The curriculum reform movement that began in the late 1950s is widely viewed as the result of a broadly academic effort to restore disciplinary rigor to education in the United States. Much of the work that went on developing the new curricular materials and educational approaches, however, is better understood as an experiment in applying innovative research and development techniques perfected by scientists during World War II. This essay traces the development of these newer methods of scientific analysis and examines how they were imported from the military research programs to the field of education by a select group of physicists centered around Jerrold Zacharias at the Massachusetts Institute of Technology. It is to these physicists, I argue, that this curriculum reform movement owes its fundamental operational characteristics, its conception of the problem of education, and the means of its solution in America at midcentury. Exploring the historical origins of these reforms reveals a good deal about how scientists of the time framed the portability of military techniques, organization, and administrative models of action and provides insight into the current emphasis on technique and performance that has come to characterize United States educational policy.

The willingness of educators and the public to draw on, or accede to, the intellectual authority of science in the development of educational systems and school curricula is well documented in the history of education. The classic example is found with the social efficiency experts Franklin Bobbit, W. W. Charters, and David Snedden, who early in the twentieth century labored to bring schooling into line with the scientific management ideas of the time. Their influence, in the forging of a functional curricular ideology designed to match schooling to the life needs of students and the community, survives to a large extent in current educational practices. But, although the advocates of Taylorism and scientific management have undoubtedly made a lasting impression on the contemporary nature of schooling, these early century reformers by no means exhausted the potential of science (or practices advanced in the name of science) to inform education.
Nor did they represent the last word on science itself, which like any social practice is historically variable. Looking back over the last century, one finds that perhaps the most significant scientific influence on American educational policy can be traced not to the factories of Detroit or Gary, but rather to the wartime laboratories of Berkeley, Los Alamos, and MIT.¹

The place to begin exploring the impact of these labs on education is with the National Science Foundation (NSF)- and U.S. Office of Education (USOE)-funded curriculum projects of the late 1950s and 1960s. All of these projects, from the Physical Science Study Committee (PSSC) to Project English, shared a set of defining characteristics: they adopted a systems view of education; sought to incorporate the latest technology into the classroom; were generously supported by the federal government; and were organized and developed by small, interdisciplinary groups of subject-matter specialists who were newcomers to education reform. On the surface, these characteristics appear unexceptional—especially to those familiar with the standard history of this period.² All seem simply part of the government-assisted reassertion of academic authority in precollege education that emerged in the early 1950s and refocused the country on disciplinary content as the most desired learning outcome. But these very characteristics—the origin within the academic elite; the focus on disciplinary knowledge; the nurturing by the federal government; and the systematic, technology-based approach to reform—are, from a historical perspective, the most interesting attributes of these projects. For nearly all of these were inextricably bound up with the scientific community of the time—a community shaped by the unprecedented circumstances in which it found itself at the end of the Second World War.

To understand the nature of the changes that took place, one must look to the individuals involved. The educational reforms of the 1950s and 1960s were not the result, as is often implied, of simply a resurgent interest in public education by the nation’s academics, nor did they originate primarily within the field of cognitive psychology as some histories tell us.³ Such accounts have served only to obscure the fundamental nature of what took place. Rather, the reforms of this era were designed and implemented by a small cadre of scientists—specifically, the individuals who had made their way to the inner circles of federal policy making as a result of their service providing national security advice to the government during the Cold War.⁴ Not surprisingly, these scientists, who moved from their work on defense-related projects to the problems of education, brought with them particular intellectual skills and technical methods—skills and methods that had proven their worth during the World War II and increasingly defined the nature of scientific research thereafter. In the eyes of both scientists and policy makers, there seemed to be no limit to the power of these analytical tools to solve nearly any problem.⁵
This essay will trace the development of the new wartime institutional arrangements and methods of scientific analysis and examine the historical question of how they were transferred almost seamlessly from the military research programs of the postwar period to the field of education by a select group of Cambridge-based physicists centered around Jerrold Zacharias at the Massachusetts Institute of Technology (MIT). It is to these physicists and their experience in weapons research and development that this curriculum reform movement owes its fundamental operational characteristics, its very conception of the problem of education, and the means of its solution in America at midcentury. The models of scientific research on which reformers such as Zacharias drew had a profound influence on the subsequent course of educational reform in nearly all the traditional disciplinary subjects. This socioinstitutional context—the pervasive influence of the military-industrial-academic complex—has surprisingly gone largely unnoticed in the history of education literature.

My examination of this reform movement from the perspective of techniques provides more contemporary lessons as well. It brings to light the highly permeable nature of our educational systems and institutions, at least at the policy level. It illustrates the susceptibility of education reform to the prevailing social forces and reigning political and technological commitments of the day. Moreover, the emphasis on technique—on the curriculum as “instrument”—that epitomized the scientists’ approach has, in many ways, become deeply embedded in our current view of education research and reform, as is evident in the current emphasis on standards, testing, and systemic reform, among other trends. By their very design, such approaches to teaching children have precluded ways of thinking about education that embrace uncertainty, that encourage creative inner direction and growth, that enable critical self-examination. All is geared instead to performance, to external goals and direction, to efficiency within the system. It is this that appears to comprise the most important legacy of postwar educational reform in the United States. We would do well to understand its origins.

THE BIRTH OF BIG SCIENCE

By all accounts, World War II radically transformed the conduct of scientific research in the United States. Scientists, responding to their sense of duty, offered their technical expertise and training to the military in its time of need and quickly demonstrated their inestimable value to the Allied war effort. The Manhattan Project to build the atomic bomb at Los Alamos was certainly the most heralded of the new technological contributions made by the scientific community and, perhaps, the most definitive example of the value of scientific expertise to the state. Others included
the proximity fuse developed at Johns Hopkins, radar developed at MIT, and solid fuel rockets engineered at Cal Tech’s Jet Propulsion Laboratory—all provide examples of the new kind of “big science” research and development model that the war had wrought. These large-scale projects were not externally orchestrated by the military, nor were they simply an extension of past scientific practices to the problems of war. They were an altogether innovative approach to research that emerged from the amalgam of military needs, theoretical advances, and the particular conditions and constraints under which scientists were put to work.8

Identifying the essential nature of these new models of scientific research has been problematic for historians of twentieth-century science. “Big Science” has been the label of choice. But, as Bruce Hevly has aptly noted, “the phrase is conveniently murky, appropriate for an activity that few can define or describe precisely but many feel able to recognize on sight.” One might say that whereas science in the prewar years could best be described as small-scale research, pursued with private funds by solitary investigators in the pursuit of knowledge for its own sake, the new science was increasingly driven by interdisciplinary teams of researchers that marshaled both science and engineering expertise in the service of goal-directed projects possessing social or political significance of national scale. Such large-scale projects required high levels of government funding, which was dispensed primarily through government contracts with scientists at leading universities in order to preserve the intellectual autonomy and the preferred working environment of researchers. Institutional manifestations of this trend toward large-scale research facilities included the Radiation Laboratory at Berkeley (site of the first particle accelerator), along with the postwar facilities at Brookhaven and Stanford.9

This new type of scientific work, however, was not defined solely by technical accomplishment (such as that demonstrated by the atomic bomb) or by mere physical size (as in the case of the various particle accelerators that sprang up across the country). The most significant factor in the widely acknowledged success of these research teams is to be found in their broad-based, analytical approach to solving the complex problems encountered in modern technological combat—problems that involved not just the satisfactory performance of a given piece of technical apparatus, but also consideration of the most efficient use of that apparatus within the larger context of other existing weapons technology and of the combatants themselves. It was, as one analyst described it, the ability of new technological apparatus “to work hand in glove with other systems” both human and material that made them so powerful.10 It is this characteristic—the efficient integration of emerging technologies into goal-directed systems—that is the most significant in terms of the subsequent impact on education reform.
Of all the wartime projects in which scientists were involved, the one that epitomized this analytical approach was the Radiation Laboratory established at MIT in 1940. The Rad Lab as it was called—named to suggest militarily inconsequential (or so it was thought at the time) nuclear research similar to that occurring at Berkeley—was the site for the development of radar. By 1942, teams of physicists and engineers there had perfected a plane-mounted microwave radar surveillance system capable of detecting enemy submarines at sea. Scientists, recognizing that how the technology was used in combat was crucial to its ultimate effectiveness, pushed hard for a greater role in the tactical decision-making process. They were able to demonstrate mathematically to navy commanders that the probability of pinpointing German submarine locations and removing the offensive threat they posed was much greater using radar-equipped aircraft than with any other method. Hunter-killer groups outfitted with the microwave devices and following the instructions of the scientists dramatically increased the number of successful strikes on German U-boats.11

The use of mathematical tools of probability and statistics to improve the efficiency of weapon systems in combat was characteristic of the new field of operations research (OR), and was the seed of the new analytical techniques that emerged from the war. Developed by the British in 1937, OR concerned itself with optimizing weapon effectiveness by altering the variables of deployment based on quantitative studies of past performance. This approach was quickly adopted by the American scientists, as in the case of the antisubmarine group, and was further refined to a point where such analyses provided a clear military advantage to those willing to heed such advice. Following the navy’s successes, the other services rushed to establish their own OR advisory groups. “By V-J day,” observed historian Daniel Kevles, “civilian scientists were in vogue as strategic and operational advisors to a degree without precedent in the annals of American military history.” They had become particularly adept at using their technical and analytical expertise to optimize the performance of any manner of complex system to meet carefully defined military objectives.12

SUMMERS AT MIT

MIT, the performance leader in the development of defense systems, was a primary beneficiary of the evolving relationship between scientists and the state. Taking advantage of their wartime experience in research planning and organization, scientists and Institute administrators established a number of new interdisciplinary research labs devoted to the emerging areas of electronics and nuclear science.13 Not surprisingly, the transition to peacetime research was remarkably smooth. Historian Peter Galison remarked that the “war laboratories . . . provided the managerial models, the techni-
cal expertise, and even the personnel for the establishment of postwar collaborative work.” In addition to continuities in organization and personnel, nearly all the labs extended the analytical techniques and operational approaches to problems and research questions that had been perfected during the war. At MIT, in large part due to the work of Jerrold Zacharias, these techniques found an enduring home—as well as an expanding range of problems to which they would be applied.

As the country settled into the growing Cold War, operations research—freed from the immediacy of worldwide conflict—increasingly gave way to systems engineering analysis. In contrast to OR, which focused on improving the efficiency of existing systems, systems engineering broadened its analytical scope to consider not just the optimum performance of a given human/technological system, but the array of possible alternatives that might be created using existing or nascent technologies. Scientists at MIT, in other words, had the freedom to design new systems from scratch without the constraints OR analysts faced. The potential of this approach to meet increasingly complex military and even social needs was explored in a variety of institutional settings beyond MIT as well—from the Air Force think tank RAND, which among other things exhaustively calculated multiple thermonuclear war scenarios, to the National Research Council’s Committee on Operations Research, which called for the application of systems thinking to nonmilitary areas such as traffic management and industrial retooling.

It is important to note that these newer scientific approaches were historically distinct from Taylorism and its allies. While scientific management was directed toward improving the efficiency of ongoing manufacturing processes in business and industry, the systems approach was applied to more finite, R&D type projects. It employed heterogeneous assemblages of experts, who variously drew on the research base of their respective disciplines in fashioning innovative solutions to the problems set before them. The principal players were, of course, the physical scientists; and it was the integral connection they made between the systems approach and “hard”-science theories and research methods in fields such as physics that endowed their work with legitimacy. Among policy analysts, it was widely held that this approach could only be successfully applied by those with proper scientific training. According to one report, its power lay in the scientist himself, in his “ability to use more refined and powerful numerical techniques, . . . to penetrate to the essential variables in a situation, and to relate phenomena to them in a simple and cogent manner.” The public prestige scientists had accumulated as a result of their wartime successes was vast. It was this prestige that they traded upon as they extended their techniques beyond their specialized domains. The systems approach with its science-based authority thus “marked an entry point for natural scien-
tists, mathematicians, etc., into the traditional preserve of managers and social scientists.”

Over the course of the postwar struggle with the Soviets, a variety of government agencies charged with maintaining U.S. national security came to depend on the systems expertise scientists had developed. One of the more interesting conduits for channeling such advice was the summer study—an ad hoc consultation arrangement developed at MIT. The Atomic Energy Commission had convened the first summer study in 1948, Project Lexington, in the Massachusetts township of the same name for the purpose of examining the feasibility of nuclear-powered flight. Over the next eighteen years, well over a dozen intensive studies related to defense strategy, tactics, and technology were put together—all were essentially system studies, the recommendations of which significantly influenced various defense projects and programs.

Though Project Lexington was the first, the summer study that demonstrated the real effectiveness of the systems perspective in military consultation was Project Hartwell. Hartwell was commissioned by the navy in 1950 to study the problem of Soviet submarine detection. It was led by Zacharias, who, drawing on his experiences at the Rad Lab, immediately redefined the scope of the study. The real problem the navy should be interested in, Zacharias insisted, was not submarine detection, but rather the security of overseas transport. The final report, issued after three months of intense work, contained a range of recommendations related to this broader question of the navy’s mission in light of the integrated systems involved. The report had an immediate impact on naval programs. The Chief of Naval Operations was impressed enough to refer to it as the “bible of undersea warfare.”

A retrospective appraisal of summer studies completed by the navy some years later found the Zacharias-led Hartwell to have set the pattern “in matters of approach, conduct, and impact . . . for the studies of the next ten years.”

The military, however, was not the only group seeking the systems expertise of the scientists in the early 1950s. Only days after the navy pulled out of MIT at the conclusion of Hartwell, the State Department moved in. Undersecretary of state James Webb, well aware of the services MIT had been providing to the defense department, had earlier approached MIT president James Killian about the possibility of establishing a study to examine the problem of getting information behind the iron curtain. These discussions led to the organization of Project Troy—named for the ancient city conquered with the aid of a wooden horse. This project is particularly useful in giving us insight into both the defining characteristics of these summer studies as well as the government’s willingness to engage problems that extended beyond narrow technical and military concerns to areas that bordered much more closely on social problems.
Troy was part of a comprehensive effort by the Truman Administration to counter the widespread propaganda offensive then being waged by the Soviet Union. The Cold War was increasingly becoming, in Truman’s estimation, an ideological battle—a “struggle, above all else, for the minds of men.” What was needed was a great “Campaign of Truth” that would make the United States known to the world as it really was, not as it was depicted by the enemy. Project Troy’s charge was to find the most effective means of disseminating the American point of view to the people of the Soviet bloc countries. In early November of 1950, the study participants settled into the same Lexington facility that housed Project Hartwell only a few months earlier and began to consider the various options for achieving this goal.

Following the pattern of earlier summer studies, Troy participants worked in an environment designed to provide for intense, distraction-free analysis of the problem at hand. They were encouraged to take advantage of the common on-site housing in order to maximize the number of informal contacts and conversations. These were enriched by the variety of disciplines represented by the participants—bringing together people with different backgrounds was essential to the stimulation of truly original thinking. The roster of Troy participants included, in addition to the physicists and engineers, psychologists, an anthropologist, and even a historian. But the foremost criterion for success was that the participants themselves be first-rate intellects, eminent in their given fields—“the most capable and well informed American scientists and specialists that can be found.” Of all the Troy participants, eight had worked on Hartwell and nearly all of these individuals had been key contributors to the Rad Lab during the war. Some might have argued that the small circle of experts limited creative thinking; but from Zacharias’ perspective, whatever the problem, these were simply the best minds available to do the job.

As with Hartwell, the Troy group broadened its consideration of the initial problem, in this case recasting it from one of information dissemination into that of waging what it termed “political warfare.” The final report reflected the now familiar systems point of view. Their key contribution, the project leaders explained, was “in the understanding of the strategic power of the several elements when combined as a well rounded and coordinated whole.” In this view, an information program by itself was “relatively useless.” Such a program, they argued, could only be effective when “designed as one component in a political ‘weapon system’.” The information weapon system the Troy group envisioned included the coordinated use of radio transmissions, balloons, motion pictures, library services, and even student exchanges as a means of spreading U.S. propaganda. The ultimate goal, according to the report, was to “bring the Soviet Union to its knees in a relatively short time without resort to widespread military action.”
The work undertaken by Project Troy gives a clear indication of the expanding scope of the federal government’s interests in national security and the willingness of federal agencies to become directly involved in non-military activities related to those interests during the 1950s. The apparent successes of the newer analytical models heavily influenced the government’s decision to cast its lot with the scientific community in nearly all things. Troy was modeled directly on Hartwell, which Killian noted provided “an admirable precedent.” This stepwise progression from military to what one might call less traditionally military topics seemed increasingly inevitable. In justifying this expansion of scope, one Troy participant pointed out that “U.S. armed forces have, for about ten years, utilized analytical facilities of science on a large scale in development of weapons and tactics. The military success of these models in the hands of American science was dramatically demonstrated during the last war when new weapons . . . provided a substantial measure of superiority to American arms.” He went on matter of factly, “As the power of analytical methods in the hands of American science has become understood it has been applied progressively to broader and more difficult problems.” The implications were clear, and policy makers could find no reason to dispute the claim that “this analytical technique is already a proven method that promises great benefits wherever it is adopted.”

As the report had noted, “the first application of these analytical techniques to international political problems had been Project TROY.” But the kinds of questions with which it dealt regarding the dissemination of information in Truman’s Campaign of Truth—“What is the nature of the people to whom the United States’ messages are and ought to be directed? What ultimate effects are desired? [and] What sort of messages ought then to be sent?”—were equally applicable to another emerging national security concern that became the focus of the Eisenhower administration in the early 1950s, the poor state of American science education.

EDUCATION AND NATIONAL SECURITY

The intensification of the Cold War and Eisenhower’s pursuit of defense strategies grounded heavily in sophisticated technological systems made it increasingly apparent that scientific expertise would be an important factor in the ongoing struggle with the Soviet Union. In the fall of 1953, a top-secret CIA report revealed that the USSR was “training a body of scientists and technicians which [was] increasing in size and quality and approaching comparability with that of the United States.” CIA director Allan Dulles’s warning to the President that “the Soviets may well pass us in the near future, particularly in those scientific fields which were most closely related to matters of national security” set in motion a series of events that ulti-
mately moved the federal government into the field of science education reform. The architect of that reform was the National Science Foundation (NSF), the only federal agency with the requisite expertise in training scientific manpower and the legislative authority to act.34

Establishing a program to improve science education in American schools, however, was no simple undertaking; indeed, it was a task fraught with administrative and political complexity. Public schools in the late 1940s and early 1950s were in a state of acute crisis as a result of surging student enrollments after the war, which exacerbated existing building and teacher shortages. These infrastructure problems were accompanied by waves of criticism leveled at the seemingly soft, even subversive, nature of the existing curriculum. And public suspicion of government interference in local affairs repeatedly surfaced as the issue of school integration moved up the federal agenda. The National Science Foundation had only recently been established in 1950 and only then after a protracted political struggle. Its institutional base as well as its prospects for continued Congressional appropriations were far from secure. Accordingly, Foundation director Alan T. Waterman urged a cautious approach in all its programs, especially any that intersected with the more volatile societal issues of the day. Thus NSF limited its initial activities in education to a relatively safe graduate fellowship program. The intelligence reports regarding Soviet educational trends, however, particularly the observation that the Soviets were placing “far greater and more consistent emphasis on scientific subjects in secondary schools than is found in the United States,” led Eisenhower’s national security advisors to pressure NSF into expanding its educational programs.35 In their view, the Soviet technological threat far outweighed any selfish concerns on the part of the Foundation to avoid political controversy.36

However reluctant Foundation officials might have been initially to involve themselves in educational reform, it was clear that those in the Eisenhower administration concerned with national security were confident in the scientists’ ability to get the job done. In fact, their wartime successes lent an aura of infallibility to nearly everything they did. It should certainly come as no surprise then that the problems of science education would be approached with the same analytical perspective scientists had previously used so successfully in the service of the military.

The leading officials at the National Science Foundation were heavily invested in the newer analytical techniques of the postwar scientific elite. Before his appointment as director of NSF, Alan Waterman had worked for both the National Defense Research Committee and the Office of Scientific Research and Development (OSRD), organizations responsible for coordinating the work of the scientific community during World War II. After the war, he had assumed the position of chief scientist at the newly organized
Office of Naval Research (ONR)—a key patron of postwar research that had helped cement the relationship between science and the military in the early days of the Cold War. He also served on the NRC Committee on Operations Research that advocated the extension of OR to nonmilitary problems. Harry C. Kelly, the Foundation’s assistant director for scientific personnel and education (the NSF division that would direct nearly all the Foundation’s education programs), experienced the new models of scientific research as a staff member at MIT’s Rad Lab during the war and was later the science section head at the ONR’s Chicago bureau.37

When Kelly and Waterman began to consider NSF’s role in improving science education, it was immediately apparent that, irrespective of the political sensitivity of the issue, no single program would do. The education of scientists was a long-term, multistep process that varied by educational level. The easy steps, such as providing fellowship money to ensure the training of the best prospective researchers, had already been taken. It was at the precollege level where things got complicated. One of the key difficulties they faced concerned the highly decentralized nature of the American educational system. Whereas in the Soviet Union with its monolithic state system of schooling one could simply command an increase in the numbers of scientists and engineers produced, in the United States one had to contend with, in the words of Commissioner of Education Samuel Brownell, “48 state governments and the school boards of some 59,000 school districts, as well as with a great variety of private and religious agencies.” Given this organizational structure, it was obvious to Waterman at least that “it is far harder to formulate and undertake . . . plans that involve changes in as huge and as complicated a system as that concerned with the education of millions of children.” But it was perhaps just such a complex system with which the NSF was best prepared to deal. The parameters were fairly well defined and the problem straightforward: given limited legislative authority and financial resources (at least initially), how might the educational system best be manipulated to improve the training of scientific and technical personnel in the United States?38

The sense of many in the scientific community was that the problem could be traced to the high school classroom. Members of NSF’s education division had felt specifically that “poor teaching [was] the heart of the problem.” And Waterman himself agreed that “all who had studied the problem felt the same way that, namely, the crux of the problem lay in the secondary schools and there the chief difficulty was in securing competent teachers of science.” The scientists recognized at the same time that, apart from drawing public attention to the primary factors they believed turned qualified students away from the teaching profession, such as low pay and even lower prestige, there was little they could do to alleviate the raw shortage of teachers. As for improving the quality of those who did
decide to become teachers, the Foundation was up against the deeply entrenched professional education establishment, which, although publicly cast as anti-intellectual and even un-American by vocal critics, tightly controlled the educational program of preservice teachers nonetheless. Given these constraints, Foundation officials decided that the most strategic approach would be to target existing science classrooms, where teachers were largely free from the demands of the education schools and students could be influenced most directly.

NSF’s first programmatic effort to improve the quality of practicing teachers came in the form of science training institutes, the first of which was held in 1954. These were designed to help teachers shore up what the scientists believed was a woefully inadequate knowledge of modern science—a condition which, they insisted, resulted from an overemphasis during college on courses in educational methods at the expense of course work in the sciences. Based on earlier private-sector programs sponsored by General Electric and Westinghouse, these institutes provided intensive summer instruction in the latest scientific advances untainted by educational theory. To accompany the institute program, NSF began considering ways to improve the instructional materials available to teachers in the nation’s schools. “Clearly the teacher is the key to quality,” Harry Kelly observed. But he also recognized that “the teacher must have the tools, the course content, the curriculum.” To this end, Kelly began looking to fund the development of supplementary teachings aids as well as studies directed at improving high school science curricula. Support for curriculum conferences became one of the key “points of attack” of the NSF education program in the spring of 1955.

Both the summer institute program and the curriculum studies initiative adopted the administrative contract model used by the OSRD during World War II. The advantages of this type of arrangement were clear. From the working scientists’ perspective, the contract model offered familiar ground and provided freedom from government interference. From the Foundation’s point of view, the contract provided an essential legal buffer that protected it from charges of interfering in the affairs of local school districts. When the issue was raised by one congressman during appropriation hearings early in 1955 that NSF’s education program might be perceived as an attempt by the federal government to control American education, the contract arrangement provided Waterman an easy out. He explained that it was not federal bureaucrats deciding what was good for science education, but rather the goal was to find out “what it is the scientists feel should be done. . . . what they think is the right thing to do.” The Foundation’s role was merely to facilitate these private initiatives. The initiatives looked upon most favorably, of course, were those that were in step with the new analytical approaches of the postwar period.
THE PHYSICAL SCIENCE STUDY COMMITTEE

In examining NSF’s approach to education, one could argue that the systems approach was more implicit than deliberate. The decisions Kelly and Waterman made suggest, at the very least, an appreciation of the systemic nature of the education problems the Foundation faced. The Physical Science Study Committee, the first of the NSF funded curriculum development projects, in contrast, provides us with an example of educational reform that not only explicitly employed a systems engineering framework, but wholeheartedly embraced all aspects of the new patterns of postwar scientific research—from its administrative relationship with the federal government to its reliance on new postwar technology.

Zacharias first proposed his idea for a new high school physics course in a memo to MIT president James Killian in March of 1956. Little came of it despite Killian’s effort to showcase the idea in a *Life* magazine article that ran in May. It was not until Zacharias tapped into the science policy elite in Washington that things began to happen. NSF, the institutional home of that elite, was looking for just this sort of project to get the ball rolling in curriculum reform; and Zacharias was well known as the type of person that could get things done. In the 1950s, the leaders of the scientific community made up a rather small club, and the physicists from Cambridge, along with their counterparts in Washington, dominated the professional network. The decision to go ahead with PSSC was made over cocktails in Washington, D.C. Kelly recalled that he had gone over to the Cosmos Club to follow up on a meeting Zacharias had with Waterman earlier that day.45 “As we talked,” Kelly noted, “most of the members of the Science Advisory Committee joined us. They all were enthusiastic and helpful in convincing Zach that he should take some time off to tackle the problem.”46 Before the night was over, Zacharias walked away with assurances of $200,000 to $300,000 of NSF start-up money for his project.

Most of the initial enthusiasm for PSSC was generated by the serious analytical approach Zacharias promised to bring to the task. Pointing to the important work he had done previously in the name of national security, MIT chancellor Julius Stratton insisted that Zacharias could do for education what he had done for the defense department.47 In the cover letter that went out with the grant proposal to NSF that August, Stratton wrote: “Professor Zacharias has a magnificent record of success in conceiving and carrying through such intensive study projects to fulfillment and we shall count again on his leadership.”48 As the planning began to take shape, the enthusiasm on the part of the scientific community continued to build. In briefing members of the National Academy of Sciences (NAS) on the course, Killian recounted how Zacharias “had led a group of study projects concerned with defense problems,” and that what had emerged from these was
a technique whereby “creative, imaginative people, free from interruption, [were] encouraged to think in an uninhibited way.” “Here was the opportunity,” he explained, “to try to provide [this] set up and approach to the problem of teaching in secondary schools.” NAS president Detlev Bronk commented favorably that the “wedding of scientific and effective new methods to traditional methods of education has exciting as well as challenging potentiality.”

The scientists Zacharias mobilized to remake high school physics were all well versed in defense-related research and development. The group included former OSRD director and MIT corporation chairman Vannevar Bush, James Killian, Rad Lab veterans Philip M. Morse, F. Wheeler Loomis, and I. I. Rabi and Edward Purcell, both of whom were Nobel laureates as well. From the Manhattan project came Francis Friedman and Philip Morrison. All the participants, as Zacharias would have it, were eminent in their fields—“Nobel Prize level” he recalled. In addition, many of the physicists in this small circle had participated in a number of key summer study projects. Zacharias and Friedman, the physicists who made up the heart and soul of PSSC, worked together on both Hartwell and Troy, as did Purcell. The lessons they learned from those experiences—particularly what Friedman had taken away from Project Troy with respect to information dissemination—would serve them well in their new educational endeavor. The connections to Troy drew even tighter in 1958 when James Webb, the former undersecretary of state who had initiated Project Troy on behalf of the Truman administration, took over the administrative reins of PSSC upon its conversion to Educational Services Incorporated.

From its formal inception in September of 1956, PSSC began to explore the benefits of a systems approach. At a three-day conference in December, participants hammered out the specific plan for the new physics course. The consensus of the group was to develop a comprehensive curriculum package. Zacharias was convinced that integration of both content and instruction would be central to producing an appropriate course. “The most effective way of teaching,” one early report outlined, “is to use several methods and media concurrently. Some parts of physics, like the evolution of ideas, mathematical deductions, etc., can best be learned if read repeatedly. . . . The significance of physical phenomena, on the other hand, will best be understood if the phenomena are seen repeatedly. . . . And the experimental method can be mastered both by seeing how demonstrations are prepared and carried out, on film and in the classroom, and by actual experimentation in the laboratory.” To realize this multimedia instructional vision, the group proposed to make “some seventy twenty-five minute films to be shown sequentially during a school year and that these films be supplemented with textbooks, teachers manuals, apparatus, examinations, and other devices.”
The film component of PSSC provides an excellent example of the systems influence. Instructional films had been used increasingly during World War II for both skill training and political indoctrination of military personnel. By the 1950s, researchers in education and psychology saw film as a powerful means of addressing the shortage of qualified science teachers. Henry Chauncey, the president of Educational Testing Service and soon-to-be PSSC Steering Committee member, made the argument that the ideal solution “would be to have fewer teachers than at present and utilize them to better advantage than we now do.” In the matter of imparting knowledge to students, Chauncey claimed that “instructional films can do as good a job in this respect—if not better—than the average classroom teacher.” In this way the very “best teachers in the country” could be reproduced at will and the celluloid distributed wherever needed. The day-to-day classroom tasks could then be handled by the classroom teacher or even a clerical assistant. The primary pedagogical task, the conveyance of content, in this scheme would be “given over, in effect, to the experts who prepare the instructional films . . . and accessory materials.”

In the spirit of Chauncey’s vision, Zacharias and the rest of the PSSC group devoted the bulk of their time and money to the production of the film series. To ensure the highest quality product, they consulted numerous film industry representatives including filmmakers Frank Capra and Walt Disney. Capra, whose experience with instructional films derived from his work producing the *Why We Fight* series of propaganda films for the Research Branch of the army’s morale division during the war, stayed on as a member of the Steering Committee for the duration of the project. On the technical end of things, the group, at the urging of Polaroid founder and Steering Committee member Edwin Land, seriously considered shooting the films in 3D technicolor to improve the realism of the presentation. And though never widely used, the project also developed a “school proof” projector, which had a variable projection speed that reduced the total amount of footage needed per film (and thus the cost to schools) and was loaded using film cartridges in order to eliminate the need to thread the film through the usual complex of sprockets. The ultimate fruit of this filmmaking effort for the physicists was to be direct access to the classroom via a medium in which they possessed complete control of the pacing and content of the physics taught.

But, as confident as they were in their ability to produce intellectually compelling physics presentations on screen, they worried about what would happen in the classroom when the projectors were turned off. “I am sure,” Zacharias commented to Webb, “that if we leave undone the job of converting a twenty-minute film into a fifty-minute period, we are going to get all sorts of funny results.” Both Zacharias and Friedman were specifically concerned with classroom discussions being “taken off into side alleys by
teachers who did not deeply understand the material that was being taught.”62 They also recognized the difficult position in which the films might place the teachers. Zacharias wanted to be sure that the teachers’ authority was not undermined; they at least “have to appear wise” to the students, he insisted.63 This was where the supporting materials came into play. By adding carefully designed student textbooks, problem sets, demonstration materials, wall charts, and lab experiments, they hoped to enmesh the teacher in a web of technical and textual support that would presumably ensure both teacher and student success according to the dictates of the course designers. Especially important was the teacher’s manual, a monstrous document over 1000 pages long that provided a day-by-day roadmap for teaching the course; it was, in a sense, the operations manual for the PSSC instructional system.64

For Zacharias, the success of PSSC was merely a question of proper engineering, which extended beyond the course proper to the dissemination network that PSSC and NSF had begun to build. While the MIT group worked on the production of course materials, the Steering Committee began to consider how to get those materials into the hands of the nation’s physics teachers. Here the physicists drew readily on the Cold-War thinking of Project Troy. In the words of one grant proposal, the next step for PSSC was to find “mechanisms for indoctrination of teachers, distribution of materials, and evaluation of their effectiveness.”65 Zacharias has already set his sights on the NSF summer institutes as a potential vehicle of dissemination. The Foundation “will sponsor 85 institutes in 1957 and probably more in 1958.” “These are,” he noted, “prime grounds for propagandizing our program and we must try to have materials ready for these institutes.”66

Clearly the well-defined patterns and models of scientific research that were exploited for defense purposes were brought over and applied with few modifications to the problems of education being considered at MIT. In typical “big science” fashion, the Physical Science Study Committee consumed over $5 million of federal funds from 1956 through the fall of 1960. It was, NSF felt, “one of the best investments this country has made.”67 Perhaps the most striking aspect of all this was the conscious manner in which the physicists worked to emulate the successful Cold War projects they had earlier developed. There was nothing implicit about it. Philip Morrison made this point clearly in comments he made in 1962. “We have seen a change occur in the research laboratory,” he stated, “and now the same processes are plainly at work in the classroom. This teaching of a new sort is analogous to the new sort of laboratory.” The shift in approach, as Morrison saw it, was profound. “Just as the Radiation Laboratory at Berkeley [home of the first synchrotron] has become a kind of symbol of the great laboratories of our time . . . so around the group at MIT, six or seven years ago, a new order of magnitude was entered in teaching. For the
synchrotron, read the film studio, for the teams of theorists and experimenters, substitute the many people of diverse backgrounds brought together in the committees, panels, [and] summer studies characteristic of this method.”68 Looking back some years later, Zacharias described his primary contribution in PSSC as bringing a systems engineering approach to the problems of education. “I did it,” he stated flatly—“there’s no question.”69

WOODS HOLE REVISITED

In September of 1959, Zacharias, Friedman, and curriculum reformers in mathematics and biology met with psychologists and other experts at Woods Hole in Massachusetts to take stock of and provide direction for the educational reforms that were taking place. For most educators looking back, Woods Hole is viewed as the birthplace of disciplinary structure as the guiding framework for curriculum development, of the importance of cognitive psychology for understanding children’s readiness to learn, and of a renewed appreciation of the intellect as a foundation for educational policy. These seminal ideas were all given form in *The Process of Education*, the formal report of the conference written by Harvard psychologist and conference chair Jerome Bruner.70 In many ways, Woods Hole has served as a marker indicating the starting point of the curriculum reform movement of the 1960s. But, as I have tried to demonstrate in this essay, history is rarely this discrete, particularly with respect to intellectual resources and material techniques. Although Woods Hole has come to represent the cognitive turn in curriculum development, as a place and an event in the history of education, it bears no less the imprint of World War II, the Cold War, and the increasingly influential scientific research establishment of the time. Despite its widespread circulation, Bruner’s best-selling book, in the end, served only to cloak the technocratic, systems approach in the guise of disciplinary structure, not to dilute its ultimate extension as a blueprint for educational reform in the United States.

The Woods Hole Conference, formally called the “Study Group on Fundamental Processes in Education,” was conceived by the NAS Advisory Board on Education as a summer study no different than Project Troy and Hartwell before it. It was held over the course of ten days at the Whitney Estate, a secluded facility with a history of hosting various defense studies, and funded by NSF, the RAND Corporation, and the Air Research and Development Command, among others.71 The initial NAS proposal laid out the by now familiar Zacharian argument that the most effective way to facilitate education reform was to bring “together imaginative people from various disciplines, engage them in provocative discussion, and challenge them to bring all their talents to bear on fundamental questions in education.” In the intense environment of the summer study, insights of anthro-
pologists, social scientists, and psychologists would combine with “the scientist-
engineer with a wide range of competence in communication theory and
electronics . . . to bring a fresh approach to problems of school learning.”

Bruner’s study was, in fact, the third of three studies sponsored by the
National Academy—all of which were part of redoubled efforts to improve
the nation’s schools in the months following the Soviet launch of Sputnik.
The first, held in Easton, Maryland, in April of 1958, explored the possible
contributions experimental psychology might make to curriculum reform;
and the second, which took place in Madison, Wisconsin that summer,
developed a long-range plan for conducting research in education. Both
studies reflected the growing concern among scientists that “much educa-
tional research [was] of doubtful quality and that first-rate psychologists
[were] too little involved in such research.” As one member of the PSSC
group commented disparagingly, “the great need in education today is
‘basic research’ . . . not ‘educational research’.” The Madison group re-
commended that significant short-term work be done as soon as practical
and that, at the same time, the NAS work to establish a nonprofit Organi-
zation for Research in Education (ORE), which would function to contrib-
ute, “through research on central and critical problems basic to education,
to the improvement and advancement of education at all levels.” Brun-
er’s study group was one of the more immediate responses to the recom-
mendations of the Madison planning conference. It was to be the gathering
to get things started before ORE became fully operational.

Although long-range research planning was prominent on the Woods
Hole agenda, there was the sense among participants that, in addition, the
conference should serve a “crash program” type of function in its charge
to provide immediate support for the ongoing curriculum study projects, which
by this time included biology and mathematics. Bruner noted in a pre-
conference memorandum that “it is worth drawing a parallel . . . to the
experience of psychologists working in the armed forces at the beginning
of World War II. Both with respect to training devices being used and in the
design of instruments to be used in combat and support operations, it
turned out to be the case that a great deal of useful work could be done
without the support of new research.” He went on, “I rather suspect that at
the outset there will be a parallel in work on curricula.” As a veteran of
the Psychological Warfare Division of the Office of War Information, Bruner
spoke from experience. He had also gained valuable insights into the dom-
inant research and defense consultation models as one of the twenty-some
experts who were enlisted to work on Project Troy, as did MIT historian
Elting Morison, and, of course, Francis Friedman and Zacharias, all of
whom joined Bruner at Woods Hole. Clearly in keeping with the systems
studies of the time, the specific objectives of this group as laid out in the
proposal were: “1). Improvement of Communication of Knowledge of Sub-
ject Matter, [and] 2). Improvement of Use of Technological Devices and Systems for Education.” Consideration of the “structure of knowledge in any given field” was secondary, to be undertaken only in the service of these primary objectives.80

The meeting opened on a Wednesday with a general overview of the issues that participants were to consider. This was followed by progress reports from the various curriculum projects. The lion’s share of initial discussion centered on the practical questions of how to effect immediate gains in student achievement from both psychological and systemic perspectives. Friedman expressed concern over what he called the “Foreign Policy of education,” that is, “once you have got the ideas, principles, etc. How do you get these put into effect?” How does one go about “managing the resistance . . . [of the] people in [the] school system who are responsible?”81 This, of course, was uppermost in the physicists’ minds as they were wrapping up the development of PSSC and looking for effective avenues for dissemination. Being the furthest along, they began the informal sharing sessions and essentially provided the model for systems-based curriculum reform. They were followed by the Biological Sciences Curriculum Study (BSCS) group, the School Mathematics Study Group (SMSG), the Illinois Math Project, and a group exploring the possibilities of an analogous curriculum project for history. On Friday, Bruner assigned conference participants to one of five working groups, each of which was to draft a report outlining “doable” research tasks in the area assigned. The five areas were sequence of curriculum, motivation, cognitive processes, teaching-learning systems, and intuitive thinking. The report manuscripts were turned in the following week and discussed in plenary sessions on Thursday.82

Though represented formally in only one of the working groups, the systems approach provided the analytical framework that focused much of the intellectual give and take over the course of the ten-day conference. The newer instructional technologies were prominent on the agenda. Films from the various projects were screened in the evenings, and two afternoon plenary sessions were devoted to teaching machines and audio-visual aids. All of these various material technologies were brought together within the systems vision in the report of the teaching-learning systems group, which by the end of the conference was renamed the Panel on the Apparatus of Teaching.83

The panel members were clear about the promise the systems approach held for improving education. They lamented “the disparity between the current levels at which the educational enterprise uses modern technology and the levels of its use in commercial communications, in transport and industry, or even in agriculture.” Education, they felt, with its “resistant conservatism,” was more than a step behind the times in this regard, and that the “adoption and exploitation of a systems approach to educational
design would further the application of modern technology to the improvement of education.” There was no reason, they argued, not to take advantage of approaches that have proven themselves time and again in other areas. “In our present day society, tremendous forward strides have been made in the design and development of new technical integrations of men and machines in the form of systems.” Although education did not fit exactly “the most spectacular examples of system design . . . found in complex military situations,” they claimed that schooling at both the elementary and secondary levels did represent “the kind of complex organismic enterprise the improvement of which can aptly be planned according to system development principles.”

The panel provided a ready-made systems template for designing nearly any kind of curriculum. In developing an instructional course, the authors explained, one must begin with the definition of the course goals. Once these are set, proper systems engineering required the derivation of the “functions to be performed by the various components” of the educational system. These naturally included such things as knowledge transmission, student motivation, attitude and skill development, and achievement diagnosis. Following that, “consideration can be given . . . in detail to the problem of assigning functions to men (generally, teachers) and machines in such a way as to optimize the effectiveness of the whole system.” Comparisons with weapon systems were quite explicit during the plenary discussions of the reports. In a memo summarizing the presentation of the Apparatus Panel’s report, Bruner noted that “we introduced this subject for discussion today by suggesting the analogy to a weapon system—proposing that the teacher, the book, the laboratory, the teaching machine, the film, and the organization of the craft might serve together to form a balanced teaching system.” From the panel’s point of view, all the topics discussed at the conference—from student readiness and cognitive processes to motivation—could be incorporated into this systems framework to manufacture curriculum packages that would provide this balanced attack for improving student achievement. The modern Cold War weapon system was, in the minds of these reformers, the epitome of rational instrumentation—a model to be emulated in seeking solutions to educational problems.

Despite the wide-ranging discussion of this approach and even the group’s recommendation that it be given a prominent place in the published conference report, Bruner, in the end, cast the event as a conversation among psychologists subordinating the technical to the cognitive. His bias in this matter should have been evident from the beginning. In his pre-conference memorandum, he made clear his stand on the various educational technologies being developed. Advancing this work ahead of the more fundamental research on cognitive processes was, he explained (using a particularly apt analogy), “like constructing a beautiful ballistic missile
and then aiming it intuitively and without systematic knowledge of the principles of orbits.” In the introduction to his book, Bruner freely admitted that it was not intended to capture the consensus of the group, only to put forth his own “sense of the meeting,” which, he noted, inevitably “reflect[s] the biases and predilections I bring to the task.” Such selective reporting did not sit well with some of the other participants, however. In an unpublished review, Zacharias wrote, “I had thought, incorrectly, that The Process of Education was a report of work done collectively by a group of people thinking collectively, as so often occurs during long summer studies. But now that I read Dr. Bruner’s book five years later, I see how much of the pattern was really his own.” As a member of the Apparatus Panel, John Flory was more noticeably upset with the final report. “It seemed to me,” he complained to Randall Whaley, the NAS conference coordinator, “that Dr. Bruner had completely ignored the opinions of almost everyone at the conference. . . . Certainly in the case of the Apparatus Subcommittee, he gave us ‘the back of his hand’ by emasculating what we had worked so hard to draft.”

Though this disappointment with the final report was clearly warranted if one were looking for a faithful summary of the events and discussions that took place, participants such as Zacharias and Flory could take solace in the growing hold that the systems perspective and the postwar research models had on the actual work being done. The Woods Hole summer study itself—its organization, execution, and even its institutional location at the boundary between the scientific research community and the state—was an artifact of the Cold War research establishment. And this influence extended, as we have seen, into the substantive work that went on there. Even in the material on cognitive processes that Bruner’s own working group had produced, one could find evidence of this orientation. In the discussion of the learning cycle, to take a particularly telling example, he wrote that “the nature and length of the cycle that one can impose on a learner depends upon a great many things, but the most notable factor to be taken into account is payoff.” It was in accomplishing this predetermined goal—the “payoff”—that the systems analysis manifested itself: “How long a learning cycle may be and what structure of cycles and sub-cycles is desirable is a problem for engineering research in pedagogy. . . . It is here that there is the greatest deficiency in available research on the educational processes.” In the weeks following the conference, the consensus among the organizers was that of all it had set out to accomplish, at the very least Woods Hole had been successful at bringing “the psychologists closer to those working with the subject matter improvement programs.” But, as Bruner’s words reveal, even the psychologists by this time had been incorporated into the technocratic model of curriculum reform brought over by the physicists after the war.
CONCLUSION

Despite Bruner's deliberate effort to deemphasize systems engineering in *The Process of Education*, the scientists' analytical approach to education reform seemed only to gain momentum going into the 1960s. In the years following Woods Hole, the PSSC group, configured first as Educational Services Incorporated under James Webb and later as the Educational Development Corporation, continued to apply the wartime organizational and technical models of research and development to a variety of new curriculum projects, from the Elementary Science Study to MACOS—the social studies project directed, incidentally, by Bruner.89

The point it seems at which the scientists' authority in education reform was fully established occurred the year prior to the Woods Hole Conference. It was during this year, 1958, following the Soviet launch of *Sputnik*, that the glare of public scrutiny focused most intensely on U.S. educational policy. Responding to both the public demand for action and the real needs of science education, President Eisenhower outlined plans to improve American education. The strong science emphasis in the various proposals being floated stirred up resistance among interested observers. A number of critics derided the narrow scope of the suggested reforms and called instead for a broad program of federal aid to update facilities, recruit teachers, and develop materials in all content areas. Pulitzer Prize winning poet Karl Shapiro lamented what he saw as the government's antagonistic stance toward the humanities, arguing that the country had gone too far “in adopting scientific education.” Others viewed the science-laden proposals as ill-advised attempts to emulate the Russian totalitarian system of schooling—a move that they claimed would ultimately crush cherished democratic freedoms. These objections notwithstanding, a surfeit of federal dollars was quickly made available to meet the Soviet challenge.90

In September of 1958, Eisenhower signed the National Defense Education Act (NDEA), which provided $1 billion over four years for college student support, the purchase of laboratory equipment by local schools, the development of foreign language instruction, and for research on the classroom use of instructional media, among other things. Though nowhere near the billion dollar mark, the National Science Foundation's budget nearly tripled the following fiscal year with the new funds being directed to expand the existing fellowship and institute programs, and, most notably, the work on the curriculum reform projects that had been going on since 1956. The popular attention to the schools ignited by *Sputnik* propelled PSSC into the spotlight as the exemplar of the direction such reform should take. Zacharias’ project was featured in the *Life* magazine series on the “Crisis in Education,” and even made headlines in the *New York Times*.91
The NDEA funds distributed by the Department of Health, Education, and Welfare and the Office of Education easily dwarfed those made available to NSF. The direction of educational policy, however, under the shadow of *Sputnik* was clearly controlled by the President’s science advisors. Their public stature, intellectual authority, and technical expertise had led federal officials within the Eisenhower administration and Eisenhower himself to pin their hopes on the scientists’ success. Given the prestige of scientists at the time, it is not surprising that many deferred to their judgment.

NSF, in tandem with PSSC, not only set the standard for curriculum projects in the other sciences, but legitimized an operational approach subsequently embraced by the U.S. Office of Education in its projects as well. One USOE official, for example, indicated that they would follow the scientists’ model for reform and “do for other basic subjects what the National Science Foundation has done, and is doing, for science and mathematics.” Commissioner of Education Sterling McMurrin went so far as to solicit advice directly from Zacharias. Noting the “enormous value” of NSF’s work, he asked specifically for guidance regarding “things to be done; how the Office should go about them; and in what priority” for reform in the “non-science fields.” Project English and Project Social Studies were the result. Even the scientists’ commitment to large-scale autonomous research institutions such as the Brookhaven National Laboratory and the Lawrence Berkeley Radiation Lab was emulated in the establishment of national educational laboratories and research and development centers by the USOE beginning in 1964. Beyond education, these scientific models and techniques spread throughout the government during the 1960s in arenas as diverse as the Department of Defense budgeting process under Robert McNamara and President Johnson’s war on poverty.

Tracing the emergence of these research models and techniques from their origins in the collaborative work of World War II not only helps us fill in a neglected facet of United States educational history but also reveals a good deal about the turn toward technique and performance in educational policy. The persistence of this emphasis in education has, of course, been contingent on the alignment of a particular set of social and institutional factors—World War II gave these research practices life, to be sure, but it was the Cold War that put a premium on scientific expertise and the technological approach. The contest with the Soviet Union maintained a kind of social and political pressure on our national institutions that elevated a bottom-line performance mentality above all else and in all things.

This idolization of technique that emerged from the weapons laboratories became axiomatic by the mid-1960s. Looking back on PSSC, which had started it all, Nobel laureate and Steering Committee member Edward Purcell tried to convey the spirit in which they had worked: “We were at that time pretty much imbued with the idea that we could do anything if we
started from scratch, and did it in a rational way, and applied all our technology to it, and so on. It was, of course, a time of almost euphoria in that respect, because the wartime successes of high technology and physics applied to new problems were still very fresh in our minds."95 Though by now the nation’s deference to the Cold War technological expertise of the scientists has surely passed (certainly the curriculum projects of the time have become obsolete), Purcell’s faith in engineering educational solutions, be they administrative or technological, is a more enduring sociological artifact. It is, perhaps, the most significant educational legacy of the postwar scientific elite—a legacy that has too narrowly constrained our understanding of the role of curriculum in schools and, more importantly, our collective vision of the ultimate goals education might serve.

Notes


5 Throughout this essay I am concerned with following the development and use of particular techniques as a means of improving education during this time period. It is important to make clear that the ends to which these techniques were directed, that is, the specific objectives the scientists were attempting to accomplish with the reformed curriculum, are not the subject of my analysis. With respect to the ultimate objectives, what one finds is that these technical means were not used for technical ends.

6 Notable exceptions to this are found in some treatments of higher education; see, for example, Stuart W. Leslie, The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford (New York: Columbia University Press, 1995); and Rebecca S. Lowen, Creating the Cold War University: The Transformation of Stanford (Berkeley: University of California Press, 1997). At the precollege level, the only work of which I am aware that


14 Peter Galison, “Physics between War and Peace,” in *Science, Technology, and the Military*, p. 73.


17 Scientists at this time were actively sought out as commentators on a whole range of issues, often in areas well beyond their specific areas of expertise. As historian Daniel Kevles described, in the civic arena it appeared that “no public forum on the issues of the nuclear age [was] complete without a physicist. Physicists were asked to address women’s clubs, lionized at Washington parties, and paid respectful attention by conventions of theologians and social scientists;” Kevles, *The Physicists*, p. 376.

18 Hughes, pp. 8–9, 302; Fortun and Schweber, pp. 336–338; Agatha C. Hughes and Thomas P. Hughes, eds., *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After* (Cambridge, MA: MIT Press, 2000), pp. 8–9; and Pickering, n22, p. 18. Important from a historical perspective is the fact that the experts who articulated this new approach to postwar problems explicitly drew on the physicists’ analytical models rather than the earlier models of scientific management of which they were largely unaware.

21 Goldstein, p. 103.
22 Marvin and Weyl, p. 2.
26 Needell, p. 13.
27 L. V. Berkner to Secretary of State, 27 December 1950, Troy Report Files.
28 Marvin and Weyl, p. 6; Goldstein, pp. 155–156.
31 Quoted in Needell, p. 9.
32 Berkner to Secretary of State, 27 December 1950, [emphasis added], Troy Report Files.
33 Project Troy Report, p. 3, Troy Report Files.
39 NSF Scientific Personnel and Education (SPE) Divisional Committee meeting minutes, 19 May 1954, box 40, NSF Historian File; ATW Diary Note, 21 July 1954, box 5, Office of the


Harry C. Kelly interview, box 1, PSSC Oral History Collection, Institute Archives and Special Collections, Massachusetts Institute of Technology.

SPE Divisional Committee meeting minutes, 18 May 1955. The possibility of funding textbook revision projects had been discussed as early as the spring of 1954, SPE Divisional Committee meeting minutes, 19 May 1954, both in box 40, NSF Historian File.


House Committee on Appropriations, Hearings before the Subcommittee on Independent Offices, 84th Congress, 1st session, 9 February 1955, pp. 234–235 [emphasis added].


Killian to various foundations, 11 September 1957, box 17, Zacharias Papers.

PSSC meeting minutes, 12 March 1957, box 16, Zacharias Papers.


57 Stanley Salmen to Zacharias, 10 August 1956, 13 August 1956, box 17, Zacharias Papers; PSSC meeting minutes, 10 November 1956, box 1, PSSC Oral History Collection; Goldstein, p. 173.

58 Herman, pp. 69–71.

59 PSSC meeting minutes, 8 September 1956, box 1, PSSC Oral History Collection; PSSC meeting minutes, 20 October 1956, box 170, MIT Office of the President.

60 Elbert Little to Waterman, 20 December 1956, box 19, NSF/ODSF.


63 Zacharias to Killian, 15 March 1956, box 170, MIT Office of the President.


65 Zacharias to various foundations, 18 June 1957, box 17, Zacharias Papers.

66 PSSC, “What We Seem to Know,” 11 October 1956, box 1, PSSC Oral History Collection.

67 Kelly Diary Note, 16 December 1958, NSF Historian File.


69 Zacharias interview, PSSC Oral History Collection.


77 Whaley to Bronk, 22 January 1959, Woods Hole Papers.


81 C. Ray Carpenter, handwritten conference notes, Carpenter Papers.
83 Summer Study on Educational Processes, tentative schedule, 9 September 1959; Bruner, remaining conference agenda, 14 September 1959, Woods Hole Papers. The panel members included psychologists C. Ray Carpenter, John B. Carroll, and Donald Taylor; John A. Fischer from Teachers College; and film experts John Flory and Don Williams.
85 Panel Report on Apparatus of Teaching, 18 September 1959; Bruner to Apparatus of Teaching Group, 12 September 1959, Woods Hole Papers.
88 Webb to Stratton, 23 September 1959, box 353; see also Chauncey to Webb, 1 October 1959, box 362, Webb Papers.
89 Zacharias interview, PSSC Oral History Collection; Goldstein, pp. 192–237. The story of MACOS is told by Dow in Schoolhouse Politics.
94 Exploring the implications of this turn on the lived experience of children in schools is a subject worthy of sustained study. Such an undertaking, however, lies beyond the scope of this work.
95 Edward M. Purcell interview, box 2, PSSC Oral History Collection.
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