The Golden Fleece, Science Education, and U.S. Science Policy[1]

Richard C. Atkinson
President
University of California

I was pleased to accept Roger Hahn's kind invitation to participate in this colloquium series. It gave me an opportunity to rethink some events I was associated with at the National Science Foundation (NSF) in the 1970s. I would like to review briefly U.S. science policy since World War II from the perspective of the National Science Foundation, and in particular from the narrower perspective of science education and the social sciences at NSF. This is a personal account, not a schol- arly one, and I would be delighted if my remarks were to stimulate some aspiring young historians to undertake a more careful study of the events I am going to discuss.

My story begins with World War II and the remarkable success of U.S. science in the war effort—a critical factor in our victory. President Roosevelt's science adviser, Vannevar Bush, had been a long-term member of the faculty at the Massachusetts Institute of Technology; he was one of the key people responsible for building the quality of that institution. Bush had a close personal relationship with Roosevelt. Near the end of the war the president asked him to define a plan for American science in the postwar period. That request led to Bush's landmark report, *Science, The Endless Frontier*, one of the great documents of American history. The Bush report defined science policy for the post-World War II era.

What was the nature of that report? No summary could do justice to Bush's masterful analysis, but essentially he made three principal arguments about the future of the U.S. scientific enterprise. First, he argued that most aspects of R&D were the responsibility of the private sector. But he also recognized that market mechanisms would discourage the private sector from investing adequate funds in basic research. This led Bush to

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This led Bush to his second argument: ensuring support for basic research in the postwar period should be the responsibility of the federal government, because the enormous benefits to society at large justified the investment. He did not believe basic research should be conducted in government laboratories, however, but in the universities of the nation. As the institutions responsible for the nation's basic research, universities had pride of place in Bush's vision of the research enterprise. Third, he argued that decisions about which university research projects the government would fund should be made via a peer-review process.

Bush envisioned a federal agency that would be responsible for funding these research activities. Legislation was introduced in 1945, but because of disagreements between the Truman administration and Congress, as well as within the Congress itself, the National Science Foundation was not created until May 1950. The events of this five-year period are nicely described in an excellent recent biography of Vannevar Bush by G. Pascal Zachary.

One of the debates surrounding that legislation involved the scope of the Foundation's proposed activities. Harry Truman was now president. His associates urged a broader range of responsibilities for the foundation than Bush's supporters did, one that included science education and the social sciences. Bush, on the other hand, had only minimal interest in including science education and no interest at all in including the social sciences. James Conant, a close colleague of Bush renowned for his reorganization of Harvard's general education curriculum, was a strong proponent of including science education on NSF's agenda. In the end, Conant's view prevailed. Science education became one of NSF's responsibilities. So did the social sciences, but without a clear mandate to fund them.

NSF got off to an extremely slow start, with minimal funding in the various sciences. There was a trickle of science education activities in the early years, but they were almost wholly confined to supporting fellowship programs for graduate students. Bush and many other leading scientists of that period felt NSF was not meeting their initial expectations, and viewed the agency as of little consequence.

The world changed in October 1957, when Sputnik was launched. The public response bordered on panic: there was much alarmed discussion of an education gap—an ominous disparity between the quality of American science education and its counterpart in the Soviet Union. Within a month the administration established the President's Science Advisory Committee (PSAC), which played a very important role in the Eisenhower, Kennedy, and Johnson administrations. Congress responded with the National Defense Education Act, which dramatically

increased federal funding for student loan programs and graduate fellowships in science and engineering, among other things. In the post-Sputnik years, support for science climbed rapidly, and funding for NSF took off. Gradually the activities in the social sciences increased, until by 1968 legislation was introduced to change the NSF Organic Act to require funding in these disciplines.

In particular, science education blossomed. NSF began offering summer institutes for K-12 teachers, in which leading university scientists met with teachers to discuss scientific developments and how to teach them. Even more important were curriculum development projects. Few people trusted the Office of Education to carry out this responsibility; NSF was the agency everyone turned to. NSF started in physics, with a curriculum developed by Zacharias of MIT, and a mathematics curriculum quickly followed. So did a program in chemistry; faculty at UC Berkeley played an important role in developing the chemistry curriculum. One can criticize these programs. They were too difficult for the average student—too focused on the best students—but the simple fact is that if you go anywhere in the world today, you will find that these programs are still in use and are regarded as outstanding curricula.

The curriculum projects went so well that NSF decided to be even bolder. It ventured into the biological sciences and began to develop and distribute biology courses to the high schools. Teachers were given special training, and the curricula were widely used. Eventually these curricula expanded to include topics on evolution, which brought out the creationists in force. They criticized NSF's involvement both as undermining religious beliefs and as a federal intrusion into local authority. But the loudest outcry was reserved for a social science curriculum called *Man: A Course of Study* (MACOS). MACOS was developed under the intellectual leadership of Jerome Bruner, who was at Harvard at that time.

MACOS focused on cultural diversity, principally from an anthropological viewpoint, and was aimed at students in grades seven, eight, and nine. One of the films produced for the course told the story of an Eskimo village above the Arctic Circle. Among the Eskimo practices depicted in the film was the custom of borrowing someone else's wife to keep you warm on a long journey across the ice if your own wife was not well enough to accompany you. Another was the practice of abandoning grandparents on an ice floe when they became too old to contribute. MACOS succeeded brilliantly in demonstrating cultural differences; it was equally effective in arousing public outrage. There were protest rallies, public meetings at schools that

adopted MACOS, and vitriolic editorials—Jim Kilpatrick wrote extensively on the damage MACOS was inflicting by undermining the moral character of America's young people.

Around this time Senator Proxmire began presenting Golden Fleece awards for instances of government fraud, waste, or abuse. An early award went to the Air Force for spending $2,000 per toilet seat for bombers. But soon Proxmire's interest shifted to NSF, and the agency became a perfect target. One of the early awards was a Golden Fleece for a research grant entitled "The Sexual Behavior of the Screwworm Fly." Proxmire got tremendous attention for that; I'll return to it a little later.

Correction, February 2020. As pointed out to me recently by Jeffrey Mervis of *Science* magazine, this statement is in error. NSF did not make a grant for research on the screwworm fly. The actual research was first funded by the U.S. Department of Agriculture in the 1930s, well before the Golden Fleece awards. When I first arrived at NSF, there was a document listing examples of research projects that appeared frivolous at the time they were conducted, but later yielded important applications. The screwworm fly was one of those examples and was frequently cited in Congressional testimony in defense of basic research. It proved to be the perfect counterexample to the Golden Fleece. How I managed to misrepresent this work is an instance of what psychologists would call the malleability of human memory.

When he delved into the social sciences, he found an NSF-supported grant dealing with an experimental analysis of love from a social/psychological perspective, and another grant concerned with a theory of love. At that time the *National Enquirer* was paying a $500 bounty to freelance reporters who came up with a story of this sort, and many writers would just scan the titles of research projects supported by NSF. The *Chicago Tribune* had a field day with the theory of love grant, and as if this weren't bad enough, they found a project titled "A Theory of Necking Behavior." We tried in vain to find this grant on NSF's list of social science projects. Days later we finally unearthed it among the engineering projects—the necking referred to was of a metal, not a human, variety.

Several of the faculty grantees who were recipients of the Golden Fleece wore it proudly as a badge of merit and made the most of their notoriety on the Johnny Carson show. This was serious business for NSF, however, because it played havoc with the Foundation's public image and relations with Congress.

This is where my story begins. I came to NSF on 1 July 1975. Guyford Steever, director of NSF at the time, had been a long-term

professor of physics at MIT and later president of Carnegie-Mellon University, as well as having served as an aide to Vannevar Bush during World War II. He had landed at Normandy on the second day of the invasion to seek out and investigate V-2 sites. The beach commander told his group that such a site had been liberated thirty miles up the road. When they arrived, they found the report had been a bit premature—the site was still occupied by the Germans. The German commander seized the opportunity to surrender, however, and all ended well. Newspaper reports established Stever as a national hero.

I was recruited by Stever to be the deputy director of NSF. I had never had any interest in administration as a university professor, and frankly had a rather low regard for academic administrators—university presidents included. But the prospect of spending some time in Washington, D.C., was appealing to both my wife and me, particularly since our daughter was due to go off to college that fall. Why, one might ask, was I chosen by the people at NSF? I had a good relationship with the Kennedys; I had worked on Robert Kennedy's presidential campaign, and Senator Ted Kennedy was the chairman of NSF's Appropriations Committee. Even though I was a social scientist, I worked on mathematical problems, had been featured in *Life* magazine for having developed computer-based systems for education, and was a member of the National Academy of Sciences. I was not a hard scientist, but my pedigree was not too suspect. Guy Stever proved to be a persuasive recruiter and so I joined NSF on a two-year leave from Stanford.

At this time considerable criticism was being directed toward science activities of all sorts. Ever since the publication of Rachel Carson's *Silent Spring* in the 1960s, there had been a growing feeling abroad that the purity of science, as it had emerged from World War II, was not quite as pure as it had seemed. This was immediately after the Vietnam War and there were sizable cuts in science budgets; money was hard to come by and scientists whose grants were not funded were critical of peer review and in turn of NSF. Proxmire was tapping into this public unease about science, and Congress followed his lead. During the winter before I came to NSF, two congressmen—John Conlan of Arizona and Robert Bauman of Maryland—were particularly severe critics. They introduced a series of bills eliminating science education from NSF. Bauman had one bill that would have required every grant from NSF to be reviewed by Congress; it passed the House and it was only thanks to the conference committee that the requirement was eliminated. The Congressional Record for that

period is replete with speeches by senators and congressmen targeting NSF for criticism.

The criticism of science education programs became so intense that Steever wrote to Congress in March announcing his intention to establish an in-house group to review NSF's science education programs and to assess the criticisms that had been leveled at them. The group, which included some longtime insiders at NSF, was chaired by Bob Hughes, a new presidential appointee who served as one of the Foundation's assistant directors. Hughes had a very heavy travel schedule, so his personal involvement in the study was limited.

The Hughes report was published a few days before I arrived at NSF, and it was the first thing I read. The report did not deal with the philosophical criticisms of NSF. Instead, it discussed NSF's business dealings and the appropriateness of its peer-review procedures as they applied to NSF curriculum projects. The report made a persuasive case that NSF had done its business in an orderly and thoroughly appropriate way, and I finished it convinced that the cloud of criticism hovering over NSF would soon be dispersed.

A few weeks later I was asked to testify on the Hill about the peer-review process as it was used throughout NSF. Director Stever was on a trip to Russia, so I went solo on my first appearance before Congress as a member of a federal agency. The chair of the committee was James Symington, son of the former senator Stuart Symington. He was sympathetic to NSF and many years later characterized his experience and the events associated with NSF's science education programs as comparable to his famous father's experience with Senator Joe McCarthy. Bauman and Conlan entered the room shortly after I started my testimony and immediately accused NSF of having produced a report that was "a pack of lies." We were deliberately misleading the Congress, they charged. I was stunned; there had never been criticism like this. When Stever returned from Russia, he joined me at the next peer-review hearing, where the same accusations were repeated. Finally Stever responded in exasperation that we had done our very best to examine these matters, and if the Congress didn't think we had done a thorough job, it should call for a General Accounting Office (GAO) investigation. After the hearing ended, Symington suggested that such an investigation would surely silence the critics. Stever agreed, and so that summer the Congress initiated a GAO investigation.

The fall passed with hardly a mention of the GAO investigation. One Friday in early January, I received a call from Symington, who said he wanted to see me at three o'clock. When I arrived at his office, Symington was alone, with a stack of documents on his desk. One was

the GAO report, sent first to him as the committee chairman. He told me to read the executive summary. My heart beat quickly as I scanned it. Then he handed me a press release, which he told me to read and change as I saw fit. The press release, he informed me, would be issued before I left his office. He wanted to be sure that Conlan and Bauman didn't get a jump on him and release the news before he did. The news, needless to say, was very bad indeed.

I got in touch with Stever as soon as I could. It was about six and he was in a tuxedo, about to go to a White House dinner for the president of France. We decided to assemble a group to examine the GAO report. Time was of the essence. I pulled together a small investigative team of people whom I had gotten to know at NSF and whom I trusted; none of them had served on the Hughes committee. By nine that evening we had sequestered the relevant files and were hard at work. We worked all night Friday, all day Saturday, and Saturday night as well. On Sunday morning I called Stever and went to his house in Georgetown. I ex-plained to him that our investigation had made it clear that the GAO report was not only correct, but had merely scratched the surface. Matters were even worse than the GAO portrayed them. We spent several days in despair, struggling to decide what to do. My view was that we had to reveal everything as quickly as possible; others thought we should tough it out. A few days later, Stever met with Rice University president Norman Hackerman, chair of the National Sci- ence Board (NSB), the presidentially appointed oversight board of NSF. Stever explained the problem to him, and the two of them then asked me to outline a plan for dealing with the situation. I did so, and was told that afternoon to proceed without delay—to get the whole story out, and quickly.

What did the GAO report say about our science curriculum projects? 1. NSF engaged in poor business practices. 2. It failed to do appropriate audits. 3. There were some inappropriate expenditures of funds. None of this was criminal, but it was clear that the Foundation was doing a less than effective job. Many of these projects had gone on for more than six years with little effort to assess their quality or effectiveness. A particularly difficult criticism was that the curriculum programs often involved major commitments of funds—so much so that they had to go for final approval to the National Science Board. Yet the peer reviews sent to the NSB were redacted by program officers so that they were highly selective, emphasizing positive assessments and deleting negative ones.

Why did the Hughes group fail so badly? Hughes is a fine individual and a distinguished chemist, who has been an important

contributor to science policy. But he was a new presidential appointee with an incredibly heavy workload and travel schedule. He did not have time to monitor the committee's activities on a day-to-day basis or involve himself in a detailed analysis of the relevant documents. Unfortunately, some of the staff on the Hughes group conspired among themselves to cover up the problems. And how did Conlan and Bauman know what was going on? They had two people inside the NSF who were keeping them informed daily. A few years later one of Conlan's aides remarked that they knew within hours after an NSF staff meeting exactly what had transpired.

NSF's response to the GAO report proved to be very effective. Our candor stunned the Congress and took the wind out of our critics' sails. We acknowledged the faults in our procedures, the questionable character of our business practices, and the inappropriateness of some of our expenditures. Two individuals were placed on administrative leave and one was later terminated. We restructured the science education programs, revised our policies, and recruited new leadership. There is an account of these changes in various NSF news releases and reports issued at that time.

We also changed the peer-review process throughout NSF. The program officers had, and still have, great flexibility. They solicit peer views for a given proposal, and then use the information—as they judge appropriate—to decide whether or not to fund the project. Program officers should have that kind of decision-making authority, but there is also a need for oversight. Accordingly, we established an audit office that did random samples of peer reviews to ensure that they were being used appropriately.

In addition, we changed the procedure for soliciting peer reviews. Reviewers, in the past, had been told that applicants could request a copy of their review, but that the review would be redacted to protect the identity of the reviewer. Redaction proved to be a serious problem in the GAO report and more generally throughout the Foundation. Too many errors were made in the process (especially when many reviews had to be redacted), compromising the entire peer-review system. Accordingly, we told reviewers that in the future their reviews might be shared with applicants, and that they should write them in a way that protected their anonymity. Reviewers quickly adjusted to this procedure and redaction was no longer necessary.

We also began to edit titles and abstracts of proposals to avoid the kinds of problems we had with the *National Enquirer.* This proved to be necessary only on rare occasions, but the very idea created a stir in the academic community. How dare you edit our work? was a

common reaction. I don't know whether they still do this at NSF, but in my day it was useful in preventing reporters from misrepresenting the true nature of a research project.

In the summer of 1976, Stever resigned to become science advisor to President Ford. Nixon had fired his science advisor, Ed David, and had abolished PSAC in 1973. He was unhappy with the academic community in part because of its anti-Vietnam War activities. Nelson Rockefeller, Ford 's vice president, believed that PSAC had played an important role in the past and should be reestablished, but with congressional legislation this time. That took a while, however, and in the summer of 1976 Stever became the director of the newly established Office of Science and Technology and I became acting director of NSF.

The next few months were possibly the most interesting of my life. I took steps to phase out the RANN (Research Applied to National Needs) program; in many respects it was a reasonably productive program, but its approach to the support of research was not appropriate for NSF and did not live up to our standards. I closed several regional offices, including one in San Francisco. I ordered a reduction in force—a RIF—a very unusual action in the federal government. These actions raised some hackles in Congress and OMB, but in my view I was cleaning house for the next director.

By the time Jimmy Carter was elected in 1976, I had the strong support of the National Science Board, whose membership included Frank Press, soon to be named the president's science advisor. The next thing I knew I was nominated to be director of NSF. It was a move I had neither intended nor expected. Nor did I, with my social sciences background, quite fit the mold of an NSF director. Not long after my appointment, on a visit to Columbia University, I saw Dr. I. I. Rabi, an influential physicist during and after the Second World War. He told me he had heard only the best things about me, and was so pleased I was going to be the director of NSF—and by the way, what field of physics was I in?

Perhaps my most important contribution as director was to recruit George Pimentel, from this campus, as deputy director. George was a world-renowned chemist, whose death a few years ago was a great loss to science and to UC Berkeley. George and I worked well as a team and accomplished a great deal. Together we brought the business and administrative practices of NSF into the modern age. We expanded the behavioral and social sciences. We elevated engineering to the level of a full directorate. This pleased the

engineering community, many of whose members were trying to get the Foundation's name changed to "National Science and Engineering Foundation." We also established a research program in economics, focused on the role of R&D in stimulating economic growth; that field of research has prospered over the past twenty years, and has led to an important development in economics known as "new growth theory."

It was also clear to us in the late 1970s that, while the nation's research universities were amazingly fruitful in producing new ideas, the process of transforming those ideas into applications—technology transfer—was not working as well as it should. We responded in several ways. NSF initiated the Industry-University Cooperative Research Program, a venture that was controversial in the 1970s but today is standard practice. In addition, we assembled a working group to address the federal policy that patents generated from government-supported research at universities should reside with the government. We conducted a series of policy studies that laid the groundwork for the passage in 1980 of the Bayh-Dole Act, which transferred patent rights to universities.

Those were the years when China, with the end of the Cultural Revolution, was beginning to open to the West. During my tenure as NSF director, I negotiated and signed the first memorandum of understanding in history between the People's Republic of China and the United States, an agreement for the exchange of scientists and scholars. Finally, I claim sole credit for establishing the Vannevar Bush Medal, awarded annually by the NSB to an individual who has made major contributions to the well-being of the science enterprise. As may be obvious, Bush stands tall in my eyes.

During my years as director, NSF received no Golden Fleece awards; Senator Proxmire, indeed, became a good friend to the Foundation. In my last few weeks at NSF, Proxmire spoke at a seminar on biological methods of pest control. At the seminar he freely admitted that the study of the sex life of the screw-worm fly had been of major significance to progress in this important field.

I left NSF in July of 1980. Ronald Reagan was elected the following fall. He appointed as director of the budget David Stockman, whose first budget eliminated from NSF all science education activities (except graduate fellowships) and all of the social sciences. By the time the budget made its way through Congress, some of the social science activities had been reinstated, but at greatly reduced levels. A few years later, in an article in the *New York Times*, Stockman stated that he had made a mistake in eliminating these programs. On the

other hand, he said, it was the kind of mistake he didn't mind making. But as the 1980s unfolded there was a renewed focus on science education throughout the country, and gradually NSF reintroduced and added programs in that area.

Congress always liked science education. One of NSF's problems was that most of the research it funded went to a relatively small group of universities; their concentration in a few large states complicated NSF's ability to gain broad support in Congress. In science education, on the other hand, funds went to virtually all of the states. While I was director, we started a program to work with universities in states that received few NSF grants, giving them advice and assistance so that they could be more competitive in seeking grants. It was called Experimental Program to Stimulate Competitive Research, or EPSCoR, and is still in existence today. That is an interesting story all by itself, one that needs to be examined.

By 1992 the science education directorate was reestablished and the social sciences were viable if not prospering, but clearly the reemergence of these two areas was influenced by earlier events. Some people argue that the Foundation—shaped by these events—has been too cautious in its approach to science education and the social sciences.

Conlan lost the 1978 election. Bauman prospered throughout the 1970s—he was a leader on the floor of Congress and an important figure in the conservative movement. Everyone thought he would run for the Senate in 1982. Then the world came apart for him—he was arrested for sexually molesting a young boy. This story is told, with admirable candor, in his book *The Gentleman from Maryland: The Conscience of a Gay Conservative*. Once he had been arrested, his career was finished. He had been NSF's most severe, persistent, and unrelenting critic, charging that our efforts in science education served only to undermine the moral character of American children. Reading his book, I felt a certain sadness about what happened to him. But when I recall him across the witness table, my sadness is easier to bear.

The purpose of these remarks has been to give you a sense of the evolution of federal policy on science and science education in the postwar era, through the lens of my personal experience at NSF. The science enterprise during the postwar period needs to be interpreted from a variety of perspectives. Perhaps my experience will prove useful. Let me end as I began, with the hope that, if nothing else, these remarks may stimulate some young historians to take a fresh look at this fascinating era in the annals of American science.