Articles

The National Institutes of Health: Some Critical Years, 1955–1957

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It has been my pleasure to participate in the conversion of a small but superb federal institution into a driving force for the development of excellence in the nation's biomedical sciences. The initial step was the establishment of an adequate science base for the developing enterprise. Given this, it was agreed that the nation's medical establishment could use the evolving support system effectively. What follows is a brief consideration of some events contributing to the reduction of a possibility to reality. Much of the material that follows was derived from a presentation to a presidential commission established by President Gerald Ford in 1975 and later published as a supplement to the *Journal of Medical Education* (*1*). The latter encompassed what happened during the critical years of development. But it seemed too recent at that time to discuss with grace the "how" of program changes. It is the "how of things" that will be treated in this article.

DURING THE LATE 1800S, THE UNITED STATES HAD FEW medical scientists of note, and laboratory resources were scarce. Yet the threat of cholera and other epidemic diseases in the many port areas of the United States was very real, sufficiently so to stimulate the Marine Hospital Service to provide a "Laboratory of Hygiene" within the Staten Island Marine Hospital for studies relating to the problem. The laboratory was modest, the mission broad, but the chief, Joseph J. Kinyoun, was well trained; all this took place some 100 years ago, in August 1887.

The Hygienic Laboratory and its parent body, the U.S. Public Health Service (USPHS), were together noted early for their excellence and cordial relations with the Congress. The National Institute of Health (NIH) of 1930 was the derivative of such a background. And with early growth within a responsible USPHS it was ready, in 1945, for serious postwar development. At the time, the Office of Scientific Research and Development (OSRD) and its Committee on Medical Research were closing down their wartime programs. But some projects would be continued by transferring them to agencies with a continuing role in science: the Office of Naval Research for the physical sciences and NIH for those in biomedical areas. This was in 1945. Actual transfer was not noted publicly.

The action was taken with the prospect of a new National Science Foundation, particularly for programs in biology, mathematics, and physics. The medically related programs were assigned to NIH. The National Science Foundation would wait until 1950 for authority and funds to establish its program, defined as the support of basic science. For NIH, its programs were well in hand by 1950, within a categorical structure that did not recognize basic science as a definable entity. The mission and structure of NIH had been further defined by the so-called Omnibus Act of 1950 (*2*), the collective mission being the abolishment of major disease. The NIH would use such science as might be expected to further their mission, a simple and clear directive.

The Science Base—1955–1958

A report entitled "The advancement of medical research and education" was presented to Marion Folsom, then Secretary of Health, Education, and Welfare (HEW), on 17 June 1958 by Dr. Stanhope Bayne-Jones, who was chairman of a committee appointed by Folsom about a year earlier. Folsom had been troubled by the very rapid growth of the NIH programs as the result of congressional action on two consecutive budget years. The committee was asked to review the programs of NIH and to relate them to the committee's view of the opportunities and needs of medical education and biomedical research in the United States. Folsom also wanted an assessment of the validity of the basic philosophy underlying the patterns of support espoused by the National Institutes of Health for their further development. He indicated that he was aware of the strong support being given to NIH by Congress, and the effect of this in the congressional budgets of the immediately prior 2 years. But he also said that such support might be more healthy than disturbing. He indicated to the committee that the content of their report would be as pertinent to the further development of NIH as the previous 2 years of strong espousal of NIH by the Congress.

In what follows, we will be concerned with the events that stimulated the report, the views of NIH relating to its own future, and how these views were brought into productive action. The study includes a brief inquiry into congressional attitudes, particularly as they are expressed in reports from appropriations committees. The critical period was from the summer of 1955 through the early summer of 1958. Mr. Folsom accepted the report from the committee a few weeks before he left office. Mr. Folsom recommended to his successor that the report be used as a guide for the further development of the nation's research and education in medicine. In his official acceptance, he said that the report set forth "a philosophy and set of principles that will provide important guides to the development of medical education and research and the research affairs of the Department of Health, Education, and Welfare" (DHEW) (*3*).

To many this may seem to have been a long time ago—as indeed it was, some 30 years. Yet the report continues to be important, less for its detail than its broad espousal of what were the early views of

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both NIH and the Congress on the needs and opportunities of the biomedical sciences at mid-century.

The Summer, 1955

The fiscal year (FY) 1956 budget became operational on 1 July 1955. Mr. Folsom, newly appointed secretary of HEW, came into office 1 August. He was required shortly thereafter to make a proposal to the Bureau of the Budget (BOB) for the operations (FY 1957) of NIH. The level of NIH activity at the time (FY 1956) is summarized in Table 1. It amounted in total to $96.4 million. To some the figure may seem large. But the gross figures had little relevance to the science opportunities it would provide and to the needs of medicine.

In view of the need for new knowledge in medicine, the main deficiency preventing progress was the inadequate funding of research. There was a moderate abundance of well-trained scientists and a considerable number of well-operated medical clinics with reasonable laboratory backup and an abundance of well-defined problems. There was also increasing interest in the university laboratories in the underlying biological systems of concern to both the biologist and the biomedical scientist. Actually, the exploration of DNA was already well under way. The difficulty seemed to be in the scaling of the system. There were manpower and resources, but they were too modest in size because of the inadequacy of support funds. Perhaps this situation was not universally true, but it was sufficiently common to set a pattern that had several missing items. Among these were needs for the following: (i) a realization of how fast and well new science-oriented programs could be mounted, (ii) a definition of the likely cost of programs, (iii) a source of adequate funds, and (iv) a profound conviction that an expansion of the science base for medicine was needed, doable, and should be undertaken with a sense of urgency.

It was equally clear to some of us that success was only possible by breaking out of the confines of the then federal budget for the support of the biomedical sciences. Although resources were then available, this was a temporary situation. A realistic program would have to provide a continuing expansion of the base for scientists' production and an expansion of physical resources to house the expanding programs. The targets seemed clear.

These were my thoughts when the Surgeon General, Leonard Scheele, asked me to accept the directorship of NIH (effective 1 August 1955). In the many discussions we had I expressed the belief that all of the desirable objectives were attainable. I had been at the National Heart Institute as associate director for intramural research and had a comparable position as associate director of NIH. It was quite clear we would have little support from BOB, and perhaps little from DHEW, for the major program expansion that seemed so badly needed. But, from the numerous discussions I had with Senator Lister Hill and Congressman John Fogarty about science and its federal support, it was my conviction that they and the Congress in general were as anxious as I to provide the NIH programs with a realistic base.

So much for generalities. The problem was what to do. My first real problem, as the new director of NIH, was to propose a budget for FY 1957 for DHEW discussion prior to transmission to BOB. We submitted our estimate of what we felt was needed and defensible. It amounted to about $200 million for the coming year. We did this within about a week or 10 days after I was appointed director and without departmental instruction. I wanted to be able to start the discussions at a reasonable level.

The support material noted that much of the NIH program, in its growth during a 10-year period, had been unduly influenced by the circumstances immediately following the war. Too much of the NIH funds were continuing to support an FY 1945–type program and too little to satisfy the real 1956 need. For example, the "training" funds were directed mostly toward medical specialty training in the usual sense and little toward the training of scientists. In our view, a substantial number of the programs set up needed redirection, and others needed to be replaced by new medical and surgical specialty clinics with broad scientific capability. We thought that the cleaning up of this area of activity would not be too difficult since much of the simple physician retraining and completion of interrupted training was finished by the end of the forties. And, support of physicians in training for clinical practice was becoming less desirable than the incorporation of a broad science base in the specialty clinics. Programs in true shortage areas could be continued, for example, in mental health and neurology. We also indicated that the then current programs were sufficiently small that closeouts and priority reordering would not be disturbing, particularly if there were an expansion in other, more productive areas.

Table 1. The operating budget of NIH in FY 1956.

Activity	Budget ($ in millions)
Research grants	38.0
Training and fellowships	17.3
Control and field activities	7.6
Direct operations	29.5
Technical assistance (states)	3.0 to 4.0

The need to face up to such a decision was emphasized by the distribution of training-related funds in the budget for FY 1956. Of the $17.3 million for fellowships and training (Table 1), only $2.5 million, for the fellowship program, was actually and explicitly devoted to the production of scientists for research and educational purposes. Also, of the $96.4-million total NIH budget for FY 1956, about $10.0 million was for technical assistance. The $10.0 million was made up from $7.6 million budgeted for control and technical assistance, and $2.4 million for administrative costs. Manpower-rich programs were buried in direct operations.

The figures were a reflection of a steady growth of the initial extramural programs derived from OSRD. But the growth had been surprisingly slow. By FY 1956, the cost of these programs was something less than $40 million, hardly an adequate expression of support for a broad national need. With these thoughts in mind, we submitted a budget proposal that sought the following:

1) Closeout or transfer of programs not related to science.
2) Expansion of scientific manpower production.
3) Provision of funds for a substantial number of new projects.
4) Broad expansion of selected existing programs.
5) Marked increase in the proportion of grants with long-term support.
6) Provision of a means to stabilize academic careers.
7) Provision for new construction on a substantial scale.
8) Exploitation of the full capabilities of the Bethesda, Maryland, facility.

This was a serious, though informal, proposal from me to the secretary of HEW, sent on by Dr. Scheele with PHS approval. The cost of the proposed program, including the ongoing activities that were retained, was carefully priced at $200 million for the first year. The secretary's response was quite prompt. Folsom suggested a general discussion and an examination of each item. He was courteous and most pleasant, but we left the meeting without a substantive discussion of the submission. He indicated later that he would seek external advice. In the meantime, he hoped we could

continue our program discussions. About a month later, after five or six program discussions and NIH site visits from two sets of consultants (largely industrial but with some academic representation), he indicated that he had excellent reports on our organization and programs and our views of the opportunity of the times. He was convinced that what we had been proposing made good sense. But, he said, "Let's take it in three bites, and if we can, then I think I can convince the President." As an upshot, an increase of some 30 percent in the upcoming budget would be proposed and no less for each of the next 2 years. Also, money saved in closing out programs would be available for reprogramming.

All things considered, I thought his proposal was reasonable, but I was equally certain that Hill and Fogarty would provide substantial increases. It was indeed up to the Congress—what would they do?

As I look back on those meetings, we talked of many things. I learned a lot about Folsom and he, a great deal about me. He was particularly interested in my career, the wartime programs (the malaria work for OSRD), the earlier days of the thirties and how a young instructor got along in the Depression, then the industrial experience at the Squibb Institute, and finally my adaptation to a federal environment. I always came away with the feeling that his contact with medicine and its science base was an intriguing aspect of his position at DHEW. Such a fine person.

Appropriations

Normal contact of an agency with the Congress is through its committee structure for either budgetary or legislative action, each agency being responsible to specific subcommittees. Each legislative body has its own committee structure, and each subcommittee is serviced by a small bipartisan staff. The staff prepares the ground for hearings, collects pertinent information, and, by direction of the chairman, prepares a report of the hearings. In the case of repetitive hearings, the senior clerk serves as the chairman's primary agency contact before, during, and after the actual hearing. The reports of committees are the congressional source of information about the agency. It is not usual for an agency, at the organizational level of NIH, to have contact with more than a single staff person on a continuing basis. Such a person has, as his prime responsibility, the satisfaction of the needs of the chairman and is commonly a simple extension of the chairman in many matters.

On the House side, Robert Moyer was the chief clerk of the House appropriations subcommittee. A conservative, yet he followed directions from his chairman, John Fogarty, regardless of his own convictions. Initially, Moyer was uneasy about my relations with his chairman. It was this general attitude that led me to give John Fogarty much more detailed information than was in general given to Senator Hill. In critical matters, as the preparation of a report, Moyer was initially loath to use the written material we had prepared for the committee; but later he did use the material in much the same fashion as was done on the Senate side.

On the Senate side, the clerk was Herman Downey, an older man who was rather opinionated. I never knew his educational background, but he was extraordinarily competent, permitting Senator Hill to give him quite a free hand in how a given determination was executed. Fortunately, I came to know him well long before I dealt directly with him on the budget. He was a severe diabetic whose illness had not been managed too well, and he was convinced he knew more about the handling of his disability than any physician he had seen in a 10-year period. But, I had grown up within the developing diabetic field beginning in the twenties, and was quite competent. I told Herman his attitude toward his disease made little sense and that he was likely heading for trouble. Discussion of his disease continued for several years, until I finally told Herman I was finished with further comment on his health problems. He said "Fine," and we never talked about it again.

Table 2. Summary budgets of the National Institutes of Health.

Fiscal year	Appropriation ($ in millions)	Construction ($ in millions)
1956	98.46	
1957	183.01	30.0
1958	211.18	30.0
1959	294.38	30.0

But his disability had indeed served an incidental purpose. It created a situation about which we could argue without rancor so that his contentious attitude was sated by discussions of his diabetes, rather than nit-picking on the NIH budget when much later I was director of NIH. In preparation for the hearings, he felt my discussions with Hill were highly effective in helping the senator understand the gamut of our needs and problems and how fast the programs might evolve. Herman was opposed to the way lobbying groups were conducting their interaction with Senator Hill. He perceived their requests to be unending pleas for more money for NIH without at the same time giving Hill a realistic feeling of program needs and progress. Though rough at times, Downey was effective in helping Hill provide a rational base for the rapid expansion of the programs of NIH.

I had little experience with the appropriations subcommittee while I was at the National Heart Institute but became involved with it in the 1952–1955 period as an associate director of NIH. I did have continuing contact with John Fogarty, though more on a personal than a technical basis, and had come to know Senator Hill quite well through his interest in the clinical programs as NIH prepared for the opening of the new Clinical Center. Both legislators knew me as a physician with an interesting and diversified experience in science. The Surgeon General, Dr. Scheele, obviously had taken considerable time to inform both Hill and Fogarty of my background in detail, which was most helpful. Fortunately, my early contacts with them were on substantive matters relating to the mechanics and opportunities of science. Over the period I saw a great deal of each. We talked about the deficiencies of American science and its needs for changes in attitude as well as simply for more money. In these discussions, both legislators felt the deficiencies were correctable and the need was for more than simply more money, though each felt additional funding was essential and could be made available. Out of these discussions came the belief that, properly handled, realistic budgets for NIH would be forthcoming. But as the initial move, this had to be made within the Executive Branch.

Shortly after I was appointed director of NIH in August 1955, both Hill and Fogarty called to offer congratulations and to say they were looking forward to working with me in the further development of NIH. Each invited me to drop in to see him as soon as things were stabilized. I thought the telephone calls were nice though not surprising from John Fogarty, who by then I had come to know quite well. Hill's call included a reference to the budget and a comment to the effect that Herman Downey would be seeing me shortly to talk about some budget items that the "citizens groups" were pushing quite hard. I thought all this was quite good as a beginning, but it was a little early for me to talk about budget. I knew my proposals to the department would be quite different from the requests that had come from either the citizen groups or other supporting groups in prior years. I was less concerned with the budget per se, than the development of a conceptual base for the

mission of NIH in broad terms. So, my discussions with Hill and Fogarty were quite similar in content with those I was having concurrently with the secretary of HEW throughout the late summer. My salable item was opportunity, not need.

Hill and Fogarty were as anxious as I for a sound, understandable, and adequate budget for the biomedical sciences in the United States. And I was quite certain that were the need approached in a responsible fashion, my problem would be the provision of an equally sound program. This was the presenting problem, not its financing. Actually, my informal meetings with Hill and Fogarty had gone very well, a good preparation for the hearings.

Later hearings went even better. Both Fogarty and Hill indicated a wish for help in preparing technical aspects of the committee reports, and in defining in dollars what would be needed sequentially in the next few years for a rapidly growing program. As it turned out, my participation in the budget hearings had been going on for the past 3 to 4 years with both Hill and Fogarty and would continue on a very personal level as long as I was there. Such an approach to the formulation of the NIH budget as an instrument of program development would go on apace. But all things, particularly good things, come to an end. John Fogarty died in January 1967, within my last year at NIH, and Hill was to retire some 6 months after I left. They both were great men and I treasure the opportunity I was given to know them so well over a period of almost 20 years.

But to return to the early years of growth and development. The dollar value of the programs can be simply shown in tabular form and total annual budgets for the early years of growth. For all our conversations during the course of a year, we did not ask for specific things in terms of specific budget increases we would like. Rather we discussed long-range goals and the strategy we would use in their attainment, given adequate funding. From several such general discussions would emerge a consensus of what fields could be planned for development, how fast we could move while maintaining excellence, and how long a general program would need to mature. We talked about 2 to 5 years of an immediate program need consisting of grants, fellowships, and resources. Before the hearings began, we had a clear idea of what to shoot for and a good idea of what to expect. The chairmen, on the other hand, had a series of definitive questions that would bring out the character of each program and clearly demonstrate a need and an opportunity, an estimate of the doability of a proposed endeavor. Sometimes these questions would come to me, other times to an institute director. In the latter case we would be certain he was alerted and he would be expected to respond along reasonable lines of development. This the directors did quite well.

Much of my presentation and testimony was less about budget than the evolving science base of medicine and the fields of activity that were rapidly opening, even at the modest support level that then obtained in the early years. These were relaxed sessions and had as a backdrop the mutual conviction that the federal support of the biomedical sciences was then at too low a level to permit truly productive programs. Prior informal conversations had provided a comfortable base for later discussions that would deal with the same topics but with hard dollars as the primary subject of discussion.

Table 2 summarizes the initial years of this period, the base year of FY 1956, and the three succeeding years. Folsom was surprised at the extent of the increase the first year, but then said that if I could recall, he had cut the increase I had proposed by dividing it into three succeeding years, one third of the total in each year's package. And he recalled to me that his two advisory groups had not thought the sum too large. But then, when a second large increase came from the Congress the following year, he really was bothered and felt that he needed further advice.

This time he selected Dr. Bayne-Jones and a superb committee and asked for a full-scale study of the problem. The Bayne-Jones Committee reported to Secretary Folsom a year later in a detailed report of some 82 pages, a mix of general and detailed comment and conclusions. The report praised both the direct and grant operations of NIH and was not disturbed by their size or complexity. With such advice as he could obtain, Secretary Folsom replied to the report as noted earlier and indicated to me how much he appreciated the opportunity to have been a part of such an important undertaking. The report was highly laudatory of NIH and placed a stamp of high quality on its programs, a finding deeply appreciated by the NIH staff as well as by John Fogarty and Lister Hill, who together had brought it all about.

Closing Comment

It would have been unwise in the early days of program development, 1955 through 1957, to attempt to define targets within the complexities of the many chronic diseases of concern to NIH. Rather, the immediate objectives were to increase the order of magnitude of the effort, provide a broader base of understanding of the biological systems involved, and with an increasing knowledge of the natural history of a disease, approach its solution in an opportunistic fashion. From such an approach, major lines of profitable investigation would arise. The fields of concern to NIH seemed at the time ready to burst open, and the situation would be best handled with a very loose rein.

REFERENCES AND NOTES

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2. U.S. Congress. *Omnibus Medical Research Act of August 15, 1950.* 81st Cong., 2nd sess., Public Law 81-692.
3. "The advancement of medical research and education through the Department of Health, Education, and Welfare: Final report of the secretary's consultants on medical research and education" (Government Printing Office, Washington, DC, 1958).

4. Aided in part by the Lucille P. Markey Charitable Trust.