

The NSF: Past and Present Issues

A Minor Miracle. An Informal History of the National Science Foundation. MILTON LOMASK. National Science Foundation, Washington, D.C., 1976 (available from the Superintendent of Documents, Washington, D.C.). x, 286 pp. Paper, \$2.75.

To mark the occasion of its 25th anniversary the National Science Foundation commissioned this account of its life and times. Such a procedure, while normal enough in big business, is a dubious practice for a government agency. It is bound to raise the suspicion that the intention is to invite a flattering birthday portrait rather than a frank account by an objective historian. All the more is this the case when the government agency is one that prides itself on supporting rigorous scientific research and that usually provides this support through grants—designed to free the investigator from constraints on his critical judgment—rather than, as in this instance, through negotiated contracts.

Lomask succeeds in allaying some but by no means all of the suspicion. To his credit, he has taken pains to dispel the myth “according to which Vannevar Bush simply lifted his hand one day and said ‘Let there be a National Science Foundation’ and there was one” (p. 112). While fully acknowledging Bush’s indispensable part in providing a rationale and much of the political impetus for such a foundation, he shows that the legislative work of a host of congressmen, including Harley Kilgore, Warren Magnuson, Elbert Thomas, Leverett Saltonstall, H. Alexander Smith, James Priest, and Robert Crosser, of government lawyers like Oscar Cox and Oscar Reubhausen, and of legislative assistants like Herbert Schimmel—not to mention scientists and engineers, whose motives can perhaps be said to have been more self-interested—was also essential to the success of the effort.

Similarly, while he devotes considerable space to the works and wiles of the four directors of the Foundation—Alan Waterman (who guided the agency for the first 12 of the 25 years, under three presidents), Leland Haworth, William D. McElroy, and H. Guyford Ste-

ver (who resigned the post in January of this year)—he does not neglect the activities of some of their key assistants. In particular, Ray Bisplinghoff and Joel Snow are credited for developing the Research Applied to National Needs (RANN) program and Bernard Sisco is singled out for his effort to revamp and tighten internal organization. Too often, creative but unglamorous administrative accomplishments like theirs go altogether unnoticed, although they are vital to the performance of the public’s business. Lomask did well to distribute the laurels widely, even though none are assigned directly to the many program officers and policy analysts who also serve in vital capacities.

The one analytic theme that is dealt with in any depth by Lomask is certainly important to any understanding of the character and role of the National Science Foundation. This is the conflict that has persisted from its prehistory to the present—and will likely endure as long as the agency retains its present form—over whether it should take its direction from the research communities and universities that are its prime beneficiaries or from the President and Congress, for whom research and higher education are instruments of the national policies they seek to shape and implement. This is a classic dilemma facing virtually all government agencies that provide support or services to particular clients, but it is especially acute in the case of support for basic research and higher education because it involves the question of who knows best, the government or the scientists and educators, how the national interest is served most effectively.

In his foreword, Dael Wolfe poses the issue with characteristic bluntness, discussing it from the point of view of the agency:

How much should it serve the interests of its own constituency, and how much should it use its powers to further the objectives of the most central parts of the Federal Government[?]. In this case, scientists generally wanted the Foundation to be *their* agency, while some other parties—most typically represented by the Office of Management and Budget—wanted it to be *their* aide and ally.

It was precisely because of this principled tug-of-war between the scientists who were promoting the idea of such a foundation (led by Isaiah Bowman of Johns Hopkins) and President Truman’s budget director, Harold Smith, that the legislative effort to draft and pass an acceptable bill foundered in contradiction and confusion for no less than five years. When the issue was finally “resolved”—more, one suspects, out of exhaustion than out of conviction—the result was a bill calling for the appointment of a director who would be responsible to the President and for a board with powers roughly equivalent to the director’s, also to be appointed by the President but presumably to reflect the views and interests of the scientific community.

Two years after the NSF came into being, the same conflict expressed itself when William D. Carey, on behalf of the Budget Bureau, tried to persuade Waterman to live up to the broader role envisioned for the Foundation in its legislative mandate by evaluating the research activities of other federal agencies and developing a national science policy. Waterman thought it was “too dangerous and difficult” (p. 73) for so new and frail an agency to try to sit in judgment on other, far better established agencies, and he was reluctant to divert the Foundation from its primary mission, as he interpreted it, of supporting basic research. Waterman succeeded in fending off the Bureau’s entreaties, with the result that it was not until the Sputnik crisis of 1957 that steps were taken, leading ultimately to the creation of the Office of Science and Technology in 1962, to provide the Executive Branch, and the Budget Bureau in particular, with the help it needed to coordinate and manage the burgeoning federal involvement with research and development.

In the 1960’s, when the primary commitment of the NSF was well established along the lines laid down by Waterman, those who guided its destinies proved more willing to accede to pressure from Congress to allocate some of the Foundation’s resources to supporting applications of science that might address acute social problems. The NSF’s eventual response to the 1968 statute amending its charter was the RANN program, but it is interesting that when, at first, Haworth interpreted the new charter to mean that the NSF should solicit proposals for applied research on a broad front, he was promptly advised that it was not the intent of Congress to make the NSF a general supporter of applied research. The intention was merely to make it pos-

sible for the Foundation to support some research efforts, however defined, that would address major social needs.

By this time, in other words, the political pressure put on the NSF was mediated by congressmen like Emilio Daddario and Charles Mosher, who appreciated the need to preserve the primary responsibility of NSF as a patron of basic research, particularly in the physical sciences and in the nonmedical areas of the life sciences. This position was shored up by the passage in 1970 of the Mansfield amendment, which ordered the military agencies (and induced others) to refrain from supporting basic research unless it was directly relevant to an agency mission and called upon the NSF to take up the resulting slack.

Nevertheless, the conflict persists. Should the NSF take its cues from professional judgments of needs and opportunities, such as those of the National Academy of Sciences Committee on Science and Public Policy (COSPUP) or "proposal pressure" from researchers? Or should it frame its allocations to reflect political determinations of national need so that research is "targeted" to "relevant" concerns? Should it confine itself to its traditional role of supporting academic science or, as Senator Edward Kennedy would apparently prefer, should it invite applications for the support of basic research in private industry?

This important conflict is traced, at least in outline, by Lomask. Otherwise, however, his account leaves much to be desired. Certain important issues are either neglected entirely or touched upon only very lightly. Controversial questions are invariably approached with the benign aim of showing that, when all is said and done, those in charge were men of integrity who did their best under trying circumstances to keep the Foundation on a steady and balanced course despite conflicting pressures. The carping criticisms made by Daniel Greenberg are shown to be unwarranted because other, presumably more judicious, observers disagree with his assessments. The best (or worst) example of this approach is the discussion of the Mohole episode, which is pursued at greater length than any other topic, but to absolutely no point, either explicit or implicit. Lomask may be the only writer to have published an account of this affair—which, as he himself points out, involved the only NSF project ever to be terminated by an act of Congress—without attempting to explain its failure so that future administrators might avoid repeat-

ing earlier mistakes. Imbalanced or not, at least Greenberg has made an effort to assign the blame and draw the lessons.

The trouble with this approach is not simply that it splashes whitewash too liberally but that it fails to do justice to the historical materials. Despite the title, for

example, Lomask never explains why he thinks the NSF or its accomplishments should be regarded as "a minor miracle." It may be that he means to suggest that in a country notorious for its hostility to impractical knowledge and ivory-tower intellectuals the establishment of

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such a foundation and the level of support it has been provided amount to something of a miracle. Or maybe the wonder is that the NSF survived five years of congressional wrangling and another five or so of precarious budgets and then finally took root—a phenomenon that might be counted a miracle at least in terms of the theory of continuous creation.

The problem with either of these notions is that during this same period every major industrial nation has acquired an organization of some kind with the responsibility for supporting basic research. All these countries, moreover, devote about the same proportion of national resources, measured in relation to gross national product, as the United States does to basic research. The reason, in each case, is the same: the fear that by failing to support some basic research the country will jeopardize the future strength of its economic and military capacities. If the American achievement is a miracle, even on a minor scale, it seems to occur with remarkable frequency and at rather predictable orders of magnitude elsewhere as well. The really interesting question about the NSF, which this comparative perspective suggests, is how it differs in structure and function from its counterparts in Britain, France, Germany, Canada, and elsewhere. Now that there is such a wealth of comparable experience against which to measure our own, there can be no convincing justification for continuing to examine the American case as though it were still *sui generis*.

Even from a more parochial domestic perspective, it is impossible to appreciate the impact of the NSF on science and higher education, for better and for worse, without paying much more attention than Lomask does to the ways in which the Foundation provides its support. In discussing the origins of the decision to adopt the grant rather than the contract as the primary instrumentality, Lomask correctly notes that this decision was made by Waterman and his aides, who themselves had come from the Office of Naval Research, where contracts were used exclusively, because their studies and inquiries showed that the grant, with its greater freedom and flexibility, was more appropriate. He fails to point out, however, the important precedent established by the private foundations, which had been the principal supporters of science before the federal government assumed this responsibility. Without this precedent, it is questionable whether any of the federal agencies supporting research would have

been able to avoid the use of contracts, since these were standard in all types of procurement involving nongovernmental agencies.

More important, by adopting the project grant, coupled with peer review, the NSF was able to channel the bulk of the support it received into the laboratories where—in the opinion of the best-qualified specialists in each field—it would yield the best results. In other words, the technique has enabled the scientific community to support the researchers it has judged best qualified even though the funds have been appropriated by a representative system well known for “pork barrel” politics—or, to put the case in its best light, for a concern with equitable distribution among regions, institutions, and qualified individuals. So sacred is the project principle in this country that even when institutional grants have been tried out, they have generally been “formula” grants tied to the degree of project support. Except for the effort in the 1960’s to create new “centers of excellence” and for certain of the fellowship programs, notably the National Defense Education Act, which was formulated by Congress, the project system based on peer review has been the preferred mode of support.

Much has already been written about the consequences of this system for science and for the university system. It remains debatable, however, whether the Foundation is not too much committed to this system both for its own political good and for the good of the research universities. In a period in which the universities must adjust to diminishing levels of support both for research and for education, it is open to question whether exclusive emphasis on the project system does not provide too much autonomy to the individual researcher and too little leverage for the university administration to consolidate and trim existing activities and open up new ones. It is even questionable whether, in view of the pressures the universities are now under not only to cope with overextensions induced by government support but also to endure the burdensome costs and interference with academic freedom due to the vogue for “accountability,” their degree of dependency upon the NSF and other government agencies is as healthy and as welcome as it was once almost universally considered.

No history of the NSF that so neglects its impact on its principal institutional clients can be considered even minimally adequate. A serious historical effort would also have to inquire into the links between the project system and the

American attachment to the liberal ethos, with its traditional emphasis on equality of opportunity and the more recent shift to a stress on equality of results. It would also have to discuss both the remarkable benefits of peer review and the criticisms that have been made of its real and imagined weaknesses.

None of these subjects is explored at all adequately in this account. In fairness to the author, it should be stressed that he was evidently commissioned to compose the sort of tale he has in fact told—an “informal” history blending “selected” important information with lighter material in an account that a wide variety of readers would find palatable. This he has certainly done, and done well, by the usual standards of such work, and it would be churlish to criticize him for not writing another, more serious kind of historical study. The Foundation, however, is not beyond criticism for soliciting this kind of scrutiny when it could have defined the need differently. Miracle or not, the NSF is one of this country’s most important instruments of self-government. As such, from time to time, it deserves and should encourage searching examination of its structure and function. Periodic check-ups of this sort are at least as valuable as a birthday greeting, however well deserved.

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Curricula and Political Conflict

Science Textbook Controversies and the Politics of Equal Time. DOROTHY NELKIN. MIT Press, Cambridge, Mass., 1977. xii, 174 pp. \$12.50.

Aristotle, Copernicus, Galileo, Lamarck, and Lysenko were all individuals whose scientific theories were branded acceptable or heretical according to ideological circumstances. Dorothy Nelkin has written a book that reminds us that examples of such politicization need not be drawn from another culture or another era. More than a century has passed since the publication of the *Origin of Species*, and 50 years have elapsed since Clarence Darrow defended Darwin’s views against a literal interpretation of the Bible in the famous trial of John Scopes. Nevertheless, the theory of evolution is still not universally accepted in this country. Evolution has come to be a *sine qua non* of modern science; yet public school teachers are not