link private sector patents to publicly funded research in two ways. First, "directly" linked patents that emanate in the course of a government-grant or intramural research. Second, "indirectly" linked patents that cite a paper acknowledging public sector research. Both studies find the magnitude of the "indirect" links to be substantially larger than the "direct" links (consistent with Fleming et al 2019). Li et al (2017), focused explicitly on the NIH, finds that while only 8 percent of NIH grants result in a patent directly, 31 percent generate articles cited by a private-sector patent.

A unique feature of the drug industry is that for drugs, in particular the small-molecule pharmaceuticals that comprise the majority of drug approvals, an administrative data source—the Food and Drug Administration's Orange Book—provides information on patents relevant to the products (Durvasula et al 2021). This allows researchers to link grants not only to patents, but products (and product sales, social benefits, etc). Sampat and Lichtenberg (2011), Li et al (2017), and Durvasula et al (2021) find the vast majority of links from drugs to NIH research are through indirect (citation) links rather than direct (Bayh-Dole) links, which has implications for a number of policy debates related to public patent ownership and drug prices (Sampat and Lichtenberg 2011; Ouellette and Sampat 2024).

The studies above are descriptive accounting exercises. Azoulay et al (2019) use the grant-publication-patent linkages to examine the effects of NIH funding econometrically. While the approach requires making assumptions on the meaning of citations (more on this below) it has advantages in letting the data reveal where and when the impact of NIH funding appears, not requiring specification of a particular lag structure or confining attention to effects within a particular disease. The paper examines patenting in areas "related" to NIH research, using the PubMed Related Articles (PMRA) algorithm to find patents citing articles related to NIH research, and

uses an IV strategy based on windfall funding (based on the institutional features of the two-stage peer review process, and data on application scores), finding that an additional $10 million in NIH funding generates a net increase in 2.3 private-sector patents. Notably, and related to discussions about targeting, rough calculations suggest about half of this effect comes from patents in diseases different from the funding Institute's main area.

The papers above use "front-page" citations from patents to publications. Bryan et al (2020) argue that in-text citations are likely better measures of true intellectual linkages, given their legal roles in patent prosecution. Azoulay, Clancy, Li, and Sampat (2025) link Marx and Fuegi's (2022) dataset on in-text citations to Orange Book patents for drugs approved 2000–2023, finding that 59 percent of the drugs have at least one in-text citation to an NIH grant. The specific focus was to assess the reach of a large cut to the NIH's budget, motivated by the 40 percent cut proposed in the FY2026 budget. The authors use data on scores for funded grants, finding 51.4 percent of the drugs have a patent citing at least one publication from an "at risk" grant, one that would *not* have been funded if the NIH were 40 percent smaller. And for about 12 percent of the drugs, 25 percent of cited publications would not have been funded with such a cut.

Overall these studies are suggestive of significant positive effects of NIH funding on private sector patenting, and links to drug development. It bears mentioning, however, that many of the value measures used in analyses like these are incomplete relative to the outcomes emphasized in the historical debates. For example, drugs are associated with, but not equivalent to health improvements, and many health outcomes NIH targets are beyond those that are traceable through patent-protected drugs. Even the focus on small molecule drugs, done because the Orange Book provides a link from grants-patents-products, is limited, given the growing importance of biologics in drug approvals and healthcare in general. Citation linkages underlying these analyses themselves require further validation.

Several of the papers above used data on scores from the peer review process, typically not available to independent researchers. These have also been used to examine outcomes of NIH training programs. For example, Jacob and Lefgren (2011) use these priority score data to look

at a specific type of NIH funding, the F32 postdoctoral fellowship, and researcher-level outcomes. Using a fuzzy RD design (instrumenting for grant receipt with score information) the authors find effects of getting the fellowship on publications and likelihood of being an active researcher. Scores data have also been prominent in the study of the peer review process itself, discussed in the following section.

In sum, relative to the impressionistic assessments in the early policy evaluations of NIH, economists have made considerable progress on assessing some aspects of NIH value, in particular on links to patenting, drug development, and human capital outcomes. NIH also supports research whose social, economic, and clinical value will not be captured by these measures. Expanding the scope of outcome measures and investing in validating existing ones are important for better answering questions around the return on investment for publicly funded medical research.

3.2 Peer Review

The NIH peer review process is another favorite topic in the economics of science. As noted above, there have long been questions about whether the process effectively screens for high quality research. The strongest evidence on this is from Li and Agha (2015). Using data on scores of funded grants, the authors find that even after controlling for indicators of applicant prestige, NIH first-stage peer review scores are predictive of several measures of scientific impact: publications, highly-cited publications, and patents.[10]

Economists have also studied the issue of bias in peer review. Ginther et al (2011) use data from an internal NIH database (IMPAC II) for R01 applications submitted between 2000 and 2006, to examine funding gaps by race and ethnicity. One of their findings is that African American applicants have a 10 percentage point lower likelihood of being funded, after controlling for a suite of covariates. A series of subsequent papers (by economists and others) have sought to explain this "Ginther Gap" including through more detailed controls (Ginther et al 2018), topic choice (Hoppe

[10]Fang et al (2016) re-analyze the data and suggest that while this is true across the range of funded applications, scores are not as predictive once one focuses within the narrow range of percentile scores near the more stringent paylines typical in recent decades.

et al 2019) and to understand where in the peer review process it emerges (Ginther et al 2018; Hoppe et al 2019; Lauer et al 2021).[11] A retrospective by Ginther (2021) concludes the gap is not primarily due to bias in peer review, but rather publication differences and other potentially structural features of the system outside of peer review.

A different type of bias, raised in the Fountain, Rogers, and Biomedical Research Panel reports, is that peer review has an "old boys club" aspect where researchers reward those they know. There is a fundamental tension: those that are in a field are most likely to know the field, but perhaps also biased toward applications in the field as well. Li (2018) tries to tease out expertise from bias using detailed information on study section rosters and applications (unfunded and funded) assigned to a study section. Leveraging institutional aspects of the review process, the paper finds applicants do benefit from drawing reviewers closer to their work, but there are informational benefits as well, which in the paper's assessment offset any bias.

Still another question raised in several of the reports above is around peer review and innovation. Using data on the vintage of medical terms associated with biomedical publications, Packalen and Bhattacharya (2020) find that NIH-funded articles are less likely to build on recent ideas, and this tendency has become more pronounced over time. The authors conclude NIH "has become more conservative over time, despite a variety of policies that [it] has implemented in the past 2 decades to reward innovative and high-risk proposals."[12]

A seminal paper indirectly bearing on the NIH peer review process and innovation is the Azoulay, Graff Zivin, and Manso (2011) comparison of HHMI (Howard Hughes Medical Institute) investigators to matched NIH investigators. HHMI provides longer-term funding before renewal (and thus requires less paperwork), detailed feedback, and funds people not projects (allowing for course corrections and pivoting in the course of a grant). The authors find HHMI-supported researchers have more novel and highly-cited papers than the matched NIH-funded researchers

[11] Ginther et al (2017) also examine gender variables, finding the gap to be driven by race not gender.

[12] They suggest several potential mechanisms, including that NIH reviewers have become more conservative over time or less able to discern innovative research; the fecundity of ideas may be changing; and bureaucratic limits on grantees' ability to pivot in the course of their work. An important limitation the authors acknowledge is that their analysis is done on publications, not applications or even grants.

though also more flops (papers that underperform relative to previous work). This paper has helped fuel calls for experimentation for "people not project" approaches at NIH and other funders as well, though the authors acknowledge that such flexibility may be less feasible in a large public bureaucracy than for a small private philanthropy.

None of these papers provides direct evidence that the NIH peer review process is itself the cause of the relative lack of innovativeness, and through what mechanism(s). This may reflect difficulty in getting access to unfunded application data. Researchers have also probed features of the peer review process that could limit innovation using public data. The score assigned to an application (after first stage review) is based on the mean across reviewers. Carson et al (2024) ask whether that approach may be ignoring useful information about dissensus. Using discrete choice experiments to assess the weight scientists place on the mean versus variance of experimentally generated scores assigned to grant abstracts, the authors find that (conditional on mean score) scientists also prefer grants with higher variance scores. The authors argue that under the assumption that higher variance projects are correlated with innovativeness (not testable because of lack of actual score data), the current aggregation rules may contribute to conservatism in NIH peer review decisions.

Greenblatt and Azoulay (2025) also look at riskiness and peer review using funded grants, looking at how renewal decisions are affected by measures of disruptiveness, pivoting from previous work, tail outcomes, and standing out in the field. The authors find risky research is penalized in renewal, especially for new investigators.[13]

Taken together the papers provide a nuanced view of NIH peer review. There is some correlation between scores and standard outcome measures. There is limited direct evidence of bias in the process itself, and even the work suggestive of bias from close links between reviewers and applicants suggests these links provide useful information. Several studies are suggestive that the NIH process discriminates against innovative research but, lacking detailed application level score data or true experiments, there is no smoking gun.

[13]The authors distinguish between risk and novelty (new combinations of ideas), finding that unlike risky research their measures of novel research are positively related to renewal.

3.3 Targeting and Management

Another set of questions in the historical debates that has drawn attention in the academic literature is how well the allocation reflects disease burden, and adjacent questions of whether more targeted research mechanisms are needed in the allocation process (whether through first stage peer review, second stage IC review, or elsewhere in the process) to achieve better alignment with health priorities or other aspects of NIH's mission. Implicit in these debates are questions about the efficacy of investigator-initiated versus more targeted mechanisms or approaches, on which there has also been some empirical work.

3.3.1 NIH Funding and Disease Burden

In 1996 Gary Becker observed in an op-ed: "Spending for research on major diseases, such as cancer, heart attacks, and AIDS, arouses intense emotions because life-and-death choices are involved. Unfortunately, the distribution of funds among diseases deviates greatly from the socially most desirable allocation that would give the greatest overall benefit" (Becker 1996).

This argument echoed the longstanding concerns that NIH funding allocations were not responsive to U.S. disease burden, perhaps because shaped by politics (powerful disease lobbies), or perhaps because the peer review process is optimized for science and scientists rather than health (IOM 1998).

A number of papers have looked at the extent of NIH responsiveness to disease burden empirically. Until the 2000s, it was difficult to get reliable data on the disease categories associated with specific NIH grants. Gross et al (1999) used selected data that had been reported to Congress, finding mixed evidence of responsiveness, depending on the particular burden measure used (deaths, disability adjusted life years, costs, global burden). Lichtenberg (2001) used CRISP data and thesaurus terms to examine these issues, also finding mixed results depending on which specific disease burden measure was used. These types of analyses were dismissed by NIH leadership, in part due to noise and non-comparability in how NIH assigned grants to disease areas in its publicly-reported figures (Varmus 1999).

It was not until 2008 that the NIH stood by its mapping of grants to diseases with the creation of the Research, Condition, and Disease Categorization (RCDC) system, developed in part due to Congressional scrutiny and the IOM (1998) report (Sampat 2012). Sampat et al (2013) linked the RCDC data to two measures of disease burden (deaths and hospitalization), finding strong positive correlations for each, though noting that the mechanisms of responsiveness (within or across Institute level funding allocations) was unclear, though there was some evidence that more applied grant mechanisms (RFAs, clinical research) are more responsive.

Economists and NIH leaders have also noted that in addition to disease burden, an optimal allocation should be responsive to scientific opportunity considerations (Zeckhauser 1967; Lichtenberg 2001; Varmus 1999; See Sampat 2012 for a review). One could go further, adding that spillovers to other disease areas should be a factor, along with consideration of market failures.

Within the NIH there are different types of funding mechanisms, some more targeted to health and other considerations than the rest. This relates to a classic debate in science policy about the relative value of more fundamental versus more applied funding or in medicine on basic versus clinical science (Comroe and Dripps 1976).

Several of the papers surveyed above on the effects of funding also examined the relative efficacy of types of funding. Toole (2007) finds stronger effects of basic research than clinical research funding on pharmaceutical R&D. Li et al (2017) examine proxies for "basicness" of research—whether a grant is disease-oriented, patient-oriented, solicited through a request for applications (RFAs, more targeted than investigator-initiated research)[14] and complexity of the model organism studied. Across the measures, they find no systematic relationships between these indicators and whether a grant generates or is cited by a patent, concluding the basic/applied distinction (or at least these specific measures) "may not be so useful in thinking about what types of research funding is

[14]RFAs are also studied extensively by Myers 2020, to estimate the elasticity of science, how much it costs to shift scientists' research direction. While his main focus is on estimating switching costs (which he finds to be large) he also finds that RFAs are more productive than investigator-initiated grants, though this difference disappears when controlling for scientist and field effects. Hegde and Sampat (2015) pick up on themes in the 1998 IOM report, assessing how disease group lobbying affects Congressional earmarks for a disease, and how those earmarks affect NIH RFAs and other funding.

more productive."[15]

The targeting literature has made more modest progress than scholarship on value of research or peer review. While a number of studies suggest NIH funding broadly tracks disease burden, there is limited attention to scientific opportunity or market failure in these analyses, and the results are sensitive to the specific burden measure used. On research targeting: despite some evidence that available measures of basic and applied research don't matter much for outcomes, big questions that have recurred since Bush-Kilgore around the value of different types of programs (fundamental vs targeted; more or less discretion) and NIH mechanisms are underexplored.

3.4 Less Well Studied Topics

Other recurring topics from the NIH's history have been less studied. These include administrative discretion in funding decisions, the effects and effectiveness of large targeted programs, the NIH organizational structure, the public-private division of labor, red tape and administrative burden, and the geography of funding.

Administrative discretion. A major topic in NIH research policy has been whether the second stage of review (meant to prioritize mission goals) rubber-stamps the first stage (meant to gauge merit), or instead exercises meaningful discretion. Many economics papers implicitly assume limited second stage discretion (Azoulay et al 2025; Azoulay et al 2019; Jacob and Lefgren 2011). Ginther and Heggeness (2020) show that for some grant mechanisms (including the F32/NRSA fellowship they study) discretion is common, and note that some Institutes report routinely using discretion. Ginther and Heggeness study the effects of peer review scores vs discretion on applicant careers, finding applicants chosen through discretion have worse career outcomes than those chosen through peer review scores. Extending this approach to other outcomes (including mission related outcomes) and other grant types is a promising line of research that would help speak to long-standing policy debate around the role and effectiveness of the second-stage of peer review.

[15] Focusing on inputs not outputs, Packalen and Bhattacharya (2020) find that NIH-funded clinical research is less likely to be "edge science" than basic research.

Large targeted programs. Also on the topic of targeting, there have been limited empirical examinations of large programs including the War on Cancer, Total Artificial Heart Program, and Framingham Heart Study (Sampat 2012).[16] Beyond NIH, the Department of Defense's Congressionally Directed Medical Research Program is another targeted approach to funding biomedical research that may be informative, insofar as it explicitly seeks to distinguish itself from NIH peer review process (NAS 2016). While previous research on the World War II Committee on Medical Research (Gross and Sampat 2025) provides one data point on the effectiveness of a top-down more directed approach to funding, extending to other such programs in the history of biomedical research funding would provide a stronger and more representative evidence base. This is especially important given resurgent interest in more "mission-oriented" funding in medical research and beyond (Sampat and Cook-Deegan 2021; Foray et al 2012).

Organizational structure of the NIH. Since 1955 major external evaluations have raised questions about expansion through categorical institutes. One set of questions, related to the discussions about targeting more generally, is whether categorical structure compromises scientific freedom and attendant benefits of non-directed research. Separately, several reports (most explicitly IOM 1984 and NRC 2003) note worries that the proliferation of categorical institutes may create unnecessary overhead, and limit cross-disease spillovers and the ability of the agency to coordinate and set priorities effectively.[17]

Economists have generally ignored the organizational structure of the NIH (though ICs have a role in identification in some papers, e.g. Blume-Kohout, Kumar, and Sood 2015; Azoulay et al. 2019). Assessing the effects of new ICs on funding and cross-disease spillovers may be a promising line of research. To the extent there is a strong first-stage effect, new ICs could be shocks to assess the value of NIH funding. Using the "narrative history" approach of Fieldhouse and Mertens (2023) to classify new ICs as exogenous to scientific opportunities (Congressionally created, over

[16] See however Rezaei and Yao (2024) on the NIH Brain Initiative.

[17] On the other hand it is worth noting that Stokes (1997), in his influential book *Pasteur's Quadrant*, holds up the NIH approach as the epitome of how to fund use-oriented basic research, relying on Institutes to funnel allocation to uses and relying on scientific peer review to make allocation choices within priorities. If other strong targeting or priority setting mechanisms are absent within the peer review process (IOM 1998), this may be a significant feature.

the objections of NIH leadership) is one promising approach, feasible given the small number of ICs and the availability of rich narrative histories and Congressional deliberation surrounding the formation of most.

Public-private sector division of labor. Another theme in early reports in particular was around the relationships between NIH and other funding sources, including around whether NIH research crowds out private or philanthropic efforts. This discussion, most pronounced in reports written as the modern institutional structure was being developed, has resurfaced in the context of concerns about recently proposed large cuts to the NIH budget: if not the public sector, will firms and philanthropies fill the gap?

Several of the studies above (Toole 2007; Azoulay et al 2019) suggest that NIH funding is a complement rather than a substitute to private sector biomedical research, but a stronger knowledge base is needed. More information on the topics that public and private sector focus on (including whether the public sector is more focused on market failures, less focused on appropriability; see e.g. Budish et al 2015, Babina et al 2023), also a topic raised earlier in the context of targeting and optimal allocation, would be more generally useful to better understand the ecosystem in general and the respective roles of different funders. And given interest among economists and policymakers in whether NIH research has become less focused on "edge science" over time (Packalen and Bhattacharya 2020), even descriptive data on the roles of different types of funders (private sector, philanthropy, non-U.S. funders) in funding this or other measures of disruptive research would also be valuable.

Red tape. The original social compact for science promised limited paperwork and reporting, in the name of scientific autonomy and freedom. This was usurped following the post-Fountain rise of regulations. By all accounts the volume of regulations has increased sharply since (COGR 2024). Numerous accounts point to administrative burden as a tax on science, which may privilege grantsmanship over innovation, and contribute to the rise of incrementalism in science, though to my knowledge direct evidence is lacking. Though the Azoulay et al (2011) HHMI study provides

suggestive evidence that less burdensome applications can generate more novel research, HHMI also offers other features (including freedom to pivot, detailed feedback). Moreover, as the authors emphasize, generalizing from a small private foundation like HHMI to a large public bureaucracy like NIH is fraught. Assessing the impact of specific large shocks to regulations is one way forward, as may be randomized experiments limiting paperwork and reporting requirements, to move beyond anecdote in debates around red tape reform.

Complementary qualitative and historical research on the causes of red tape at NIH would also be valuable. An important question is what share of current regulations are what Bozeman and Anderson (2016) call "rule-inception" red tape, flawed from the start versus "rule-evolved" red tape (emerging in implementation or change of rules, or rules that become less functional over time). Bozeman and Anderson (2016) also usefully introduce the concept of stakeholder red tape: rules that are functional for some stakeholders but not others. For example, some security related research restrictions could be "red tape" from the perspective of promoting health and economic growth, but functional rules from a national security perspective. The COGR running list of current regulations on universities (COGR 2024), or the evolution of the regulations at NIH[18] could be a good place to start.

The geography of funding. The geographic concentration of NIH funding (and questions of how this relates to geographic concentration of peer review) have resurfaced periodically as issues of policy concern. In the early 1990s Congress created the Institutional Development (IDeA) program at NIH (modeled after the Program to Stimulate Competitive Research EPSCoR) to try to broaden the geographic distribution of funding, though the scale of this and related programs remains small (Schaller 2024).

While there is a large economics literature on how geography mediates the effects of academic research (see the Jaffe chapter for some discussion), there is limited empirical work on efficiency-equity tradeoffs in NIH funding. A notable exception is a recent working paper by Chandra and Xu (2025) that finds strong evidence of positive institutional and location effects in scientific

[18] https://grants.nih.gov/policy-and-compliance/notice-of-policy-changes

productivity, and concluding that if efficiency were the only goal of policy funding scientists at the most productive institutions (all else equal) could be reasonable policy.

There are also dynamic considerations and related questions of political economy. Broadening funding in the short run could entail an efficiency loss, but expand capacity in the long run. It could also broaden the political base for science support, as Gruber and Johnson (2019) argue, and in doing so help build a more resilient social contract for biomedical research going forward. These topics, too, are ripe for future research.

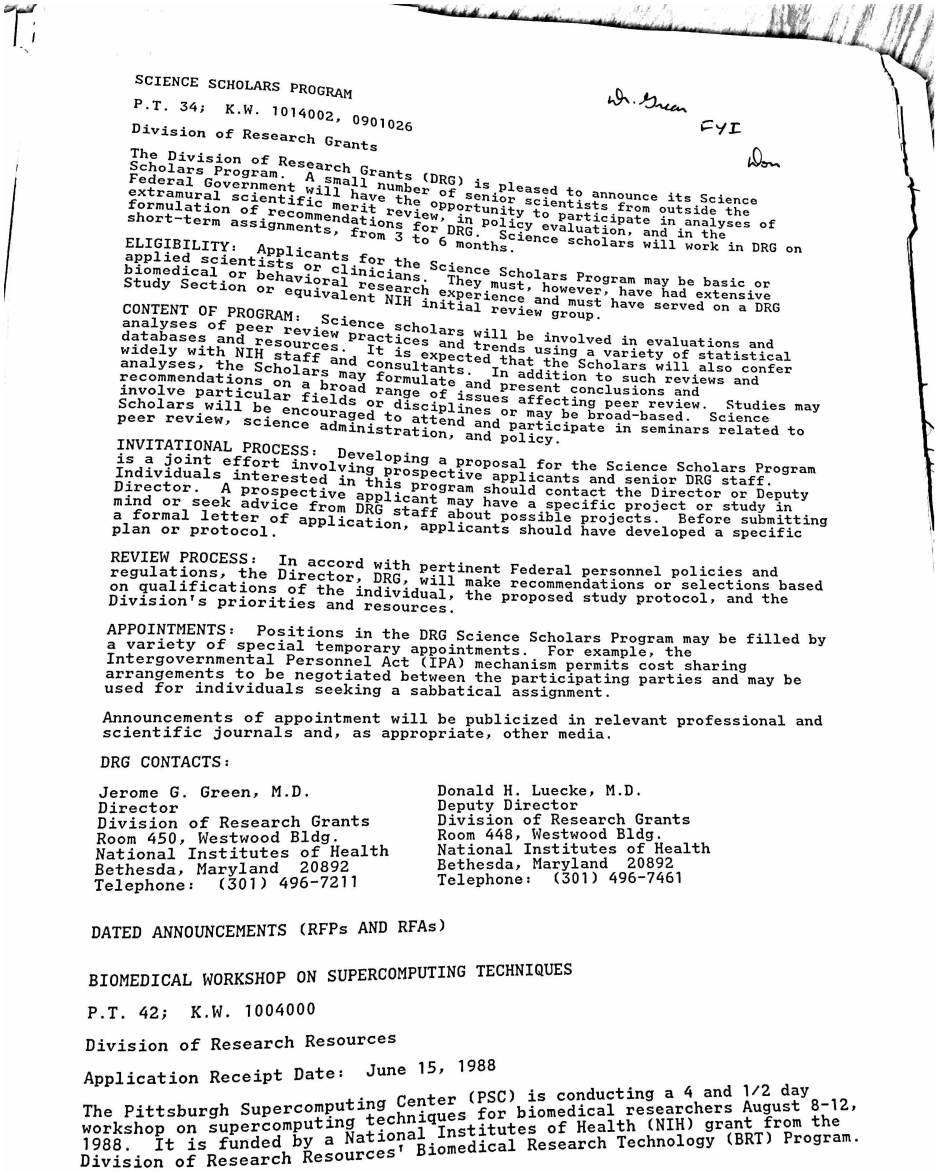
4 Conclusion

The need for better science of science funding at the NIH has been recognized for decades. For example, after his storied tenure as NIH Director, James Shannon (then at Rockefeller University) writing in the context of Nixon administration cuts to and criticisms of NIH, bemoaned the state of discourse as being prone to "after-the-decision exchange of biases" rather than "a search for sound solutions" through "open and dispassionate examination." He called for "a carefully selected group of thoughtful professionals from within the university community" to undertake studies that would develop "a coherent philosophical base for the nation's biomedical activities" (Shannon 1975).

As discussed in Sampat (2023) various internal reports on peer review at NIH have also emphasized the importance of testing changes to the process before implementing them. One of the GPRST (1976) recommendations was that NIH design an experiment around support for "speculative, high risk research proposals" part of a broader effort to assess the allocation process. The NIH director responded such an experiment would be difficult for "definitional problems" alone, deferring any action until better methodology for such an assessment became available.

Jerome G. Green, the Director of the NIH's Division of Research Grants (which later became the Center for Scientific Review) from 1986 to 1995, was a strong proponent of experiments, remarking "It is regrettable that many who go from conducting research to administering research lose their fondness for careful experimentation and tend to accept their intuitions about the process

of review." Green set up a short-lived Science Scholars program to bring in academics for 3–6 month sabbatical stints at NIH to evaluate peer review (in consultation with NIH staff, and using internal data) but the eligibility was restricted to "basic or applied scientists or clinicians" who had served on NIH study sections.



SCIENCE SCHOLARS PROGRAM

P.T. 34; K.W. 1014002, 0901026

Division of Research Grants

The Division of Research Grants (DRG) is pleased to announce its Science Scholars Program. A small number of senior scientists from outside the Federal Government will have the opportunity to participate in analyses of extramural scientific merit review, in policy evaluation, and in the formulation of recommendations for DRG. Science scholars will work in DRG on short-term assignments, from 3 to 6 months.

ELIGIBILITY: Applicants for the Science Scholars Program may be basic or applied scientists or clinicians. They must, however, have had extensive biomedical or behavioral research experience and must have served on a DRG Study Section or equivalent NIH initial review group.

CONTENT OF PROGRAM: Science scholars will be involved in evaluations and analyses of peer review practices and trends using a variety of statistical databases and resources. It is expected that the Scholars will also confer widely with NIH staff and consultants. In addition to such reviews and analyses, the Scholars may formulate and present conclusions and recommendations on a broad range of issues affecting peer review. Studies may involve particular fields or disciplines or may be broad-based. Science Scholars will be encouraged to attend and participate in seminars related to peer review, science administration, and policy.

INVITATIONAL PROCESS: Developing a proposal for the Science Scholars Program is a joint effort involving prospective applicants and senior DRG staff. Individuals interested in this program should contact the Director or Deputy Director. A prospective applicant may have a specific project or study in mind or seek advice from DRG staff about possible projects. Before submitting a formal letter of application, applicants should have developed a specific plan or protocol.

REVIEW PROCESS: In accord with pertinent Federal personnel policies and regulations, the Director, DRG, will make recommendations or selections based on qualifications of the individual, the proposed study protocol, and the Division's priorities and resources.

APPOINTMENTS: Positions in the DRG Science Scholars Program may be filled by a variety of special temporary appointments. For example, the Intergovernmental Personnel Act (IPA) mechanism permits cost sharing arrangements to be negotiated between the participating parties and may be used for individuals seeking a sabbatical assignment.

Announcements of appointment will be publicized in relevant professional and scientific journals and, as appropriate, other media.

DRG CONTACTS:

Jerome G. Green, M.D.
Director
Division of Research Grants
Room 450, Westwood Bldg.
National Institutes of Health
Bethesda, Maryland 20892
Telephone: (301) 496-7211

Donald H. Luecke, M.D.
Deputy Director
Division of Research Grants
Room 448, Westwood Bldg.
National Institutes of Health
Bethesda, Maryland 20892
Telephone: (301) 496-7461

DATED ANNOUNCEMENTS (RFPs AND RFAs)

BIOMEDICAL WORKSHOP ON SUPERCOMPUTING TECHNIQUES

P.T. 42; K.W. 1004000

Division of Research Resources

Application Receipt Date: June 15, 1988

The Pittsburgh Supercomputing Center (PSC) is conducting a 4 and 1/2 day workshop on supercomputing techniques for biomedical researchers August 8-12, 1988. It is funded by a National Institutes of Health (NIH) grant from the Division of Research Resources' Biomedical Research Technology (BRT) Program.

2

Figure 4: Announcement of Science Scholars Program

Going forward, institutionalizing a spirit of experimentation and learning at NIH could help inform the process, including robust institutions (with funding) for collaboration with economists

and the broader social science community with experience in evaluation. For some of the big questions, especially around peer review, infrastructure for randomized evaluations could yield large dividends.

It is striking from the review above how much of the progress in academic research on the NIH has benefitted from internal data on unfunded applications and scores. To date, these have been available only to select researchers. Finding ways to make IMPAC II data more broadly available to researchers could stimulate a large body of academic research on topics of interest to the agency, just as publication of non-granted patent application data helped unleash research on the USPTO and its outcomes. This may involve revisiting the costs and benefits of legal and privacy related concerns around publishing unfunded applications which are now over 50 years old and may no longer be relevant (GPRST 1976).

In taking stock, we can also ask what more economists studying science policy can do. Above I flagged a number of topics that I view as neglected and ripe for academic research, including research on administrative discretion, large targeted programs, NIH organizational structure, the public-private division of labor, red tape, and the geography of funding. Beyond that, more validation studies of existing measures (citations as measures of influence; bibliometric measures of "edge science" novelty riskiness) would be valuable, as would extending NIH health impact measures beyond small molecule drugs, where we have data, to other types of technologies and health outcomes. A broader set of measures and validation of them is crucial before we can make confident claims about the social returns to biomedical research spending, and more progress on the myriad other issues discussed above. In general, ensuring the outcomes we measure (and policy targets) are in fact valuable to taxpayers, or suitable proxies for those outcomes, may help forge a more robust social contract for science going forward.

All of this is perhaps more important now than ever, as the postwar social contract faces its strongest challenges in decades. In rethinking and rebuilding NIH amidst what some view as a crisis in science[19] there is a generational opportunity to do so with a commitment to evidence and

[19] See e.g. the 2025 *STAT News* series "American Science, Shattered"

learning rather than intuition.

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