THE STORY OF THE NIH GRANTS PROGRAMS

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INTRODUCTION

Over the past twenty years, I have spent several lengthy segments of time examining the National Institutes of Health, its programs and its accomplishments. I have done so as a working political scientist, an officer of an association of higher education, and an independent analyst and writer. In my first book on NIH, *Politics, Science and Dread Disease,* published in 1972, I picked up the story of America's pursuit of better health through science where the late, great scholar, Richard H. Shyrock had left off in his *American Medical Research,* in 1947. I recorded the general growth and development of NIH in the larger context of the socio-political world in which it operates. In 1978, my second book on NIH, *Research and the Health of Americans,* emphasized my own views of how some organizational and programmatic changes in the government's larger health policy framework could enable the Institutes to be even more effective.

Not until 1986 and 1987, with a grant from the National Institutes of Health (my first) was I able to spend focused time reviewing closely the role of the principal component of the agency for the support of biomedical research and training—the Grants Programs. I had come to know, and admire, some of those persons who helped to create the "new" program just after the Second World War. I was already convinced that the grants programs had been the principal reason for the United States having the largest, most diverse, most productive biomedical scientific enterprise any nation could boast. But until I began this project, I had not been able to explore and record this remarkable story in an ordered way.

The project has been timely in several respects. First of all, 1987 was the one hundredth anniversary of the founding of the Hygienic Laboratory of the U.S. Public Health Service, that small entity to which the NIH traces it origins. 1986 was the 150th year of the National Library of Medicine, the indispens-

able sister institution of NIH. 1986 also marked the fortieth anniversary of the creation of the Office of Research Grants, which shortly became the Division of Research Grants. That step, I would argue, was as much a determinant of what the National Institutes of Health has become as any other single legislative or administrative action in the past century.

Further, the Public Health Service itself has, in the last few years, reclaimed a leading role—largely because of its outspoken leader Surgeon General C. Everett Koop—in public health education and health service vis a vis the most serious health problems of our times: cigarettes, drug abuse, and AIDS. Fifty years ago, the Public Health Service was the lead batallion in the battle against pervasively threatening diseases of that era. Today's Surgeon General must rely more on a bully pulpit than the thousands of public health workers available for service under his predecessors of the 1930s, but he is fortified by a stronger medical research base and has the further, related advantage that most of the public is aware, and appreciative, of the value of research-based information.

To be able to understand the full story of the establishment and evolution of the NIH research grants programs, it was essential that I talk with those persons still with us who had been present at the creation of postwar program, or who came soon enough thereafter to have had a hand in shaping its direction. These included Dr. Ernest Allen, who helped Dr. Cassius J. Van Slyke set up the office in 1946, and Drs. David Price and Kenneth Endicott who joined soon afterwards. The pioneers also included Dr. J. Roderick Heller, the fourth Director of the National Cancer Institute; Dr. Ralph Meader who joined NIH on the same day as Rod Heller, as its Associate Director for Extramural Programs and then went to the National Cancer Institute; Dr. J. Franklin Yeager, an early member of the team at the Heart Institute, some of whose "proteges" help run NIH today; Dr. Ralph Knutti, who came to NIH to help create the new Arthritis and Metabolic Diseases Institute in 1951, as Chief of Extramural Programs, and later served as Director of the National Heart Institute; and Dr. Frederick Stone, recruited by Dr. Van Slyke himself, who ap-

prenticed with Ernest Allen in the Office of Research Grants and later became Director of the National Institute of General Medical Sciences. Their recollections and judgments—including those about ideas, struggles, frustrations, successes and pleasures—are what give fuller flavor and more vital truth to what might otherwise be a bureaucratic history documented in the perhaps firmer but definitely colder, less informing evidence of summary facts and figures. These pioneers are now in the autumn of their lives, but every one of them provided lively, thoughtful and solid accounts of the early period of the grants program and their experiences in it.

One of the best oral histories I conducted was with my longtime acquaintance, Kenneth Endicott, whom I much admired. The interviews with him, in the spring of 1986, turned out to be the last in which he recorded his own personal history and perspective on the growth of the American medical research empire. He died in the late summer of 1987.

Thus this monograph is based significantly on the oral histories I undertook, eighteen of which have been presented to the National Library of Medicine and are available in its History of Medicine Division for use by other scholars, analysts, journalists, and any interested person. The interviews include those with figures of more recent times, such as Dr. James Wyngaarden, current Director of NIH; his predecessor, Dr. Donald Fredrickson, and Dr. John F. Sherman, another old and valued friend, who served as Deputy Director and Acting Director of the Institutes for a number of years, and has added to his insider's perspective a new one from his position as Executive Vice President of the Association of American Medical Colleges. I also had valuable sessions with the current directors of two Institutes, Dr. Ruth Kirschstein of the National Institute of General Medical Sciences, and Dr. Murray Goldstein of the National Institute of Neurological Diseases, Communicative Disorders and Stroke, and with Dr. Jerome Green, a thirty-year veteran of NIH who recently has become head of the Division of Research Grants.

It is gratifying that others had an opportunity to interview, before their passing, Dr. Rolla E. Dyer, Director of NIH from

1942 to 1950, who was principally responsible for NIH's assuming responsibility for wartime research contracts that were still underway when the Office of Scientific Research and Development went out of business at the end of 1945; and Dr. Cassius J. Van Slyke, whom Dr. Dyer brought in as, ostensibly, a part-time supervisor of these contracts. Such interviews were done in the 1960s under the aegis of the Columbia University Oral History Project; oral histories with these important men were conducted by Harlan Phillips and are on file in the History of Medicine Division of the National Library of Medicine.

Even when one has worked as often and as long as I have in an area such as this, it is impossible to collect, by oneself, all needed information or to reconstruct particular passages of events. I am indebted to a number of persons for their help in this regard, including Dr. John Parascandola, Chief of the History of Medicine Division of the National Library of Medicine and his associate, Peter Hirtle; Dr. Jeanne Brand who, as an officer of the NLM and project officer for my grant, and a distinguished medical historian, gave useful and timely counsel at several important points; Dr. Jerome Green and his staff for helping update information on numbers and processes so that the work could be as current as possible; and to Dr. John Sherman of the AAMC who once more, as in a number of earlier instances, helped to explain some historical and programmatic contexts that I was unable easily to discern on my own.

Particular thannks go to my three research assistants, Hans R. Bachmann, Michelle Sotiropoulos, and Tamara G. Strickland. My wife Tamara also served as my good and reliable "blue pencil" editor for the manuscript, as she always does. My assistant, Ms. Sotiropoulos, not only helped on research and did all the typing—beautifully—but also served as liaison with the whole spectrum of distinguished interviewees and other principals essential to the completion of the work.

I am also grateful for important boosts, given at the beginning of this project and along the way, by Dr. Thomas E. Malone, then Deputy Director of NIH and now Vice President for Biomedical Research of AAMC, and by Dr. Donald

Lindberg, Director of the National Library of Medicine. And I want to record a special word of appreciation and admiration for my great friend, Frances Humphrey Howard, also of the National Library of Medicine. Her keen sense of the comparable importance of the past, the present, and the future—and of the ideas and individuals that link them—inspired and nourished this undertaking.

The monograph that follows is, obviously, a history of a program and, because that program has now become a large one and has always been housed in government, of a bureaucracy. It is about science and health, free inquiry and accountability, basic science and disease. It is also a human story of what a few men with ideas and energy, a cause and a support system can do for the good of all. Each of us can identify developments or accomplishments which seem to epitomize "ideas whose time has come." But examination of concrete cases shows that it is the specific application of human minds, hands and hearts to needs and possibilities that truly makes great things happen. This is certainly the case of the Grants Programs of NIH, as I hope the following pages make clear.

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Washington, D.C.
June, 1988

I
The Public Health Service in the Depression Years

In the years of the Great Depression, most everyone knew about the Public Health Service. They were the people who worked on pellagra, malaria and yellow fever, tuberculosis and venereal disease. They worked on these problems in Public Health Service hospitals, many in seaport towns, and sometimes through physicians and sanitary engineers and public health workers employed by the states but supported with federal money.

The Public Health Service was certainly working on the right problems in those times. Pellagra had been common in rural areas of the South through the 1920s; PHS efforts had brought it under control by the 1930s. Dr. Thomas Parran, Surgeon General of the Public Health Service, reported that "malaria was present in the United States in epidemic proportions during the summers of 1934 and 1935 to a greater extent than any other period during the last twenty years."[1]

An extraordinary number of persons were infected with venereal diseases. For most of the decade, more than a half-million new cases of syphilis and gonorrhea were recorded each year. Dr. Parran asserted: "Syphilis ranks with cancer, heart disease and pneumonia as leading causes of death." But yellow fever and malaria still loomed as major public health threats.[2]

Dr. Kenneth Endicott, reflecting on his thirty-five years in the Public Health Service, recalled vividly the situation existing in the country when he was a medical student in the 1930s:

> A huge percentage of the population had syphilis; the general population, not just the Merchant Marines. The

treatment for gonorrhea was improved very rapidly in the mid-'30s with the discovery of sulphanilamide. But syphilis could only be treated with heavy metals until after World War II when penicillin came in. Heavy metals included arsenic and bismuth and mercury. It required weekly treatments for several years. They alternated courses: intravenous arsenic for about six weeks then intramuscular bismuth.

When I was a student in Colorado, the V.D. outpatient clinic operated four days of the week. One day was ladies' arms, then ladies' hips, then men's arms, and men's hips. And you'd spend the whole morning giving intravenous arsenic or shooting bismuth into people's buttocks, hundreds and hundreds and hundreds of patients. At the time I was an intern, out of ten floors in the Public Health Service Hospital, two were devoted to venereal disease.[3]

What the Public Health Service was especially known for in its efforts to combat these afflictions was its outreach to affected, usually poor, populations, bringing standard treatments of the day to those in need. Indeed, everywhere in the country there were public health programs in part supported by the national Public Health Service. Meanwhile, virtually nobody knew about the research going on in laboratories operated by the Public Health Service or about the few grants made to scientists working in laboratories outside government.

In 1938, the budget of the Public Health Service was over $20 million. $6 million was budgeted for its hospitals around the country and $8 million was for grants to states for a variety of public health programs. The National Cancer Institute received $400,000 that year, and the National Institute of Health, $64,000. Of that combined amount, $140,000 was for extramural research grants, fellowships and training, and other programs directly related to the investigation of disease and the scientific components of it.[4] A handful of scientists worked in the laboratory of the National Institute of Health in Washington, continuing a fifty-year old tradition. A few others worked in other sites under PHS supervision, including the station in

Montana monitoring and investigating Rocky Mountain Spotted Fever.

The decade of the '30's was a time of legislative expansion of the research program. In 1930 the Ransdell Act converted the Hygienic Laboratory to the "National Institute of Health", placed new emphasis on research grants, and established a system of fellowships. In that same year, another law gave the Surgeon General authority to investigate the causes, treatment and prevention of mental diseases. In 1935 land was acquired in Bethesda to build a permanent home for NIH, and in 1937 the National Cancer Act was adopted, making provision for grants and fellowships for cancer research as well as for new laboratories for professionals in the Public Health Service's employ to investigate that dread disease. At the end of the decade, in 1939, the Public Health Service was transferred from the Treasury Department to the Federal Security Agency, the predecessor of the Department of Health, Education and Welfare.[5]

Then came war. With the U.S. entry into World War II at the end of 1941 and the national mobilization that followed, the Public Health Service grew again, in traditional directions. The whole nation organized itself; young men—even up to middle age—volunteered in their country's cause. The possibility of serving in uniform in the U.S. Public Health Service instead of in the Army or Navy was appealing to some physicians and other health professionals who wanted to serve but not necessarily to fight. The military itself provided such an option.

Dr. Martin Cummings was in the Army almost from the outbreak of the war, but as a young officer was going to medical school, first at the University of North Carolina then at Yale. He remembered two officers of the Public Health Service visiting the Yale Medical School:

> They came and talked to our class and said, "Hey, we're in the war too and we're short of officers. We have a series of hospitals around the country and we need medical officers." I decided that sounded pretty interesting. They had a hospital in Seattle and one in Staten Island, and one in Boston, so I agreed to have an interview with them. They

> said if I would join the Public Health Service I would be transfered from the Army and I would be assigned to a Marine Hospital. That's how I wound up in the Public Health Service. I went to the Boston Marine Hospital and I was in the PHS from 1944 to 1953.[6]

Others found themselves in the reverse situation. Ken Endicott had joined the Public Health Service in 1939, immediately upon graduation from medical school at the University of Colorado, doing so because one of his professors had been an officer of the PHS and had told him about the enterprise, and more importantly, because he was offered "a handsome stipend of $1,044 per year." When the war broke out, he was a quarantine officer in the Port of San Francisco and was delighted when he received orders to join the Coast Guard:

> But before I could carry out the orders, they were cancelled and they sent me to the penitentiary to do a research project on homosexuals. In Springfield, Missouri, at the medical center for federal prisoners, they had collected 100 passive homosexuals from the military and federal prisons and had them waiting for me there to set up a study to find out if there was something wrong with their sex hormones. So I really started winning the war right after Pearl Harbor, with this research project at a federal penitentiary.[7]

Another assignment was in the wings for Dr. Endicott. Within six months he was transfered to the National Institute of Health to do pathology in connection with research on nutrition and blood formation.

These illustrations suggest the fluid and cooperative manner in which all agencies of the government, civilian and military, and indeed all professions, joined in the war effort. Army officers became Public Health Service officers; PHS officers became Coast Guard officers; medical students switched schools to make room for visiting allied officers whose facilities in Europe, particularly in England, had been bombed. Federally supported research was carried out in PHS laborato-

ries by persons from other divisions of government; a quarantine officer one day became a laboratory researcher the next. The PHS Commissioned Officer Corps had been statutorily authorized in 1889 as a mobile corps subject to duty anywhere upon assignment. But the wartime situation produced new possibilities for such assignments never before dreamed of. One example was that of Dr. Boyd R. Sayers, who, without giving up his PHS commission, was named by President Roosevelt as Director of the Bureau of the Mines.

The pattern of cooperation was so extensive and so varied that it had the air of confusion. And the confusion—of function, of assignment, of who was paying who's salary to do what—masked both considerable general growth in the Public Health Service and considerable stability in its internal research program.

As regards the enormous, almost universal desire of adult Americans to volunteer for the war effort, and the receptivity of the "system" to that desire, Dr. J. Roderick Heller remembered:

> We would take any sort of person into the Public Health Service if he was warm! It was about like that, but not *quite* that primitive. Dentists, medical officers, engineers, nurses, technicians, sanitarians, and public health personnel were either brought in as civil servants or as commissioned officers, depending on their wishes and whether they would qualify.[8]

In 1940 there were probably no more than one thousand commissioned officers in the Public Health Service, a number which increased rapidly for the next several years and reached its zenith in 1945. But because Public Health Service Officers were detailed to assignments of such myriad variety and under the most assorted auspices, the growth pattern was not as striking at the time as it seemed in retrospect. Furthermore, millions of Americans were donning uniforms, and those who were declared ineligible to serve in the military—or the Public Health Service or the Coast Guard—put on the caps and

stripes of the Civil Air Patrol or the Red Cross. All considered themselves to be wearing the uniform of their country in time of desperate need and urgent business and few thought much about specific institutions or branches or corps beyond the war's end.

All the while, the laboratory of the National Institute of Health continued perking along out in Bethesda, altering its work only slightly from earlier objectives or priorities. Dr. Endicott recalled:

> The people interested in infectious diseases devoted a lot of attention to the development of vaccines for tropical diseases or diseases likely to be encountered in combat zones. There was a very active program, for example, in malaria control, and development of substitutes for quinine because the source of quinine was largely cut off by [enemy] submarine activities. So that those who had been involved in the Laboratory of Industrial Hygiene became involved . . . in various toxicológical studies related to the war effort. There was a project on toxicology of DDT; and I remember working on projects on the toxic effects of some of the cutting oils that they were using in war production facilities.[9]

One surprising thing was that many of the basic research people at NIH during the war just kept on doing basic research. The scientific staff were predominantly commissioned officers, and they were part of the armed forces, so they stayed where they were. The exception was an interesting one:

> People, particularly those in the Cancer Institute interested in radiation, radiation biology, radiation injury, and so on, vanished. They were taken to a very secret place in Oakridge, Tennessee and they sort of disappeared from the scene. There they manned the biology division.[10]

NOTES TO CHAPTER 1

1. *Annual Report of the Surgeon General* U.S. Public Health Service, Fiscal Year 1936, p. 2.

2. Id., p. 11.
3. Interview with Dr. Kenneth Endicott, March 16, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 4.
4. The overall PHS budget figures are taken directly from "The Budget of the U.S. Government, 1938." The NIH and NCI figures, including the amounts for grants, were taken from the *1986 NIH Almanac,* inasmuch as these figures have been reordered, by function, beginning in FY 1938, to provide a consistent base for all subsequent years.
5. These landmark dates and statutory authorities have been recorded in a variety of places. I have relied primarily on two: Marian Oakleaf, *History of the Extramural Programs, 1930–1960,* unpublished manuscript, National Library of Medicine, History of Medicine Division; and the *1986 NIH Almanac,* U.S. Department of Health and Human Services, NIH publication No. 865, Sept. 1986, pp. 4–5, 10–11.
6. Interview with Dr. Martin Cummings, May 15, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 2.
7. Interview with Dr. Endicott, Transcript, op. cit., p. 3.
8. Interview with Dr. J. Roderick Heller, March 26, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 3.
9. Interview with Dr. Endicott, Transcript, op. cit., p. 3.
10. Ibid.

II
Research Goes to War

The coordination of the nation's scientific supplement to the war effort was concentrated in a few hands, led by a few experienced individuals. Dr. Vannevar Bush, Science Advisor to the President, was overseer of all science research and development efforts related to the war. The Office of Scientific Research and Development, the principal coordinating mechanism, included as a very important component the Committee on Medical Research, presided over by Dr. A. N. Richards and including the Surgeons General of the Army and the Navy. Sitting in for the Surgeon General of the Public Health Service was the Assistant Surgeon General and Director of the National Institute of Health, Dr. Rolla Eugene Dyer.[1]

CMR's assigned job was that of "mobilizing the medical and scientific personnel of the nation" and "recommending the need for and character of contracts to be entered into with universities, hospitals and other agencies conducting medical research activities . . . related to the national defense."[2] To accomplish this task, CMR set up (with the help of the National Academy of Sciences and its National Research Council) fifty-one committees and panels to review medical needs and scientific possibilities. Members of the committees also specifically reviewed proposed contracts and made judgments about their adequacy before the contracts were let. Altogether, in the years between 1941 and 1947, the committee awarded some $25 million for 593 contracts for work carried out in 135 universities, hospitals, research institutions and industrial firms. The whole effort involved approximately 1,700 medical doctors and 3,800 scientists and technologists.[3]

Some of the contracts focused especially on battletime problems—e.g. aviation medicine and even surgical procedures—but others encompassed broader research, including chemistry

and physiology. In fact, many of the contracts were in areas in which the Public Health Service and the National Institute of Health had expended much of its money and energy in the preceding decade: malaria and yellow fever, syphilis and gonorrhea, mental health. The Committee on Medical Research obviously saw that the laboratory and field work of the 1930s was a boon to solving wartime medical threats: troops fighting in tropical climates multiplied opportunities for malarial agents to do their damage; millions of men in uniform flung across the globe multiplied the possibility of exposure to venereal diseases. Later, as the conflict wound down, those same leaders, along with an increasing number of public officials and some of the citizenry, recognized that the wartime medical experience was translatable to civilian needs.

Work on malaria had continued in-house at the NIH laboratories as well as under wartime contracts. By the end of the war, more than thirty chemical agents against malaria had been proven effective, a feat which some considered to have been a major benefit to the war effort in Asia. But the greatest advance in the treatment of a variety of medical problems was mass production and expanded use of penicillin. This occured at the height of the war in 1943. Soon it was demonstrated that penicillin not only counteracted infections in war wounds and other trauma, but essentially could cure pneumonia, syphilis, and other diverse problems.

Still, in his first postwar report on the Public Health Service, Dr. Parran had to report that, in 1946, respiratory diseases, including influenza and pneumonia, were of epidemic proportion, with more than 68,000 deaths from those two maladies in that year. There were still more than half a million new cases of syphilis and gonorrhea being reported each year, but thanks largely to penicillin, syphilis deaths declined from 14,000 in 1945 to less than 13,000 in 1946. There were almost 53,000 deaths from tuberculosis. Even smallpox, thought to have been permanently eliminated, in part because of the earlier work engaged in and supported by the Public Health Service, reared its ugly head again in 1946, resulting in 80 cases and 19 deaths.[4]

Three threatening new problems emerged. The incidence of

cancer had increased significantly over the preceding decade. Poliomyelitis in 1946 reached its highest incidence—25,000 cases reported in that year—since 1916, when the number reported was 30,000. Third: "There were 33,411 deaths from motor vehicle accidents in 1946, 19% more than in 1945."[5] The end of the war meant the end of gasoline rationing, and more people driving more cars soon put motor vehicle accidents in the top rank of killers.

The practical experience and specific progress from 1942 through 1945 brought about a new philosophical attitude toward government's role in science and health and new optimism about the power of science, particularly organized science. The wartime medical administrators were themselves impressed when they compiled the list of practical results which could be put to use for the civilian population. Beyond penicillin, there had been breakthroughs in gamma globulin, adrenal steroids, cortisone and blood plasma. The public officials who listened to the recitation were awestruck.[6] As Dr. Frederick Stone later put it, from then on "science was spelled with a capital 'S' and research with a capital 'R'."[7]

A specific bit of evidence of the broader optimism was that by August of 1944, the Committee on Medical Research was already considering how, as the United States emerged victorious from war, it might dissolve itself. Dr. Bush, and apparently President Truman, thought the Committee if not the entire OSRD should stay in business indefinitely. It certainly should be kept in place at least until a new national science foundation (and perhaps an additional and separate national medical research foundation) could be created. Nonetheless, the CMR proceeded to consider alternatives to remaining operational.

In August of 1944, after consultation with Surgeon General Parran, Dr. Dyer wrote to Dr. Richards to suggest that the Public Health Service, in light of its experience in a variety of grants-in-aid over the previous decade and in cancer since 1937, had the requisite credentials to continue the wartime contracts if the Committee wished it to. The letter and its promise carried more weight in the context of recent legislative

events. On July 1, 1944, the President had signed the Public Health Service Act (Public Law 78–410), basically formulated by Dr. Parran and Assistant Surgeon General L. R. Thompson, which broadened and reemphasized the authority of the Surgeon General "to conduct and support research into the diseases and disabilities of man." To consolidate the research program, the National Cancer Institute was made a division of the National Institute of Health, and the Surgeon General was empowered to create new organizational entities to help carry out the renewed research mandate.[8]

An affirmative answer did not come automatically. Despite the consensus on the power of science against disease and injury, and on the need for federal support of science in this role, there was very great diffusion of opinion as to how the government should appropriately continue in the arena of biomedical research. Dr. Bush preferred a comprehensive new national science foundation, a formal proposal for which he would draft and the President would submit to Congress in 1945.[9] Dr. Walter W. Palmer and his committee, whom Bush had asked to review possibilities, preferred a separate foundation for medical research. The latter position was also favored by Senator Claude Pepper who chaired the wartime congressional Committee on Health and Education. Of the many voices speaking to the question of how to proceed once hostilities ceased, only the Surgeon General of the Public Health Service and his NIH director urged that PHS/NIH simply take over this function by expanding its existing role and building on its earlier track record.

There being no certainty about the direction of the transition from a short-term system of centralized support to a long-run, open-ended program of government support for medical science, it is no wonder that there was some scientific and bureaucratic in-fighting at the moment, as well as attention to it by political science specialists in later years.[10] Especially in hindsight can we see what a stunning feat Dr. Parran and Dr. Dyer performed, through ingenuity and persistence, in moving from a modest existing program to "the building of a medical research empire," simply by arranging the takeover of a handful of

wartime research contracts and converting them into on-going grants.[11] The NIH grant-making experience was really more limited than they implied; and there was increasingly clear difference between the nature and implications of "grant" versus "contract". But on substance—malaria, venereal diseases—it was a natural transition and transfer.

Much water flowed under the bridge between the time of Dr. Dyer's letter to Dr. Richards of August 1944 until the actual transfer of the contracts at the end of 1945. But on January 1, 1946, NIH found itself responsible for the administration of sixty-six contracts which more than tripled its budget and, more than anyone imagined at the time, positioned it to become the principal federal government vehicle for the performance and support of biomedical research for the forseeable future and beyond.

NOTES TO CHAPTER 2

1. The summary recitation of the work of the OSRD and its Committee on Medical Research is taken largely from my book, *Politics, Science and Dread Disease: A Short History of U.S. Medical Research Policy* (Cambridge, Mass: Harvard University Press, 1972) Chapter II, pp. 15–31. See also Irwin Stewart, *Organizing Scientific Research for War* (Boston: Little, Brown Co., 1948).
2. Chester S. Keefer, "Dr. Richards as Chairman of the Committee on Medical Research," *Annals of Internal Medicine,* Vol. 71, No. 5, pt. 2, November 1969.
3. Ibid.
4. *Annual Report of the Federal Security Agency, 1947,* Section III, "Public Health Service," p. 263.
5. Ibid.
6. Strickland, op. cit., pp. 16–17.
7. Interview with Dr. Frederick L. Stone, April 7, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 9.
8. *1986 NIH Almanac,* U.S. Department of Health and Human Services, NIH publication No. 865, September 1986, p. 11.
9. Vannevar Bush, *Science: The Endless Frontier* (Washington: Of-

fice of Scientific Research and Development, 1945; reprinted, National Science Foundation, July 1960).

10. Dr. Daniel Fox, Professor of History of Science and Humanities at State University of New York, Stony Brook, is one who has traced carefully the bureaucratic tensions and interagency rivalries of this period. Some of his conclusions were presented in a lecture at the School of Public Health, The Johns Hopkins University, March 19, 1986.
11. Donald C. Swain, "The Rise of a Research Empire," *Science,* December 14, 1962.

III
Transitions and Experimentations

Ernest Allen was running a program for the National Youth Administration in Augusta, Georgia, when Pearl Harbor was bombed. The war effort overtook New Deal programs and the mobilization effort found spaces for people—students and professionals alike—who had earlier been involved in the NYA. Indeed, the new national cause hastened the end of many of the "alphabet programs" launched by President Roosevelt to help pull the country out of the Great Depression. The termination of the particular NYA program in Augusta happened at about the same time as the expansion of the effort against venereal diseases. One day, young Mr. Allen received a visit from an important Public Health Service officer, Dr. Cassius J. Van Slyke, who came to explore the possible conversion of a youth program facility into a V.D. treatment center. Dr. Van Slyke was at that moment Assistant Chief of the Venereal Diseases Division of the Public Health Service. To assist in the transition, Van Slyke asked Allen to join the Public Health Service and help take charge of the new program in the same old building. Dr. Allen agreed. The year was 1943.[1]

Two years later, Dr. Van Slyke had a heart attack. Some of his fellow officers, being concerned, tried to find him a position in the Service with fewer strains and burdens than running one of its larger programs.[2] His friend and division boss, Rod Heller, heard that Dr. Dyer was looking for someone to look after the OSRD contracts. There could be nothing strenuous, it was thought, about overseeing 66 contracts, worth about $870,000, which were underway and were due to terminate in six months. The challenge was novel, so perhaps Dr. Van Slyke might be willing to undertake it. Van Slyke demurred. Through Heller, Dyer pressed him "as a brother officer to give him a

hand in time of need." Besides, knowing what his doctor's orders were, he assured Van Slyke that he:

> . . . positively wouldn't have to work more than two hours any day and probably not more than four or five hours a week [since] this was something that was just going to be turned off.[3]

Far from being "just an incidental, part-time, lower-left-hand-drawer of the desk sort of activity," he envisioned, Van Slyke soon found himself putting in twelve to fourteen hours per day. He called his friend Ernest Allen and asked him to come to Washington to help. Early in 1946, with strong backing from Dr. Dyer, they set up the Office of Research Grants of the U.S. Public Health Service.

From that moment on, several equations changed. Dr. Van Slyke began to see matters in a different light. For one thing, while all the contracts were officially to terminate on June 30, 1946, a number of them were of a continuing nature, and it was implicit that they could be extended further given the fact that the work in progress was productive and relevant to persistent national health problems. Dr. Dyer agreed to try to secure funds for the next fiscal year so that much of the "wartime work" could go on.

There was one small problem: whereas there had been some discussion of NIH grant authority, neither the agency nor the parent Public Health Service had explicit authority to enter into contracts. Dr. Parran, a quintessential New Dealer who believed that government should take a large and active role in solving problems and coping with new situations, sailed over the questions of definition and of relevant statutory authorities by calling what had been received and was being overseen by NIH as "grant contracts."[4] But Dr. Parran knew they were research grants, and he expected them, or others like or unlike them, to be continued. In his October 1946 report issued eight months after the "temporary contracts" were transferred, he wrote:

> Under the research grants program, the Service has undertaken to continue many of the valuable medical investigations sponsored by the Office of Scientific Research and Development during the war. The program is now being administered on a permanent peacetime basis, and non-military projects are increasing.[5]

In fact, Parran and Dyer had already proposed a budget and secured appropriations for NIH for fiscal year 1947 of $8 million, of which $4 million was for extramural grants.

The National Cancer Institute, under authority given at its creation in 1937, had been making a few grants each year. Ernest Allen reviewed that program immediately upon arriving at NIH. He found that there were only a few grants in effect. They were being run out of the NCI director's office with one person, Ora Marashino, handling the "program" and with some involvement of the National Cancer Council.[6]

The review of grant proposals by the National Cancer Council was thus an established, if limited, tradition. The Council was made up exclusively of specialists in various aspects of cancer research and treatment, so its oversight was that of any group of experts. Similarly, the OSRD had had groups of scientists review contract ideas and proposals during the war years. The NIH had handled its own review process in earlier years in even more informal ways. Review of proposed NIH grants by the National Advisory Health Council was not tantamount to expert review, for this Council included public health officers and health administrators as well as clinicians and a few bench scientists. Thus one of the first questions Van Slyke and Allen pondered in 1946 was how to insure that, if the grants program should continue and grow, quality would remain high and scientific "bets" be more sure.

The scientific review question, obviously important, also became urgent. The price of penicillin fell sharply at the point that mass production of it became possible in 1946. Consequently, expenditures for some of the existing contracts were reduced, leaving a residual sum of some thousands of dollars which could be used to support other research. Van Slyke and Allen

composed and sent out what Dr. Allen later called "the most naive letter ever to emanate from the national government in Washington." It was addressed to the deans of all medical schools in the United States and conveyed the following brief message: "We have limited funds available for research purposes. If you have investigators who need these funds, let us hear by return mail."[7]

The shedding of uniforms in 1945 and 1946 was as rapid and extensive as the donning of them in 1941 and 1942. By the time the Van Slyke/Allen letter reached the medical school deans, scores of scientists were back at their desks and in their laboratories, brimming with ideas but having only scant resources. The response to the letter was overwhelming. The grants office—the two professionals and their two secretaries—called their friends to ask for help. They procured a copy of *Men of Science* to try to identify researchers working in the same areas as the person for whom the institutions sent the applications. "We would write to three or four of these people and get their opinions on the merit of the proposal. We then took the proposals to the National Advisory Health Council."[8]

The specific and immediate response, of course, took place in the larger context of nationwide enthusiasm for the accomplishments of wartime science. Further, new health problems had been identified, importantly included among them widespread mental disorders uncovered in the course of examinations of inductees and, soon after the war's end, behavioral problems of returning veterans. The overall situation was described by Dr. Parran in his annual report to the Congress:

> This year of reconversion has been a time of unprecedented public interest in the health problems of the nation. Free from the tasks of war, we and America have once again turned our attention to the more fruitful tasks of peace. Having preserved freedom, we are more than ever determined to use that freedom to make our country a better place in which to live. High among the goals which we have set for ourselves in the coming years is the improvement of our national health.

> This new concern with problems of health springs from many sources. In part, it is the result of the conditions revealed by selective service examinations. In part, it is due to dramatic advances made in certain fields of medical science in recent years. Another factor is the experience of our service men and women, millions of whom have known, for the first time, the benefits of comprehensive medical care and preventive medicine. They have returned home with new concepts of what can and should be accomplished in their communities.[9]

Given the general enthusiasm and the specific invitation to apply for grants, it should not have been surprising that the response was so heavy. Within a year, more than a thousand research proposals had been received. From January 1946, when the contracts were added to the NIH portfolio, through August 31, 1947, $10 million was paid by the agency to scientists working in non-governmental institutions.

The ad hoc procedures used in that first year would clearly not be sufficient for the future. For one thing, the half-dozen review groups focusing on traditional PHS concerns—malaria, tuberculosis, hygiene, venereal disease, biology and pathology—would have to be expanded in number and in fields covered. A second round of letters was sent out by the Office of Research Grants, asking distinguished men and women of science if they would serve for a specified time on "study sections" to review grant proposals in important fields. By the end of 1946, twenty-one study sections had been established, peopled by scientists from universities, medical schools and research institutions, with a few additional ones from other government agencies. Dr. Van Slyke was pleased to report that, within a short period of time, "more than 250 leading scientists" were guiding the study sections.[10] Also by the end of 1946, the operations of the (newly named) Division of Research Grants had been extended to include the administration of the extramural research grant programs of the National Cancer Institute and the Division of Mental Hygiene (later the National Institute of Mental Health). When Dr. John D. Porterfield

and Dr. David E. Price joined the Division that year, the professional staff doubled.

As they reviewed that first year's activities, the most striking aspect was that many more grant applications were coming in than there was money to support. Dr. Van Slyke and his team reported to the Surgeon General that considerably more funds could be used the following year and urged him to seek increases. It was a recommendation that the broad-visioned Tom Parran readily embraced. It was he, after all, who in his first year as Surgeon General, 1936, had specified seventy research areas that he wished NIH to explore if it had the funds and the authority. Thus, the report of his research grants team in 1946 permitted him to call for "new programs to emerge from the blueprint stage," particularly programs in mental illness, heart disease, dental caries, and chronic diseases of old age.[11]

Like Tom Parran, Cassius J. Van Slyke was also a man of enormous energy and broad reach. Dr. Allen confirms that, from an early point, Dr. Van Slyke saw an opportunity for good and for growth, an opportunity and spirit which Allen, as a novice in the field of biomedical research, came quickly to share. But no one seemed to realize that the new era was to be one of unlimited expansion and unbounded budgets. Dr. Endicott, who joined the grants division in 1948, remembers asking Dr. Dyer where he thought the program would level off. Dr. Dyer responded: "It will plateau at about $25 million."[12] As it turned out, the $25 million level was reached within two years.

Dr. Dyer had not fully understood the vision and the vigor of his deputy, Dr. Van Slyke, at the point that he made that prediction. Later, when he had taken full measure of the man he had brought to work for NIH in that undemanding, possibly part-time job, he asked an interviewer if he had known C. J. Van Slyke. The response was negative. Said Dr. Dyer: "You've never seen anything like him."[13]

NOTES TO CHAPTER 3

1. Interview with Dr. Ernest Allen, 1986. Transcript (National Library of Medicine, History of Medicine Division) pp. 1–2.

2. Three perspectives on what transpired have been provided by the three principals. See my interview interview with Dr. J. Roderick Heller, March 26, 1986. Transcript (NLM, History of Medicine Division) p. 9; transcript of oral interview with Dr. Van Slyke by Harlan Phillips, 1963 (NLM, History of Medicine Division), pp. 21–22; and transcript of oral interview with Dr. Dyer by Harlan Phillips, November 13, 1963, (NLM, History of Medicine Division), p. 3.
3. Van Slyke-Phillips interview, op. cit., p. 22.
4. Daniel Fox, lecture at The Johns Hopkins University School of Public Health, March 19, 1986. Author's notes.
5. *Annual Report of the Federal Security Agency,* "Public Health Service," 1946, op. cit., p. 236.
6. Transcript of oral interview with Dr. Allen by Harlan Phillips, April 3, 1963 (NLM, History of Medicine Division) P. 4. See also Ora Marashino, comp. "National Cancer Institute Historical Materials," National Cancer Institute, History Division, Bethesda, Md.
7. Transcript of Allen-Phillips interview, op. cit., p. 10.
8. Ibid.
9. *Annual Report of the Federal Security Agency, 1946,* "Public Health Service," p. 231.
10. C. J. Van Slyke, "New Horizons in Medical Research," *Science,* Vol. 104, No. 2711, December 13, 1946.
11. *Annual Report of the Federal Security Agency, 1946,* "Public Health Service," op. cit.
12. Interview with Dr. Kenneth Endicott, March 16, 1986. Transcript (NLM, History of Medicine Division), p. 6.
13. Transcript of oral interview with Dr. Dyer by Harlan Phillips, November 13, 1963 (NLM, History of Medicine Division) p. 3.

IV
A System and a Philosophy

The more than 250 scientists recruited to serve on study sections in 1946 had as counterparts a slightly larger cadre of scientists working in the in-house laboratories of NIH in the early years after the war. Dr. Endicott believed the number to have been about 300. In their collective work, these government scientists spanned all those areas in which study sections for the review of extramural awards had been created. So it was natural that intramural scientists would be asked to help shepherd along study sections in their special fields. In the first couple of years, the executive secretaries—administrative officers—of the study sections were NIH researchers, while the chairmen were distinguished scientists in universities, medical schools, or in some cases, industry.

Study sections met quarterly, usually a few weeks in advance of quarterly meetings of the National Advisory Health Council and the National Cancer Council, and recommended to the councils whether or not a proposed research project application "is acceptable and can be supported by research grant funds."[1] From the first, study section chairmen and the executive secretaries were very important figures in their fields or at NIH. Dr. Van Slyke himself was the first executive secretary of the antibiotics study section (and four others); its chairman was Dr. Hans T. Clarke of Columbia University. Other eminent figures who chaired study sections were Dr. E. Cowles Andrus of The Johns Hopkins University, cardiovascular; Dr. Carl S. Schmidt of the University of Pennsylvania, pharmacology; Dr. Andrew Warren of the Rockefeller Foundation, tropical diseases study section; Dr. John R. Paul of Yale, virus and rickettsial diseases; and Dr. James A. Shannon, Squibb Institute for Medical Research, malaria. Other executive secretaries from NIH included: Dr. Trendly Dean, later to become Director of

the National Institute of Dental Research, dental research; Dr. Endicott, hematology, and later Director of the National Cancer Institute; and Dr. Norman Topping, who would shortly become Associate Director of NIH, viruses and rickettsial diseases.

Two concerns followed the rapid expansion of applications, study sections, and study section meetings. The first was simply the amount of time study section membership and administration increasingly took. Neither administrators like Drs. Van Slyke, Price, Endicott, Dean, and Topping, nor working scientists from the intramural program could keep pace with the ever heavier load and still do their principal jobs well. In fact when the study sections were first created, Van Slyke and Allen had a hard time getting NIH scientists to serve as executive secretaries. "They considered this as a passing sort of thing, this 'give away' program."[2] So Van Slyke served as "exec. sec." to five study sections in the early months.

That attitude soon changed, however, and a second concern arose: that asking working scientists at NIH to oversee study section activity might lead them into the temptation of exerting undue influence, even if unconsciously, on directions to be taken in their own fields, leading to perceptions of conflict-of-interest; or that they might gain an unfair, long-range advantage by gradually benefitting from the ideas of their competitors.

Dr. David Price, who became the third man in the Office of Research Grants just after Dr. Van Slyke and Mr. Allen began it, recalled:

> One of the things that we were always sensitive about, and Dr. Dyer felt very strongly about, was that the grants program ought not to be run by the intramural scientists at the NIH. That was of course the obvious, easy way to go because the program at that point was small and it would have been manageable to have the scientific review done by intramural scientists. But we realized that to do that would place the program in some jeopardy, with people feeling that their ideas were being "stolen" by government scientists. We wanted to keep any accusation of that kind from occuring, so we chose to use outside reviewers.[3]

A decision was taken to recruit, from outside NIH, scientists and other qualified persons to be permanent, full-time executive secretaries. Dr. Dale Lindsay recorded this step in his monograph on the history, organization and functions of the Division of Research Grants: "By the end of 1948, it was recognized that the office of executive secretary of a study section was a full-time responsibility. Qualified scientists were therefore invited to accept appointments as executive secretaries of the DRG staff." The responsibility of the study sections, specified by Dr. Van Slyke, was two-fold:

> (1) to review applications for research grants in their respective fields, approving them, suggesting changes or further study, or disapproving them, and forwarding their recommendations to the appropriate National Advisory Councils; and (2) as scientific leaders, to survey the status of research in their fields in order to discern neglected areas in which research is particularly wanting, and to stimulate the interest in workers competent to undertake needed research.[4]

The apparatus being put in place for the administration of research grants incorporated some very specific principles and convictions. One thing that had especially bothered Dr. Van Slyke about the wartime contracts was that they required a lot of paperwork—quarterly reports on the science itself plus quarterly financial statements. In his first review of the new program he wrote: "In order not to divert the time of the researcher unnecessarily from the actual conduct of research investigation, only annual scientific progress reports are requested." Nor, he added, is it "desired that the preparation of these reports present any long, tedious burden to the investigator". They should simply "contain only such data in a brief, clear and concise manner which would permit the appropriate study section and national advisory council to "be in a position to endorse the grant as it comes up for renewal annually." Henceforth grantees would simply be required to submit "simple

financial reports to show current status of funds . . twice each year."[5]

The bedrock principle asserted and reiterated from 1946 onward was that the U.S. Public Health Service/National Institute of Health research grants program was to be "a medical research program of scientists and by scientists." The basic tenet upon which the program and the scientific method rested, said Dr. Van Slyke, was "the integrity and independence of the research worker and his freedom from control, direction, regimentation and outside interference."[6]

No one denied the efficacy of organized research, focused on targeted directives and even centrally directed, for a short period of time, as in the duration of four-year war. The problem with that approach for peacetime purposes, suggested Dr. Van Slyke, was that "promising bypaths often had to be bypassed."[7] What was needed for the long term, the NIH leaders were convinced, was an unloosing of scientific curiosity and exploration, giving researchers free reign to pursue inquiries and studies *they* thought important. Dr. Dyer put it thus:

> Once the scene shifts from the emergency, . . . emphasis is placed not upon the goal, but upon the scientist pursuing his interest as distinct from bureaucratic control over those interests.[8]

Every leader of every institute, division and office of NIH was inculcated with the same belief. It was held more fervently than a typical philosophy might be. Dr. Robert Felix, who oversaw the development of the National Institute of Mental Health and its transmogrification from the old Division of Mental Hygiene, put the conviction in perhaps the aptest terms: "It is a fundamental tenet of our 'religion' here that research must be free and researchers must be free."[9]

In the new period of expansion, what was needed was unity of guiding principles and consolidation of administrative practice. The National Cancer Institute had become a division of the National Institutes of Health under the 1944 PHS statutory revisions. Beyond the formality of the organizational rela-

tionship, the personal relationship between the directors, Dr. Dyer of NIH and Dr. Roscoe Spencer of NCI, must have been cordial and cooperative, as there is evidence of considerable cooperation and interchange between the two entities. Soon after Dr. David Price was trained in the philosophy and practice of grants administration at NIH, he was assigned to the National Cancer Institute as director of its grants program.

Dr. Price formally organized the Office of Research Grants in the Cancer Institute and set about instituting policies and procedures similar to those at NIH. One of the first persons he asked to help in the new endeavor was Dr. Ralph Meader, a faculty member in the Department of Anatomy at the Yale Medical School who, during the war, had taken over administration of the Jane Coffin Childs Memorial Fund, administered through Yale and focused on cancer research. As Dr. Price summarizes it: "This gave Ralph contacts in the cancer research field that were rather unique. So we got him to work with me part time to help introduce me around, open doors for me, and help peddle the federal money."[10]

Changes and expansion were the NIH reality from then on, but Dr. Meader never forgot the basis on which the aggregate program was formed and continued to operate. He later wrote: "The administration of the research grants program has been designed and modified, as needed, to support competent investigators, to sustain the broad concept of relevance to disease, and to give the investigator maximum freedom."[11]

The Division of Research Grants had two and a half years of experience before the big boom started. In the summer of 1948, Congress passed and the President signed the National Heart Act, which authorized the National Heart Institute and changed the name of the National Institute of Health to National Institutes of Health. Under discretionary authority, the National Institute of Dental Research was established in September and the National Microbiological Institute and the Experimental Biology and Medicine Institute on November 1. Dr. Van Slyke became the first director of the Heart Institute and just about the same time, his old friend and colleague, Dr. Roderick Heller became director of the National Cancer Insti-

tute. With Drs. Allen, Price and Endicott at the Division of Research Grants, and Dr. Meader with Dr. Heller at NCI, a team and a unified system of grantmaking was in place.

In these first years, men and women of talent were placed in novel positions in new organizations, and were drawn from a variety of situations. But most of those first appointed to NIH leadership posts came directly from the Public Health Service, a fact which must have contributed much to the cooperation, cohesion and camaraderie of the early years.

It was by no means a closed club. In addition to Dr. Meader, Dr. Ralph Knutti was soon recruited to the National Institute of Arthritis and Metabolic Diseases from the University of Southern California. Dr. Frederick Stone, the recent acquirer of a Ph.D. degree in biology and a commission in the U.S. Marine Corps, was recruited directly by Dr. Van Slyke from the University of Rochester. Dr. J. Franklin Yeager, a specialist in insect physiology, was invited to come to NIH from the Department of Agriculture's research center in Beltsville, Maryland.

While most of the study section "exec. secs." had formal training and many had earned doctorals, others did not. Olive Meader, who for years had worked beside her husband Ralph in laboratories and other research environments but had no degree in science, was known as a very excellent executive secretary of special studies. There were others—both men and women—like her. Ernest Allen, the former French teacher, seems to have made sure that talent and experience were not denied for lack of formal degrees.

The new NIH recruits were chosen for their scientific knowledge and experience, or their leadership in the field in public health or military medicine, or their administrative capacity, or any combination of these. But it was quite clear that grants administration was to be a major part, and a crucial one, of the new enterprise. And so when administrative experience could not be found extant, it would have to be learned. Dr. Stone remembers clearly and vividly his first conversation with Ernest Allen in 1948. "Administration is a discipline just like science is," said Dr. Allen: "To learn it, you have to start from the ground up, so you will begin by taking requests and ex-

pense accounts just like the young laboratory assistant has to wash bottles."[12] But, in typical Allen fashion, he also alerted the young man that, because he had recently been a graduate student, he could help the Division of Research Grants greatly in making sure the proposed research fellowships and traineeships, which the Division was developing and would administer, accorded with real needs and stated priorities.

Thus the building of a cadre of experienced personnel went in two directions, deep into the Public Health Service tradition of career officers, many with research specialties but most who were broad-based and flexible, and out into the newly strengthened, ready-to-blossom academic centers across the land. The movement of key persons between Institutes helped to insure common experience and unanimity of purpose just as congressional and public enthusiasm for research inspired excitement. By 1950, the team was in place, the premises were established, the purposes rolled easily off all administrators' tongues; the system was ready and rolling. And it was a very good thing, for in that year Congress passed the Omnibus Medical Research Act authorizing two new institutes—the National Institute of Neurological Diseases and Blindness and the National Institute of Arthritis and Metabolic Diseases (absorbing the Experimental Biology and Medicine Institute)—and giving the Surgeon General the authority to establish still other institutes as the need and opportunity dictated. The stage was set for the greatest period of biomedical research growth in the nation's history.

NOTES TO CHAPTER 4

1. C.J. Van Slyke, "New Horizons in Medical Research," *Science,* Vol. 104, No. 2711, December 13, 1946, p. 561.
2. Transcript of oral interview with Dr. C.J. Van Slyke by Harlan Phillips, 1963 (National Library of Medicine, History of Medicine Division) p. 39.
3. Interview with Dr. David Price, May 2, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 3.
4. "The Division of Research Grants of the National Institutes of

Health: Its History, Organization and Functions, 1945–1962," (Washington, D.C.: U.S. Department of Health, Education and Welfare, Public Health Service, 1963) Pub. No. 1032.

5. Van Slyke, "New Horizons", op. cit. p. 561.
6. Id. p. 563.
7. Id. p. 559.
8. Transcript of oral interview with Dr. Rolla E. Dyer by Harland Phillips, February 8, 1963 (NLM, History of Medicine Division) p. 1.
9. Transcript of oral interview with Dr. Felix by Harlan Phillips, February 8, 1963 (NLM, History of Medicine Division) p. 41.
10. Interview with Dr. Price, op. cit. p. 2
11. Ralph G. Meader, O. Malcolm Ray and Donald T. Chalkey, "The Research Grants Branch of the National Cancer Institute," *Journal of the National Cancer Institute,* Vol. 19, No. 2, August 1957, pp. 228–229.
12. Interview with Dr. Frederick L. Stone, April 7, 1986. Transcript (NLM, History of Medicine Division) p. 2.

V
Scientific Entrepreneurs

Freedom for the scientist theoretically might have implied passivity on the part of the bureaucracy. But the pattern that soon developed was much too activist for that description. In fact, it was downright entrepreneurial. With ever-increasing flurries of activity—new institutes, new areas of exploration and support, and new study sections to accomodate them—there was increasing need for direct communication between the scientific community, dispersed as it was in most parts of the country, and the National Institutes of Health. Every early action of an NIH leader—a merely descriptive article in *Science* by Dr. Van Slyke; a simple visit by Dr. Allen to explain a particular program or ask a particular scientist for his help in reviewing it; a responsive call by Dr. Price to a university whose scientists or administrators seemed confused as to how grants were made—was used as a precedent and a building block. Soon all the key players from Bethesda—Institute directors, officers of the Division of Research Grants, and executive secretaries of study sections—were writing articles, visiting schools, making talks. If this business had been formalized, "dissemination of information" and "constituency relations" would have been an explicit part of every bureaucrat's job description.

Dr. Allen recalled that especially if he received a critical letter about the grants program from a scientist of any importance, he immediately went to see him to explain the new program and, often, to get the scientist involved in a study section as well as interested in grant possibilities. Franklin Yeager recalled: "When I went to the Heart Institute in 1948, one of the first jobs that Van Slyke had me do there was spread knowledge of the NIH programs around the United States, visit the universities and medical schools and talk up the grants

program . . . I spent several weeks visiting various institutions all up and down the West Coast, and applications came in as a result."[1] Fred Stone summed it up: "It wasn't anything to travel 200,000 miles a year."[2]

Reports on what grants were made to which institutions for what particular purposes were submitted not just to the Surgeon General and, in turn, to Congress, but were being publicized in a variety of media. To make sure people understood that the National Cancer Institute not only made research awards but also research facilities grants, Dr. Meader and his colleague W. W. Payne described them in a 1951 article circulated to all public and private state and local agencies and institutions having any connection with public health activities. It was absolutely factual in its detail, and it must have been, to any administrator who didn't know about the construction grants, an open invitation to apply for one. The authors encapsulated the origins and purposes of the grants as follows:

> Secular wartime progress in research had quickened public interest in cancer and encouraged the popular hope that cancer might be conquered. The result, following World War II, was an unprecedented increase in funds for research projects and research training . . . However, nationwide expansion of cancer research was slowed by the lack of facilities. To remove this bottleneck to further expansion and provide laboratory space for housing new studies of cancer and utilizing the enlarged force of scientists, Congress authorized the cancer research facilities construction grants program.[3]

The explicit criteria for awarding the grants were two: "One indicated that the funds should go to a few large institutions with well established medical research programs. The other indicated the aid should go to strengthen smaller institutions with limited research resources." Three other factors were involved: most of the grantee institutions agreed, and demonstrated a capacity, to "contribute a large proportion of construction costs;" they assured continued support to research programs to be conducted in the facilities; and they were ac-

tively cooperating in the development of the State cancer control program.

Those making the awards clearly had geographical distribution in mind. They proudly pointed out that "all of the nine United States census regions are represented among the grantees."[4]

To tout the availability of research grants in sanitary engineering, Irving Gerring, executive secretary of the responsible study section, wrote a special article for the journal of the American Waterworks Association that was solicitous and obvious. He and his superiors were concerned that this particular program had not grown very much in a decade since its establishment, and so he wrote: "In proportion to its importance, research in water control activities from the viewpoint of developing fundamental knowledge has made little advance, at least in this program."[5] He spelled out the criteria for grants and more or less urged that grant requests be submitted. And he paid homage to the other guiding principle of the DRG, writing: "Such proposals in almost all instances will reflect the initiative and originality of the investigator."[6]

Council chairmen and members, Institute directors, other study section executive secretaries and members—almost everyone involved in the grants program—wrote similar articles. Ernest Allen even wrote one explaining why some research grants proposals were turned down, thus illuminating the path to more successful applications.[7]

Another means of encouraging expanded scientific activity, in particular directions and special fields, was to convene meetings. Morris Graff, an endocrinologist who was a study section executive secretary for more than twenty years beginning in 1950, described the situation in which members of study sections would initiate inquiries about "areas where they would want to educate their colleagues in what was going on in research in a new area." The study section, in response, would often organize workshops, and pay for them, inviting a handful of known experts. These workshops would, in turn, often lead to larger conferences to which an open invitation was issued.[8]

Dr. Irving Fuhr, another long-time exec. sec., specified such

an instance in the 1950s. He had invited Dr. F. O. Schmidt of M.I.T. to meet with the National Advisory Health Council and describe informally recent advances and new possibilities in biophysics and biophysical chemistry. Council member Mary (Mrs. Nelson) Rockefeller was impressed; at her suggestion the Council made a grant to the biophysics and biophysical chemistry study section so that the study section could organize a national conference to publicize these recent developments and continuing needs.[9]

The Division of Research Grants was as entrepreneurial as the "categorical institutes." Through the 1950s it had the responsibility and the funds to make sure that no important fields were left completely uncovered. The program officers, the study section chairman and members, the executive secretaries—all from time to time proposed particular initiatives. But the DRG leaders and the executive secretaries were often the most active. They, after all, could recommend the creation of new study sections, or issue "Requests for Proposals" in particular fields.

It was from Ernest Allen and his DRG collaborators that Fred Stone says he learned entrepreneurial as well as administrative skills. He recalled an example used more than once when a specific research need was identified:

> You create a study section and give a chairman's grant and you have a heart-to-heart talk with the chairman and the study section to tell them you are convinced that this field needs stimulation. It's an important field, and it ought to be developed, so go out and do it.[10]

NOTES TO CHAPTER 5

1. Interview with Dr. J. Franklin Yeager, August 8, 1986. Transcript (National Library of Medicine History of Medicine Division). p. 5.
2. Interview with Dr. Frederick L. Stone, April 7, 1986. Transcript (NLM, History of Medicine Division) p. 7.

3. R. G. Meader and W. W. Payne, "Cancer Research Facilities Construction Grants," *Public Health Reports,* Vol. 66, No. 24, June 15, 1951, p. 3.
4. Id. p. 5.
5. Irving Gerring, "U.S. Public Health Service Grants," *Journal of the American Water Works Association,* Vol. 27, No. 11, November 1955, p. 1075.
6. Id. p. 1098.
7. Ernest Allen, "Why are Research Grants Disapproved?" *Science,* Vol. 132, November 25, 1960.
8. Interview with Morris Graff and Irving Gerring, July 1986. Transcript (NLM, History of Medicine Division) p. 10.
9. Interview with Dr. Irving Fuhr, July 1986. Author's notes.
10. Interview with Dr. Stone, op. cit. p. 9.

VI
Growth and Balance

The crucial feat of every living creature, whether individual or institution, is to maintain balance while experiencing growth. In the case of the National Institutes of Health, many balance wheels had to be maintained at the same time: assuring the complementary nature of the intramural and extramural programs; maintaining freedom of initiative for scientists and responsiveness to public needs and concerns; keeping fiscal and reporting requirements from being onerous to the investigator while guaranteeing that public funds were appropriately used; supporting research excellence wherever it existed and encouraging the growth of new centers of potential excellence; investing in established scientists and tested methods while not ignoring innovators and innovations.

To cover several of these balancing needs in a systematic way, the pioneers of the grants program quickly added a second step to the first one of expert review—program review. Once a grant proposal was approved by a study section, with the proposed research and the scientist responsible rated important and sound by peers, the Division of Research Grants assigned the proposed project to a particular Institute with whose mission and programmatic scope its content and purpose best matched. The entire proposal, plus a summary sheet written by the executive secretary of the study section recommending it, was provided to members of the advisory council of the Institute and reviewed at one of the council's quarterly meetings.

In those years, councils spent considerable time discussing individual applications, their relevance to the Institute's interest being assumed. Some were deemed by the council to be more important to particular priorities than others, and so that factor, in addition to the grade score provided by the study

section, could cause a reordering of overall scores. In more recent years, the summary sheets are considerably more elaborate and individual applications themselves are rarely read, dissected and discussed by the council. In contrast to the 1950s, in the 1980s the councils move from the general to the particular. They help to establish overall needs and program priorities, then fit proposals into that hierarchy of values and needs. Both the increased complexity of science and the much larger volume of proposals have necessitated this evolution. But the principle of review for program relevance remains established and operationally viable.

Other balancing acts have been harder to maintain on the basis of a single principle or operational mode. From its very beginnings, NIH had the authority to award fellowships. Before 1946, it had done so in the same way and at roughly the same level as its research grants. Ernest Allen discovered in his early days at NIH that the small fellowship program was run by the Assistant Director of NIH, Lucius Badget. There being only a few applicants annually for such fellowships, Mr. Badget reviewed the applications himself and made the decisions as to who should get them, subject to Dr. Dyer's approval. The Division of Research Grants then took over, and the fellowship program grew steadily, although never as rapidly as the grants program.[1]

Dr. Van Slyke recognized full well that to build a national biomedical science research base, more young scientists needed to be encouraged and trained. But he fretted about making individual decisions about individual potential researchers. This was doing business "retail" when what was needed was a wholesale approach.

Fred Stone discovered another problem. The way the fellowship program was actually working in 1948 was that NIH Fellows were being used simply as research assistants, as extra pairs of hands, as cheap labor. But how to get around this problem without thoroughly offending senior scientists in eminent institutions? It had to be done though: "A fellowship was not intended to give the recipient an opportunity to shoulder half the teaching load of the department, but it was given so

that within a normal period of time you could get your degree . . . and carry on the research after your degree in the same or other departments, or in the same or other institutions."[2] Over a three-year period, the criteria for fellowships were changed and strengthened: the reasserted purpose was to support postdoctoral work for M.D.s and doctoral or postdoctoral work for Ph.D.s, and a four-year maximum was established for fellowships. Under the refined rules, a Fellow could not remain a junior sidekick to a senior scientist for an indefinite length of time. With these amendments, the fellowship program budget grew from "a few tens of thousands of dollars" in 1948 "to perhaps $600–700,000 in 1952."[3]

But the effort to support the training of new researchers was still a matter of individual selection and individual review. What was needed was a system for deciding on need, relevance and quality in a way that recognized strengths and needs of institutions as well as of individuals, and which in fact was judged by institutions and not by government bureaucrats. Dr. Van Slyke proposed training grants as the answer. Dr. Jerome Green recalls Van Slyke's reasoning this way: "If we decide that the University of Chicago is a superb place to train pediatric cardiologists, and if we decide that the nation needs pediatric cardiologists, let's give the University of Chicago a grant based on stipends for individuals. Then *they'll* select the individuals."[4] Training grants also included some money for the faculty, funds for the purchase of equipment, laboratory animals and some funds to pay the trainers as well as the trainees.

There was yet another resource considered essential to the building of a national biomedical research capacity: adequate, up-to-date facilities. The National Cancer Institute was the first of the National Institutes of Health to award construction grants. It first did so, as Dr. Ralph Meader has recorded it, with line item appropriations for specific projects in appropriations bills beginning in 1947 (fiscal year 1948).[5] One of the first grants was an emergency award to help rebuild the Roscoe B. Jackson Memorial Laboratory in Bar Harbor, Maine, an important center of research on genetics and other factors in the causation of cancer, which was destroyed by fire. The more general pressure

was the same that produced increases in research grants and fellowships and training grants: "The nationwide expansion of cancer research was slowed by a lack of physical facilities."[6] When the National Heart Institute was created in 1948, it was given specific authority to award grants for facilities, and in 1950 the National Cancer Institute was given similar authority. NCI moved swiftly to employ the authority and within a year had awarded 63 grants totalling over $16 million.[7]

In the mid-'50s, as growth accelerated rapidly, NIH and its component institutes brought back another venerable research support mechanism, the contract. At first, the contract mechanism was used in small ways and in small amounts, for the purchase of equipment, for research animals, or to recruit a particular scientist for a particular mission. Dr. Ralph Knutti, who joined NIH in 1952 as Chief of the Extramural Programs of the new Arthritis and Metabolic Disease Institute and later became Director of the National Heart Institute, remembered both an early instance of a contract and later, larger examples. The earlier example was a contract with Dr. Helen Taussig of The Johns Hopkins University who was commissioned to travel to Germany to investigate the thalidomide tragedy. Her on-site examination and official report cost only a few thousand dollars and resulted in stopping the use of thalidomide in the United States. Later, the National Heart Institute resorted to using the contract frequently and in substantial ways in connection with the launching of the artificial heart program. In this instance, contracts were used in a variety of ways, from agreements with an outside group to organize an important advisory committee meeting to "the payment for research in specific areas of investigation relative to the production of an implantable heart."[8]

The urgency of another new program, the cancer chemotherapy program, prompted its director, Kenneth Endicott, in 1955, to secure specific congressional approval of the use of contracts. It was an altogether appropriate request since it was Congress which, by and large, was putting pressure on NCI for an engineer-directed program in the cancer chemotherapy field. Dr. Dyer's successor, Dr. Henry Sebrell, had asked Dr. En-

dicott to take charge of the chemotherapy program. The assignment was a tough one, Dr. Endicott recalled, because some saw the program as being more "development" than "research," and many thought that was not a role NIH and its component institutes should undertake. Dr. Endicott made accepting the assignment dependent on being given authority to make contracts. His insistence stemmed from having had responsibility during the Korean War for NIH's program in research and development in the field of blood and blood substitutes, when "it was like pulling hen's teeth" to get the necessary components of an overall research and development plan through the study sections.[9]

Even with new and specific authority to make contracts, Dr. Endicott knew the proposed work and the proposed contracts must be approved by peers if they were to be accepted in the scientific community and in the department, where "there was a lot anxiety about it."

> In the chemotherapy program when we started off, I appointed the equivalent of study sections in screening, pharmacology, clinical trials and so on. The staff and these committees decided what it was we needed done, got out requests for proposals and those same committees reviewed them then.[10]

Problems arose, however, because most of the outside advisors on the chemotherapy program were also consultants to pharmaceutical companies, leading to clear possibilities of conflicts of interest. Dr. Endicott then abolished the external committees and used intramural committees made up of NIH scientists to review contracts.

What emerges from a review of the panoply of instruments the National Institutes of Health used in support of biomedical research is that whereas the individual project grants—the investigator-initiated proposals—were living, continuing proof of the government's belief in the freedom of scientific investigation, the other means of support reflected other attitudes that were more directive and driving. The National Cancer Institute,

the National Heart Institute, the National Institute of Mental Health were among the most dynamic in asserting priorities and assuming initiatives, but there were other examples as well. Soon after its establishment, the new Institute for Neurological Diseases and Stroke created a nationwide neurological training grant program because a quick review had shown that there were very few full-time neurological medical faculty in all of the United States, a condition that rendered research in neurology virtually impossible. In similar fashion, direct stimulus was also given the fields of biophysics and molecular biology.

Nonetheless, the use of research grants has remained the principal, as well as the most revered approach from 1947 to the present day. By far the great bulk of awards has been to individual researchers who applied for grants; by far the greatest amount of dollars NIH invests in biomedical research is through research grants. Still, from the beginning to the present, the idea and practice of supporting individual researchers has been balanced with the need to build support systems. So while the pioneers and their successors had firm principles and, usually, clear priorities, they knew that their success in maintaining all the right balances depended on, more than anything else, flexibility.

Dr. Martin Cummings, while serving as NIH Associate Director for Extramural Research and Training, remembers a difficult question that arose in 1963. The National Library of Medicine applied to the National Heart Institute, through the DRG, for a grant to develop a computerized information system called "MEDLARS". The appropriate study section had approved it, as had the National Heart Council, but somewhere along the line someone raised the question: "How can part of NIH make a grant to its sister agency?" He consulted an old friend and an old hand:

> I called Ernest Allen [at that time, Deputy Assistant Secretary of HEW for Grants Policy] and said, 'I've got this thing here; it's a judgment call and I can't find any precedent for it.' Ernest reminded me: 'Oh, there is a precedent. NIH, through an arrangement you and I made a long time

> ago, makes grants to Veterans Administration investigators. . . If we can give grants to people at the VA, why can't we give grants to people at the Library?'[11]

Thus the National Library of Medicine got its first computer system through a grant from the National Health Institute.

Perhaps NIH flexibility in making unusual grants was reinforced by the importance of some it had received. Dr. Cummings later discovered that the planning money for the National Library of Medicine's proposed computer-based bibliographic system had been awarded to the Library, in a grant of $50,000, by the Council on Library Resources.[12]

Another outside grant was recalled—indeed never forgotten—by Dr. Robert Felix. The old Public Health Service Division of Mental Hygiene was converted into the National Institute of Mental Health by statute in 1946. As director, Dr. Felix wanted to call a meeting of the National Advisory Mental Health Council, but Congress had failed to make an appropriation for the new institute and so it had no money. Dr. Felix made his plight known to a small foundation, the Greentree Foundation. They responded favorably, and with the grant, Dr. Felix called, and paid for, the first Advisory Council meeting. He is probably still shaking his head:

> It has always been interesting to me that this Institute has given away hundreds of millions of dollars, but we got started with a grant from the Greentree Foundation for $15,000. Later we rounded off numbers bigger than that.[13]

NOTES TO CHAPTER 6

1. Transcript of oral interview with Dr. Allen by Harlan Phillips, April 3, 1963 (National Library of Medicine, History of Medicine Division) p. 35.
2. Interview with Dr. Frederick L. Stone, April 7, 1986. Transcript (NLM, History of Medicine Division) pp. 5–6.
3. Ibid.

4. Interview with Dr. Jerome Green, July 29, 1986. Transcript (NLM, History of Medicine Division) pp. 6–7.
5. R. G. Meader and W. W. Payne, "Cancer Research Facilities Construction Grants," *Public Health Reports,* Vol. 66, No. 24, June 15, 1951, p. 2.
6. Id. p. 3.
7. Id. p. 2.
8. Interview with Dr. Ralph Knutti, May 16, 1986. Transcript (NLM, History of Medicine Division) p. 11.
9. Interview with Dr. Kenneth Endicott, March 16, 1986. Transcript (NLM, History of Medicine Division) pp. 10–11.
10. Ibid.
11. Interview with Dr. Martin Cummings, May 15, 1986. Transcript (NLM, History of Medicine Division) p. 13.
12. Ibid.
13. Transcript of oral interview with Dr. Felix by Harlan Phillips, February 3, 1963 (NLM, History of Medicine Division) p. 41.

VII
Unfriendly Judgments

Parallel with the establishment of the Division of Research Grants of NIH, the formulation of its policies and the consolidation of its administrative structure, debate continued as to the need for a national science foundation. The debate was finally resolved in the National Science Foundation Act, signed by President Truman in May 1950. The NSF was to include a Division of Biological and Medical Sciences. In the same year the President also signed the Omnibus Medical Research Act which esentially permitted open-ended expansion of the National Institutes of Health. The emphasis of the National Science Foundation's medical research program was on "advancing our knowledge and understanding of biological and medical fields." NIH, it was stressed, "conducted and supported research aimed at the care and cure of diseases, including basic research."[1] Obviously, the delineation of function and responsibilities was not crystal clear.

By that time, NIH had a track record, a system in place that was accepted and respected. Still, there was some apprehension on the part of PHS/NIH officers as to what the impact of the new National Science Foundation would be on the NIH program. Even Dr. Van Slyke admitted: "We weren't sure what the National Science Foundation would do." The NIH attitude towards NSF was: "If you want to get funds and do the same thing and leave scientists free, that's fine with us, but that isn't going to stop us because we don't know what you're going to do." Van Slyke concluded: "I met some of those folks and talked with them afterwards, which made me feel pretty well justified that we hadn't stopped promoting our program."[2]

Two years later, however, Van Slyke and his colleagues were more confident:

> I felt that by this time [our program] was so well established that nobody would ever dare to change this type of an approach for the support of science. It wasn't a question of the Public Health Service being the big shot in this thing. It was a question of, 'Does the scientist get his support without bureaucratic meddling?' That was the whole thing, and by 1952. . . our program was six years old [and] I wouldn't have felt at all uneasy if our staff of people who knew how to run this thing had been put in some other agency to run it, something separate from the Public Health Service because they couldn't possibly have changed it. There would have been such an uprising in the scientific community that they could have never gotten away with changing it.[3]

The NIH extramural program kept building. By 1955 (fiscal year 1956) NIH had an appropriation of $81 million, of which $54 million was awarded in grants for research, fellowship, training, and research facilities. These grants were awarded through the eight institutes of NIH.

Yet in that same year, an extraordinary challenge to the extramural program occured. In the middle of January 1955, the first Secretary of Health, Education and Welfare, Mrs. Oveta Culp Hobby, who had been Commander of the Women's Army Corps during World War II, wrote a letter to the president of the National Academy of Sciences asking that the academy undertake a review of all the Department's research activities, particularly the medical research component. Secretary Hobby suggested that the NAS evaluation include the following elements:

> Consideration of the rate of growth of the programs of the Institutes of Health and other research units of the Public Health Service, in light of the responsibilities of the federal government with respect to health, medical and related research; a general appraisal of the present level of support of medical research by this department; careful consideration of the proper balance of effort with respect to the support of basic research and research aimed more directly at the prevention, diagnosis, and care of current

disease, and the relative distribution of effort among the major special fields of health research.[4]

In response, Dr. Alan Waterman cautioned that in view of the short time period Mrs. Hobby had proposed for the report—her "desire for an early review"—that "interim observations will of necessity have to be somewhat limited in scope and validity."[5]

The Secretary's January request and the NAS president's February response were the first steps in the undertaking, and the only ones for six months. The organization of a special NAS committee was not completed until July 1955. It held its first meeting on July 22. The Academy had asked the committee to submit its report in time for the annual meeting of the National Science Board in December 1955, further reducing the time available to the committee. Meanwhile, Secretary Hobby had resigned in the summer of 1955 and Marion Folsom of the Kodak Corporation had become Secretary of HEW.

The special Committee on Medical Research of the NAS was chaired by Dr. C. N. H. Long, former Dean of Yale Medical School. It included Dr. A. Baird Hastings of Harvard who had been a long-time member of the National Advisory Health Council; Dr. Charles B. Huggins of the University of Chicago; Colin McLeod, another distinguished physician, and Wendell Stanley, a distinguished researcher. Other members were Edward A. Doisy, Ernest W. Goodpasture, M.D., and C. Phillip Miller, M.D. Dr. Joseph Pisani was Executive Secretary. The committee established a schedule of two-day meetings every two weeks during the months of September, October and November, in the course of which it would interview senior officials of the Department of Health, Education and Welfare and particularly the bureaus of the Public Health Service. The committee planned to devote the first two months to fact-finding, with the last month left for the formulation of the report and recommendations. It proceeded on this basis and completed its report "on time."

Dr. Dyer had retired from NIH in 1950, but he obviously retained a lively interest in NIH. When he reviewed the report, he immediately calculated that the committee had spent eight

days at NIH, six of which were devoted to the review of the $90-million extramural program. The copy of the report in Dr. Dyer's files is peppered with caustic remarks. In the first place, he was apparently chagrined that despite his being a former Director of NIH, and an available resource, he was never interviewed. A second note was more pointed: obviously the extremely limited time the committee spent in reviewing "a complex, important, sound and well established program" was the reason they came up with "such idiotic recommendations."[6]

The reactions of Dr. James Shannon, Dr. Dyer's successor once removed, similarly emphasized "the short time which the committee had to consider some very complex problems."[7] Two major objections about the report were entered. One concerned the Long Committee's serious reservations as to whether uniformed members of the Public Health Service, who originally constituted the largest portion of the intramural scientists at NIH, should not be replaced by a non-uniformed cadre of specially recruited scientists from the universities. Naturally, the pioneers of the program, virtually every one of them out of the Public Health Service, bristled with indignation at the suggestion that scientists and doctors trained in circumstances like all others but who took positions in university laboratories rather than government ones, were more capable as scientists or administrators.

But the recommendation that drew the strongest and bitterest response from current and former officials of the National Institutes of Health was that calling for "the separation of the extramural program from the National Institutes of Health." It was the opinion of the committee "that the director of NIH, the directors of the various Institutes and their scientific staffs, should devote their whole time and energy to the conduct of the intramural program." The extramural activities—including the teaching grants, fellowships and traineeships—said the committee, had grown in such magnitude and reach that "the time has come when the responsibility for the program, as well as the study of its immediate and future effects on institutions engaged in medical research and train-

ing, should be placed in the hands of a separate authority . . . not under the direction of those responsible for the intramural program."[8] When he read this recommendation, the last of seventeen, Dr. Dyer wrote: "Jesus!" How, he wondered, after recognizing "the unique and successful part that the National Institutes of Health have played in support of medical research both within and without the federal government," could the committee come to that final recommendation. It was, to him, incredible.

Dr. Shannon's official response to the report was sent to the Surgeon General, Dr. Leonard Scheele. The response began "in general agreement with the major objectives held important by the committee," and even in "concurrence with some of the proposals and the wish to see them adopted as soon as possible." But in the main, Dr. Shannon unloaded a multi-faceted attack on the committee report, ranging from his pointed objection to the committee's composition and, repeatedly, to the little time it had. The latter problem had naturally prevented its "comprehending fully the evolution and current substance of many programs or grasping the complex realities of the operation of the federal government in general and a large research program in particular." The committee simply had not been able to absorb realities such as "the time required to explain to the Executive and Legislative Branches any proposals involving substantial program change," or "the range of factors the head of a major department has taken into account in framing the major organization of this department." The basic problem, Dr. Shannon implied, was "that the committee overemphasized the needs of medical schools as the factor which should determine policies governing the operation of the extramural program."[9]

In the end, the Long Committee report got nowhere. The only response it produced, besides the official one by Dr. Shannon to Surgeon General Scheele, was a subsequent request by Secretary Hobby's successor, Marion Folsom, to a task force of consultants headed by former Surgeon General of the Army, General Stanhope Bayne-Jones, to review the Departments' biomedical research activities; and a similar request

by the Senate Appropriations Committee to a citizens committee headed by Boisfeuillet Jones, Vice President for Medical Affairs at Emory University, to provide a similar assessment for the Congress. Both groups gave the NIH—its organizational structure, its allocation of funds, and its leadership—considerable praise.[10] Thus the treatment given the Long Committee report was negative from all sources save the one named by Dr. Shannon: the princes of academic medicine, including the heads of some medical schools.

In fact, for a full decade after the war, the attitude towards NIH on the part of the leading lights of academic medicine was an ambivalent one. They were certainly grateful for the ever-growing grants program, and relatively comfortable with it inasmuch as they participated, directly or indirectly, in its direction and its judgments. A number of them obviously doubted that, as the scientific component grew in size and complexity, it could be perfectly managed by uniformed officers of the Public Health Service who, just a few years before, were injecting bismuth into the buttocks of syphilitics. Hence the call for a new professional cadre of scientists/administrators directly out of the ranks of university departments.

But the graver reservations were about the intramural program. On the one hand, in the early 1950s the NIH intramural program provided more opportunities for serious, long-term research than any place else. Dr. Donald S. Fredrickson concluded, within a couple of years after he graduated from the University of Michigan Medical School in 1949, that the only way he could become a medical scientist from a financial point of view was to join the "vague but promising new creation rising in Bethesda." His interview in 1951 was with Dr. James Shannon, then Director of Intramural Research at the National Heart Institute, ". . . a tall fellow sprawled behind a desk, and barely audible." The result of that interview was that Dr. Fredrickson become one of twelve clinical associates who in 1953 helped to open the Heart Institute's beds of the new NIH clinical center. "This research facility, which placed five hundred beds in close proximity to one thousand laboratories, was to be the marvel of its age."[11]

Dr. Fredrickson had spent four years, since medical school, at the Massachusetts General and the Peter Bent Brigham Hospitals, great institutions which nonetheless "did not prepare me for what I found at Bethesda. There was in this sleepy suburb of Washington a density of talent, freedom of research and intellectual opportunity that may never be equalled." But some of his seniors at Harvard, Mass. General and Peter Bent Brigham thought he was making a great mistake. Dr. Walter Bauer, professor of medicine at the Mass. General, told Fredrickson that he was about to enter "a gigantic federal backwater." Ten years later, Bauer came to the "backwater" to recruit the talent which would be the next generation of the medical and basic science faculty at Harvard.[12]

Dr. Shannon had gotten a similar reaction in 1950 when he was asked by Dr. Van Slyke to leave the directorship of the Squibb Institute for Medical Research and join NIH to oversee intramural research at the National Heart Institute. A colleague, the eminent physician and teacher Dr. Robert Loeb, warned him: "If you go, you'll never be heard of again."[13]

These stories, and the shifting attitudes, could be replicated a hundred times.

NOTES TO CHAPTER 7

1. Stephen P. Strickland, *Politics, Science and Dread Disease: A Short History of U.S. Medical Research Policy* (Cambridge, Mass.: Harvard University Press, 1972), p. 85.
2. Transcript of oral interview with Dr. C. J. Van Slyke by Harlan Phillips, 1963 (National Library of Medicine, History of Medicine Division) p. 35.
3. Id., p. 36.
4. "Medical Research Activities of the Department of Health, Education and Welfare," Report of the Special Committee on Medical Research of the National Science Foundation, December 1955, Appendix, pp. 67–68.
5. Id., p. 69.
6. Rolla E. Dyer Papers, National Library of Medicine, History of Medicine Division, "Long Committee Report."

7. Memorandum to the Surgeon General, PHS, from the Director, NIH, December 12, 1955, Rolla E. Dyer Papers, op, cit.
8. "Medical Research Activities of the Department of Health, Education and Welfare," (Long Committee Report) op. cit., pp. 40–41.
9. Memorandum to the Surgeon General, PHS, from the Director, NIH, op. cit.
10. Strickland, *Politics, Science and Dread Disease,* op. cit., pp. 159–162.
11. Interview with Dr. Donald S. Fredrickson, October 10, 1986. Transcript (NLM, History of Medicine Division) p. 2.
12. Ibid.
13. Recalled by Dr. Donald Lindberg, Director, National Library of Medicine, in conversation on October 20, 1987.

VIII
Leaders in Their Times

In Dr. Dyer's eight-year tenure as Director of NIH he had overseen the creation and development of the Division of Research Grants and had helped to establish three new Institutes: the National Heart Institute, the National Institute of Dental Research, and the National Institute of Mental Health. Shortly after he left office, several new institutes were added, including those dealing with neurological diseases and blindness, arthritis and diabetes, and allergies and infectious diseases. In that period, 1942 to 1950, the NIH annual appropriation grew from $700,000 to $50 million.[1]

For the next five years, NIH was led by Dr. William Henry Sebrell, Jr., another career Public Service Officer who had begun his research career under Dr. Joseph Goldberger, the PHS scientist who had proved that pellagra was due to dietary deficiency and so paved the way for its elimination as a major health problem. Following in the footsteps of his mentor, Dr. Sebrell also became a leading authority on nutrition, making important contributions to the treatment of anemia and cirrhosis of the liver.[2] During his directorship, NIH was basically in the consolidation mode described previously, with many of the major factors and forces shaping its growth and its future operating outside its campus. Principal among them were the development of a strong, bipartisan group of pro-research congressional leaders, who increasingly ignored the hierarchies in the Executive Branch of the government as well as its own, and dealt directly with the leaders of NIH. The third partner in the new trilateral relationship was an assortment of independent citizens, including some biomedical professionals who were also interested in the development of a dynamic and forceful biomedical research enterprise and in waging war on diseases.

This group soon came to be known as the medical research lobby. It helped to insure that consolidation of organization and perfection of grant-making systems was accompanied by continuing fiscal growth.[3]

By the time of Dr. Sebrell's retirement in 1955, the NIH budget had gone from approximately $50 million to almost $100 million. That figure, impressive at the time, only a few years later seemed like peanuts. For the next decade turned out to be even more dynamic and expansive than the last, and the National Institutes of Health became the most important biomedical research institution in the world.

The triumvirate of forces guiding and governing the national biomedical research enterprise got perhaps its greatest opportunity from the sympathetic successor of Secretary Hobby, Marion Folsom. A man who understood research from his days at the Kodak Corporation, and who, in the eyes of his Administration colleagues and peers, understood budgets from his days as Under-Secretary of the Treasury in the first Eisenhower Administration, Folsom believed that the Eisenhower Administration, like those of Truman and Roosevelt before it, should support the expansion of the biomedical research effort positively and wholeheartedly rather than conservatively and apprehensively. In the first year of his tenure, he proposed large increases in the NIH budget to the Bureau of the Budget, an act which confounded the budget director, pleased the NIH director, and opened new vistas of opportunity for friends of NIH in Congress and in the private sector.

Events followed this sequence: In the first few years of the Eisenhower Administration, Congress had regularly added $8–$15 million to the President's proposed budget for NIH. Folsom, having been assured by a committee of corporate and university research managers appointed by him that NIH was soundly based and soundly operating, proposed a 1957 budget of $100 million. Senator Lister Hill of Alabama, who a year earlier had assumed chairmanship of the Health Appropriations Subcommittee for the Senate, and Rep. John Fogarty of Rhode Island who chaired a comparable committee in the House, simply asked aloud why they should believe that this

figure would be adequate for the succeeding year. After all, it was little more than the figure they themselves had proposed for the preceeding year and were told was excessive. The net result was that while Secretary Folsom succeeded in persuading his colleagues in the Executive Branch to be more generous to NIH than they earlier had been, he was unable to convince congressional research champions that *whatever* figure the Administration proposed, it would truly meet important research needs and opportunities.

The annual "proof of the pudding" to the Congress was this: As had been the case in 1947 when money ran out before all good research proposals could be funded, in 1956 (and all the years in between) study sections had given high ratings for the competence, and the institutes high ratings for relevance, of scores of proposed projects for which there were not sufficient funds in the budget. From that point forward, the cost estimate of "approved but unfunded" projects from the previous year was the single most persuasive figure of any that the congressional committees saw and used, especially including figures in the President's budget.[4]

Jim Shannon, who became NIH Director in 1955, shared with his predecessors certain important characteristics. He was a superb researcher, having been awarded the Presidential Medal of Merit for his work on malaria during the Second World War. He was a part of the NIH family, having earlier chaired the Malaria Study Section and recently served as Associate Director for intramural research of the National Heart Institute and Associate Director of NIH. He had an additional credential which his predecessors lacked, that of having been research director of a major pharmaceutical firm. And he had the reputation as being a good manager and a prescient planner, which was a good thing because the greatest growth in NIH history took place during the thirteen years of his tenure.

Looking back on the preceding period, it is fair to say that in contrast to the challenges and opportunities facing Dr. Dyer, those of Shannon's time were less a matter of creating and building, and more that of "riding herd" on a dynamic scientific

enterprise which had become so popular and important that the enthusiasm of its friends and supporters sometimes ran the risk of damaging its scientific substance.

Despite periodic problems emanating from the outside, the scientific and administrative leaders of the National Institutes of Health in the 1950s remember those years with special fondness. The positive political environment and the enthusiasm of the scientific community were matched internally by a pervasive spirit of collegiality and high morale. Serious of purpose, those in charge of particular aspects of the enterprise dealt with each other directly, frequently and informally. When a new institute was created, the newly appointed director would meet with his colleagues and, with a little give and take, they would agree to part with some of their projects which had a little more relevance to the new Institute's focus. Within a new institute, new programs were developed in part by patterning them after established ones. Dr. Knutti remembers that the creation of a training grants program for the National Institute of Arthritis and Metabolic Diseases was accomplished quite easily:

> I would tell Dr. Doft that I had studied the programs of the other institutes and their training grants programs as examples, and I had come up with a plan for our institute. I would ask him to look it over and if he said, 'O.K., go talk to Van,' I would do the same thing with Van Slyke [who had moved from the directorship of the Heart Institute to Associate Director of NIH extramural programs in 1962]. Then he'd look it over and say 'Send me a note.' So I would sent it to him through Doft.[5]

In 1956, when an outbreak of scarlet fever and streptococcal disease occured in Puerto Rico, an investigator called Dr. James Watt, then the Director of the National Heart Institute. The conversation, recalled by Jerry Green, went as follows:

> It would be great if we could follow those to see how many and in what pattern, will develop rheumatic fever, if at all." Jim was able to say, "What do you think you need to get

started on that question." When he got the information he telephoned several of his council members and in not much longer than 24 hours, he called this investigator back and said, "Go. You are going to get a grant."[6]

Even executive secretaries had considerable latitude to take initiative in helping new study sections, or organizing conferences, or proposing new emphases, or suggesting persons who should be invited to serve on study sections and councils. In a word, the spirit of those times recognized no rigid institutional, professional or attitudinal boundaries. Further, as Dr. Knutti put it:

> There was no petty competition. I don't know of any institute director in my experience that I didn't assume liked me; I liked all the institute directors and I think they all liked each other. They were broad people. Although their opinions might differ—they might fight like hell about a point—they still respected each other.[7]

The camaraderie and collegiality had one more aspect to it that, in the larger political environment, proved to be a trouble spot. As John Sherman summarized it: "We liked to say that the NIH operated the grants program for about fifteen years before it realized that it was supposed to have some regulations. That's an oversimplification, but not by much."[8]

NOTES TO CHAPTER 8

1. *1986 NIH Almanac,* U.S. Department of Health and Human Services, NIH publication No. 865, September 1986, pp. 20–21, 137.
2. Id. p. 21.
3. Stephen P. Strickland, *Politics, Science and Dread Disease: A Short History of U.S. Medical Research Policy* (Cambridge, Mass.: Harvard University Press, 1972), Chapter III passim.
4. Id. Chapter IV passim.
5. Interview with Dr. Ralph Knutti, May 16, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 5.

6. Interview with Dr. Green, July 29, 1986. Transcript (NLM, History of Medicine Division) p. 5.
7. Interview with Dr. Knutti, op. cit. p. 7.
8. Interview with Dr. John Sherman, September 5, 1986. Transcript (NLM, History of Medicine Division) p. 3.

IX
Success and New Challenges

A conviction increasingly widespread and fervently held was that medical research could eliminate health problems. The corollary conviction was that the biggest enemy of health progress was timidity in providing adequate resources for the war against disease. The budgets lept up in the first couple of years of Shannon's directorship, slowed slightly at the turn of the decade, and gradually continued to inch up for the remainder of his tenure, surpassing the billion dollar mark in 1965 reaching almost $1.4 billion in 1968, the year he retired. But if growth was the principal characteristic of that period, internal management innovations and external controversy were two others. Fortunately, the basic science support mechanisms put in place by his predecessors stood Dr. Shannon in relatively good stead as he faced myriad positive research and development opportunities and several serious political and administrative challenges.

Some of the challenges were more than that; at least three of them constituted serious threats. The first was from a subcommittee of the House Government operations committee, that on Intergovernmental Affairs, chaired by Representative L. H. Fountain of North Carolina. In 1959, the Fountain Committee began an examination of research grants management, to make sure that NIH was meeting its responsibility "for the prudent expenditure of public funds." In 1961, the committee issued its first report finding "that NIH is not adequately organized to administer the grant programs with maximum effectiveness." It offered thirteen recommendations for improving the grants operations of the Institutes.[1]

Dr. Shannon and Surgeon General Luther Terry responded positively, at first, to the Fountain Committee report. Dr. Terry said that he thought "the study and report rendered a service to

the national research effort." Senator Hill and Congressman Fogarty thought the report not very critical of NIH, and so they continued to compete to see which could get his committee, in his house of Congress, to provide the larger increases in the NIH budget.[2] But their congressional colleague proved not to be satisfied with the responsive words. Mr. Fountain quoted approvingly an editorial charging that Fogarty continued to "force-feed the NIH."[3] And he wanted to know in specific terms what Dr. Shannon and his administrative colleagues were going to do to carry out the committee's recommendations. Fountain had in fact posed the question more as a prosecuting attorney than as a non-partisan analyst of a potential problem. But it did not serve to assuage his concerns—indeed it excited them—when Dr. Shannon publicly and forcefully reiterated the cardinal rule of NIH grant-making:

> Selection of good men and good ideas—and the rejection of the inferior—is the key. All subsequent administrative actions having to do with the adjustment of budgets, and so forth, are essentially trivial in relation to this basic selection process.[4]

In its June 1962 report, the Fountain Committee responded equally pointedly:

> The Committee takes strong exception to the view expressed by NIH that all administrative actions subsequent to the selection grant projects are 'essentially trivial' in relation to the basic selection process. The selection process and grant management are essential and complementary parts of NIH research support. Excellence is required of both.[5]

The impact of that exchange might be summed up in a subsequent congressional appropriation. For the first time in years, Congress approved an appropriation of $974 million, up only 5% from the preceeding year. The critical review by a committee of Congress inspired the appointment, by President Johnson in 1964, of a thirteen-man committee headed by

Dr. Dean E. Woodridge to begin "a study of how NIH spends its $1 billion budget, to judge whether the American people are getting their money's worth from the expenditure, and to recommend any changes in organization procedure that would in our opinion increase the effectiveness of the program."[6] In announcing the appointment of the committee, the President's message made reference to the fact that NIH was engaged in "direct financial support of 40% of the nation's health research; a pattern of legal arrangements with more than one thousand universities and medical schools, involving more than 17,000 separate grants; growth by a factor of ten in eight years; an annual operating budget approaching the billion dollar level."[7]

In the end, the Woolridge Committee reported in February 1965 that "the first and probably most important general conclusion is that the activities of the National Institutes of Health are essentially sound, and that its budget of approximately $1 billion per year is, on the whole, being spent wisely and well in the public interest."[8] What was in need of strengthening, said the committee, were the organization and procedures of NIH. The latter observation surely pleased the Fountain Committee.

What pleased Dr. Shannon was not only the general endorsement of his management of the enterprise, but the Wooldridge Committee's praise of it for "making a scientifically inappropriate organization structure an effective arrangement for performing its real mission."[9] This reference was to the increasing tendency of friends of NIH to push a blueprint for growth based on institutes devoted to the conquest of categorical diseases, a tendency which Dr. Shannon resisted with considerable success. Indeed, it might be said that, in his own terms, one of Dr. Shannon's great successes was that in the thirteen years of his directorship only two new institutes were added—one of them being the National Institute of General Medical Sciences with a specific mandate to support broad, basic research.

President Johnson was also reassured, and, in 1966, added $80 million to the NIH budget. Congress added still more and pushed the appropriation, for the first time, over the $1 billion mark.

In the same period, another challenge perceived by Dr. Shannon as even more direct a threat occured. This involved that third grant support mechanism, the contract. As its use expanded, beginning in the mid-1950s, so did controversy about it. Its value was essentially two-fold. In the first place, with the extramural grants program being sometimes propelled in particular directions by Congress and the medical research lobby, and specific research grants being "controlled" by the scientific establishment outside of NIH, administrators at NIH relied on the contract as a means of filling gaps, and prodding efforts in particular areas. It was also quick and easy to use when needs and objectives were clear: the purchase of equipment or laboratory animals, or consulting services. But it was controversial in the academic science community because, it was thought, sometimes researchers who had failed to pass the peer review system subsequently were given contracts to do the same research they had proposed in their grant applications. (Dr. Endicott stated flatly on this point that the reverse was sometimes true as well; some of those who failed to pass the merit review process for a contract then submitted grant applications which were approved.)[10]

Some of the outside lobbyist friends of NIH were pleased that Dr. Shannon and the institute directors had, in their command, the flexible instrument of the contract. And on more than one occasion, a few such friends directly, and indirectly through congressional intermediaries, brought pressure to bear on directors to make particular contracts with particular researchers and institutions. The controversy reached a head in 1963 when, prodded by Dr. Sidney Farber and Mrs. Mary Lasker, chief factotum of the medical research lobby, the National Advisory Cancer Council asked to be able to review proposals for contract work submitted by industrial laboratories. The NIH balked, for there was nothing in the National Cancer Act giving the advisory councils review authority over contracts—merely over grants. On the other side, members of the Cancer Council thought such a right existed in the penumbra of statutory provisions, because their role was, after all, to oversee the whole research effort in the cancer field, especially

that supported through extramural devices. Mrs. Lasker and Dr. Farber took the matter up with their friend Senator Hill, and the Senator's report on proposed appropriations for fiscal year 1964 stated: "All monies allocated in this contractual program shall be spent only after review by the National Advisory Cancer Council."[11]

To Dr. Shannon, this meant that the outside lobbyists were trying to assume control over the internal operations of the institutes. He apparently indicated that, if the contract review issue were not resolved in favor of NIH, he would very possibly submit his resignation. He appealed to HEW Secretary John Gardner who worked out what seemed to be a satisfactory approach, the appointment of a committee. This one, chaired by Dr. Jack Ruina of the Institute of Defense Analysis, swiftly reviewed the contract oversight authority claimed by the council. The committee supported the Shannon position. The Senate desisted, and another crisis was resolved.[12]

But not for long. Mrs. Lasker next took her broader case—that the NIH was not being sufficiently aggressive in producing results and translating research findings into medical solutions—to the President.

On June 27, 1966, President Johnson invited the NIH Directors, the Surgeon General, and Secretary Gardner to the White House for a discussion of how to get research results more quickly translated into practical answers to disease problems. Ostensibly, the President was enlisting the group as a "strategy council in the war against disease." But his central question—whether "too much energy is being spent on basic research and not enough on translating laboratory findings into tangible benefits for the American people"—jarred to the core the assembled scientists and administrators.[13] Alarm was so great, and apprehensive and negative responses in the scientific community so pervasive, that Secretary Gardner convoked a meeting with all NIH consultants, two months after the White House shocker, to clarify the position of the Department. His reassurance was two-fold. First, there was to be no change in the policies supporting fundamental research and, second, that there was not necessarily a "fixed federal health dollar" for

which basic and applied research and delivery of services had to compete. The NIH subsequently produced a report asserting that sixty percent of its monies already went for "applied research" and identifying instances of medical progress resulting from the biomedical research effort over the last two decades. The President, in 1967, helicoptered out to NIH to salute the NIH directorate, staff and grantees, for their "billion dollar success story."[14] But the political climate had changed greatly and would make every recent and subsequent innovation in grants and contracts, proposed by Dr. Shannon and his colleagues, subject to almost automatic skeptical reception and critical review.

As regards other kinds of challenges—positive possibilities—John Sherman recalls two special interests of Dr. Shannon in the early days of his tenure as Director. Both related to his concern about the health of the institutions, as institutions. Research faculties were expanding and faculty members were receiving increasing federal research monies. Shannon was glad to see this happen and "vigorously defended the importance of the project grant system as the keystone of the whole extramural enterprise."[15]

> But at the same time, he was concerned about infrastructure and . . . about help to medical education. He wanted to find some way that the institutions could exert greater control over their own destiny. So he devised the idea of what he used to refer to as the 'general research and training program,' now called the biomedical research support grant.[16]

The idea was that, based on their success in the projects grant system, institutions could be awarded, on a formula basis, additional money over which deans and other administrators could use to balance internally its research activities, its teaching and training program, and its overall strategy, which otherwise might be too strongly influenced by the aggregate project awards. The idea was a thoughtful one, and gradu-

ally became accepted. Yet at first, it created, according to Dr. Sherman, "two sets of tensions.":

> One was within the institution, where the faculty frequently described these funds as 'the dean's kitty' with a considerable amount of resentment. They saw it as a draining off of money from the project grant system. The other was within the NIH, where the institute extramural staff, including myself at the time, felt that this was a threat to the categorical concept and therefore to the individual institute's categorical programs.[17]

A second challenge Shannon took up was that of expanding the nation's overall scientific capability. He was specifically interested in upgrading the group of medical schools and university science graduate programs that were "not quite in the first rank of research-oriented academic institutions" but which, with a modest but continuing infusion of overall support, might well reach the top in a reasonable period of time.[18] It was, once more, a thoughtful concept and ostensibly within the scope of the NIH charter. But it was also one which, as Dr. Sherman recalls, produced great controversy.

There were two problems, one of which was definitional: "This was the first instance in which one couldn't define the process [of selection] clearly; how to set up review criteria for example." The other was, to put it bluntly, the "have" institutions were afraid their institutional support from NIH would be diluted in favor of some arbitrarily selected "have-nots." Dr. Sherman remembers one reaction, typifying many, that occurred in the course of a meeting of the national health advisory council by the president of Ohio State University: "He just gave Jim hell because this was 'a bureaucratically dominated, poorly defined program that was giving money to a favored few'."[19] After only eleven grants were awarded, the program was discontinued because acceptable criteria for selection—the review mechanisms that operated so well in the project grant arena, accepted and controlled by peers—could not be developed.

Other initiatives fared better. Under that broad authority given the Surgeon General and NIH under the 1944 Act, the Clinical Research Center at NIH had been established in 1953. Later in the decade the NIH went back to Congress to secure, for each of its institutes, authority to develop "specialized clinical research centers" in medical schools and teaching hospitals around the country, related to the institute's respective interests. This program simultaneously strengthened the research infrastructure of individual institutions and strengthened the link between research and medicine, a concern of many health science administrators and a preoccupation of all public officials who made public monies available for the great enterprise.

Dr. Murray Goldstein, now Director of the National Institute of Neurological Diseases and Communicative Disorders was, in those years, working with Frank Yeager at the National Heart Institute. He recalls that only a short time after the development of specialized clinical research centers, basic scientists began to ask for "bonus funds" for their work just as their clinical research colleagues were considered to be getting bonus resources through the clinical centers. The first NIH response, in the context of the still dominant individual project award was: "Why would you need it?":

> So they would say, "if I am going to get an electron microscope, I can't justify it on any individual project. We need a central resource for basic research just as much as you need one for clinical research.[20]

It was demonstrably the case that a variety of resources essential to the conduct of basic research was needed, and was needed across a spectrum of activities, not just for one specific project. This was increasingly true as the technology boom—in equipment, instrumentation, and chemicals—continued. So it seemed appropriate that some support should be awarded in larger blocks to scientists and institutions for their aggregate, inter-related work. The problem was that NIH only had authority to make project grants. But ingenuity and innovation

still being very much alive in the collective leadership of NIH, and the purpose being in accordance with one of Dr. Shannon's objectives, in short order the concept and designation "program projects" was decided upon. Once more, the innovation did not come without difficulty. The principal problem lay in deciding who would review and appraise program project proposals. Was it to be the Division of Research Grants through its study sections? Or the Institute and its program officers? The decision was for the latter choice and program projects, with various ups and downs in the interim, continue to the present day.

Aware that success had brought with it an ever more intense spotlight from the public and the people's elected representatives and their surrogates, Dr. Shannon went to special lengths to minimize the possibility that awards would be made simply because there was money to fund them. At one point he established the rule that of the applications approved by study sections, none in the lower ten percent of the grade scores would be funded. Dr. Goldstein recalls: "It was not an absolute rule, but Jim made it clear that the councils would have to take very special action on an individual basis in order to get funding for a grant in the lower ten percent."[21] More generally, Shannon retained and occasionally exercised his right and authority not to make grants, even though a council had approved them, if he thought the proposed science was not sufficiently excellent or relevant. After all, the law read that the director of NIH could make awards only with the approval of the advisory councils, not that he *had* to make all that were approved by the councils.

To restrain the natural and, in an earlier time, healthy entrepreneurial instincts of NIH managers, Dr. Shannon and Dr. Dale Lindsay instituted a new policy that persons involved in scientific merit review were to be divorced completely from program development:

> In the past, it was often true that the executive secretary of an institute review committee was also the person responsible for developing the programs and encouraging re-

> search in certain fields. In a very authoritarian and purist way, the decision was made at the NIH level [by Dr. Shannon] that it was not appropriate for the same person who was developing the grants to be also reviewing the grants . . . so the study sections became divorced from having program direction implications.[22]

Overall, Dr. Shannon's management of NIH and the aggregate biomedical research effort was in itself dynamic. In the manner that outside forces treated him, so he treated his colleagues, staff, council members and grantees: pushing and pulling, suggesting and resisting, initiating then restraining. He asked Fred Stone to develop a training program in biophysics and to work with Dr. F. O. Schmidt of MIT in doing so, then watched over their shoulders and offered cautions and corrections at every turn. He persuaded Martin Cummings to come to NIH from the Veterans Administration, where he was Chief of Medical Research, to start an international program, then became the single most important restraining force on the program's growth. At weekly staff meetings, he so dominated the discussions he had theoretically invited his colleagues into, and so ordered the sequence of arguments, that some thought resulting decisions were always made in advance. In retrospect, they attributed the behavior not to egocentricity nor to intellectual arrogance, but to Shannon's broader view and more ordered thought processes. Most of his colleagues later praised him as a good administrator, strong but flexible, with ideas of his own but interested in those of others. But there was no doubt who was in charge.

Dr. Jerry Green, director of the Division of Research Grants, remembers that when he first joined the Heart Institute's extramural program, proposals and ideas would be sent up to the NIH "front office" and often no formal answers would come back. Occasionally, the response was simply "No," without reason for it:

> On a couple of occasions, having worked very hard on the development of some kind of proposal, perhaps a new

grant program or an increased emphasis in a grant program to be directed at some particular problem, a negative answer would come back, and I'd ask why. The first couple of times I didn't understand. Then a piece of paper would come back saying, 'S.S.S.' I finally found out that that meant, in house, 'Shannon says so!' That would stop all discussion! If Shannon said so, it was virtually not appealable.[23]

NOTES TO CHAPTER 9

1. Stephen P. Strickland, *Politics, Science and Dread Disease: A Short History of U.S. Medical Research Policy* (Cambridge, Mass.: Harvard University Press, 1972), pp. 171–172.
2. Id., pp. 172–173.
3. Id., pp. 172.
4. Id., pp. 174.
5. Id., pp. 176–177.
6. Id., p. 178.
7. Ibid.
8. Ibid.
9. Id., p. 178–179.
10. Interview with Dr. Kenneth Endicott, March 16, 1986. Transcript (National Library of Medicine, History of Medicine Division) pp. 10–11.
11. Strickland, *Politics, Science and Dread Disease,* op. cit., p. 205.
12. Id., pp. 205–206.
13. Id., pp. 207–208.
14. Id., p. 209.
15. Interview with Dr. John Sherman, September 5, 1986. Transcript (NLM, History of Medicine Division) p. 6.
16. Ibid.
17. Ibid.
18. Id. p. 7.
19. Ibid.
20. Interview with Dr. Murray Goldstein, June 3, 1986. Transcript (NLM, History of Medicine Division) p. 15.
21. Id. p. 9.
22. Id. p. 8.
23. Interview with Dr. Jerome Green, July 29, 1986. Transcript (NLM, History of Medicine Division) p. 8.

X
Bureaucracy

> Because of the Fountain Committee and its interrogation and questioning of the administrative basis of decision-making at NIH, all of a sudden a new kind of document was born called "the regulations," where a whole series of "thou shalts" and "thou shalt nots" were written down for the first time as guidelines which had the thrust of law.[1]

To put it another way, by the time Jim Shannon stepped down as Director in 1968, NIH had become a bureaucracy. The name and the fact had been successfully resisted for twenty years, but the agency's success and its size finally forced it into an ancient if not necessarily hallowed tradition. NIH had new institutes and old institutes with new, expanded, and disease-related names. Within the institutes, categorical programs and program staffs multiplied rapidly. In 1969 for example, the National Heart Institute established five distinct extramural program branches: Arteriosclerotic Disease, Cardiac Disease, Pulmonary Disease, Hypertension and Kidney Diseases, and Thrombosis and Hemorrhagic Diseases. In some Institutes, competition among component programs was as keen as it had ever been between Institutes.

The political and governmental environments in which NIH operated also changed dramatically in 1968, presaging a decade of almost continuous alteration, and controversy, and growth. That year, Senator Lister Hill retired from the Senate, thus from the chairmanships of the Labor and Public Welfare Committee, which authorized all legislation pertaining to NIH, and of the Health Appropriations Subcommittee, the combination of positions that for fourteen years had permitted him to put the public's money where his heart was. Representative Fogarty

had already gone to his reward. Richard Nixon replaced Lyndon Johnson as President.

The decade of the '70s encompassed the terms of six Secretaries of Health, Education and Welfare and five Assistant Secretaries for Health and Scientific Affairs. The latter pattern of turnovers was especially important because, in 1968, a reorganization of the Department's health activities gave NIH the status of an operating agency within the Department, so that the NIH director subsequently reported directly to the Assistant Secretary rather than to the Surgeon General of the Public Health Service.

New possibilities for government involvement in the health of the nation had also taken place. Before the Medicare Act of 1965, it was widely thought that just about the only clearly constitutional, hence politically possible role for government in the health field was through research-related grants to individuals and institutions and grants to states for public health programs, including hospital construction. Now, in the decade of the '70s, Congress authorized a whole spectrum of medical education and health training programs, some for a time under the aegis of NIH and most finally lodging, in the middle of the decade, in the separate agency dealing with health services and health manpower. The National Research Act of 1974 amended the Public Health Service Act by repealing the NIH's existing research training and fellowship authorities and consolidating limited authorities in the National Research Service Awards office. After that, individual and institutional training grants were restricted to those areas in which there were specifically and conservatively designated as having "shortages." And where research training was supported, service obligations and payback provisions were to be strictly enforced.

The decade began with the largest expansion of the government's biomedical research effort since 1946: Congress and the Nixon Administration teamed up to pass the National Cancer Act in 1971. This made the Cancer Institute a bureau of equal status with NIH, and made possible subsequent annual appropriations in excess of $400 million for cancer research. That act, and the new bureaucratic and hierarchial arrangements,

were seen by many within NIH and the medical science community as pushing categorization of disease problems to a ludicrous extreme, and force-feeding the already active but necessarily slow scientific effort against cancer, "the dread disease." President Nixon's, Senator Ted Kennedy's, and Mary Lasker's heavy hands drove the new initiative; the Association of American Medical Colleges and Representative Paul Rogers—Senator Kennedy's committee counterpart in the House—were the restraining forces. The Nixon-Kennedy-Lasker combination was an unlikely and, to many, an unholy alliance.[2]

In the same period, the Nixon Administration paid back political friends and Republican fundraisers with government jobs and advisory positions to an amazing and in some cases an alarming degree. A senior personnel officer in the Nixon White House said in the early days of the Administration that the "White House team" considered that there had not been a real Republican President since Herbert Hoover, that the Civil Service and the bureaucracy were overstuffed with Democrats, and that they intended to clean house and fill the ranks of government officialdom, to the maximum, with Republicans. This they proceeded to do, arbitrarily and ruthlessly. Naturally they named "friends," whether or not they had relevant experience and expertise, to the advisory councils of the NIH.

Lay members of the national advisory councils on biomedical research had, in the early years of the grants program, been a matter of concern to many scientists and science managers. But appointed sparingly and selectively, they had gradually proved their worth. Surgeon General Leonard Scheele commented in the late '50s: "The enthusiasm of the lay members is very hard to keep up with. We medical people are very conservative. These people constantly stimulate us and remind us of our responsibility."[3] The Nixon Administration took such appointments to extremes, passing them out like gold prizes to entertainers and local political chairmen. The trend reached almost scandalous proportions, and some medical and scientific professionals with traditional credentials began declining to serve.

In Dr. Robert Q. Marston, Dr. Shannon's successor, NIH had its first director who had not had long-time NIH experience. Dr. Robert F. Stone, appointed in 1973, had had none. Dr. Marston's experience was largely in administration; he had been dean of the University of Mississippi School of Medicine and director of the new (1966) Regional Medical Programs, temporarily housed at NIH. Dr. Stone was a pathologist with several important research findings to his credit and, like Dr. Marston, a medical school dean.[4] As it had been Dr. Shannon's task to ride herd, to keep the procession moving in one direction and protect it from outside forces, it was Dr. Marston's challenge simply to stay in the saddle during a particularly turbulent stretch.

Beyond President Nixon's cancer foray, and his Administration's appointments practice, there was the matter of overall budgets. Except for cancer, he was not especially generous. In his budget director, Caspar Weinberger, he had a man who set out to prove that he very much deserved the nickname given him when he served as budget director for the state of California: "Cap the Knife." He seemed particularly stringent on health and education budgets, in marked contrast to his last years in government in the Reagan Administration as Secretary of Defense. In a different decade and a different position, the man who earlier had sliced budgets became one of the biggest spenders in Cabinet history. Rigid in both instances, his sequential mottos seemed to be "Less is always better for health" and "More is always better for defense."

It was Weinberger who first questioned the need for training grants, then rescinded funds Congress had appropriated for them, then essentially removed NIH's broad authority to make them. The budgetary impact on NIH might have been even worse in the mid-'70s, had not Weinberger, when he became Secretary of Health, Education and Welfare in 1973, appointed more knowledgeable and flexible men than himself as Assistant Secretaries for Health and Scientific Affairs.

The strong impression at NIH and in the scientific community, in the first several years of the Nixon Administration, was that the President was the enemy of good science and

established tradition. What could only be seen in retrospect was that overall the NIH continued to grow. with or without the President's specific encouragement or the direct blessing of the President's men, the budget had expanded by roughly $1 billion during the six and a half years of the Nixon presidency.

If pervasive dependence on regulations is a "negative" characteristic of bureaucracies, the continuing receipt of funds regardless of who the titular leader is, is a positive characterisitc and a sign of continuing, if not inextinguishable life. An equally felicitous symbol of vibrancy and strength for NIH was the ever-increasing numbers of biomedical scientists supported by the institutes who received Nobel prizes.

In 1939 E. O. Lawrence had won the Nobel Prize in Physics; his work had been supported by the National Cancer Institute. It was not until 1950 that the next Nobel Prize winner with an NIH connection was named; he was Philip S. Hench, whose work was supported by the Division of Research Grants. In the remaining years of that decade, there were ten additional American Nobel laureates whose relevant work was supported by seven of the institutes. There were sixteen such awards in the 1960s and twenty-three in the stressful years of the '70s. NIH had clearly become a major element in the international as well as the national scientific enterprise and, in those decades, a driving force in extending the frontiers of science and medicine. In addition to NIH grantees who won the Nobel, four scientists worked in the intramural labs won such awards between 1968 and 1976. Dr. Marshall Nirenberg became the first NIH intramural scientist—and the first U.S. federal employee—to win a Nobel Prize.[5]

Two additional messages were to be found in the growing number and varied sponsorship of Nobel laureates supported by NIH. In the first place, in the early days of the Division of Research Grants, when aside from organizing and administering study sections, it had some funds to "fill in the gaps" left unattended by the categorical programs of the separate institutes, DRG had invested in ten scientists whose work subsequently won them the Nobel Prize. The National Institute of General Medical Sciences supported forty-two Nobelists from

the time of its creation in 1964 to 1987. A number of the twenty-four Nobelists supported by the Cancer Institute and the eighteen supported by the National Heart Institute were given the Prize for their work in what was considered to be more basic than applied areas, thus dramatizing, in another way, the link between science and disease conquest and assuring the scientific community that, despite their focus on particular disease problems, the categorical institutes of NIH operated under no artificially narrow mandates.

A second underlying, very potent message was that the biological revolution was in full sway. Not only were most of the Nobel awards made for breakthroughs in the basic sciences, but such advances opened brand new vistas as to how disease problems should be approached. Dr. Ruth Kirschstein, Director of the National Institute of General Medical Sciences since 1974, asserts that the revolution had an additional critical effect: broadening the scope and requirements of basic science. In her words:

> What I think is most amazing is that a basic scientist who at one time would have been considered a biochemist or a geneticist or a cell biologist is in fact all of these today. All these fields are blending together now. . . So someone who is taking a Ph.D. today in biochemistry will be broadly trained in cellular and molecular biology or genetics.[6]

The biological revolution had another effect, particularly as the possibilities for genetic engineering dawned. It was, in a way, like the dawning of the awful possibilities on those who had helped to create its practical force: an effect of deep concern and apprehension. Very early in that period when the possibilities for good and ill of recombinant DNA technology came to the minds of the scientific community, members of it rasied cautions about NIH support of such research. Gradually, at their initiative and under their scrutiny, guidelines for the protection of human subjects, were developed and instituted. Gradually as well, specific instances of research related to

genetics proved that practical, positive results far outweighed theoretical negative possibilities. So the biological revolution continued, decreasingly hindered by fear.

As the decade moved to a close, a relative sense of calm was restored at the agency and among its constituencies. In 1978, for the first time in a decade since the last collaboration between Dr. Shannon and Secretary Gardner, a Director of NIH had an opportunity to work hand-in-hand with his Cabinet officer superior in a positive, forward-looking way. Secretary Joseph A. Califano asked Dr. Donald Fredrickson to undertake to develop a multi-year strategy for health research. A national conference was held in Bethesda to draft principles for the federal support of such research, then an HEW steering committee worked out a framework for the future. Dr. Fredrickson chaired both. As he later summarized it:

> The major influence of this activity on HEW and its successor, the Department of Health and Human Services, was manifest in the establishment of a goal of funding a minimum number of new and competing research project grants as the first priority in the setting of the annual NIH budget. The keystone of this "stabilization initiative" was the objective of funding five thousand new and competing grants each year.[7]

Fortunately, the stated objective came to be accepted by both the Executive and Legislative Branches and in fact has become a guidepost in the NIH appropriations process from that point to the present.

The Administrations of Gerald Ford and Jimmy Carter treated NIH in a more orderly fashion. Inflation took its toll on the upward-moving budgets, so that the agency's higher appropriations did not automatically translate into greater purchasing power. In that same period, medical schools and universities successfully redoubled their efforts to secure a higher rate of indirect costs. The percentage of grants for administration and other indirect costs climbed from 20.6% in 1972 to 27.8% in 1979.[8] This trend made medical school deans and

university administrators happy and some scientists very unhappy, for they saw it as further eroding the financial support base for actual research. But in contrast to earlier periods, the only strong political pressure in the Carter Administration was for an expansion of the number of women, minorities and younger persons to serve on study sections.

The fact that Dr. Fredrickson's association with NIH spanned more than two decades before he was appointed director in 1975 was reassuring to the scientific community, as was his capacity for elegant statements of scientific position and cogent summaries of fiscal need. It was also reassuring that Dr. Fredrickson seemed to have the support of the new Secretary, at least in the matter of forward planning. But the political dust still had not quite settled when the next Presidential transition occured.

NOTES TO CHAPTER 10

1. Interview with Dr. Murray Goldstein, June 3, 1986. Transcript (National Library of Medicine, History of Medicine Division) p. 9.
2. Stephen P. Strickland, *Politics, Science and Dread Disease: A Short History of U.S. Medical Research Policy* (Cambridge, Mass.: Harvard University Press, 1972), Chapter XII passim.
3. Id., p. 136.
4. *1986 NIH Almanac,* U.S. Department of Health and Human Services, NIH publication No. 865, September 1986, pp. 21–22.
5. Id., pp. 166–167.
6. Interview with Dr. Ruth Kirschstein, April 30, 1986. Transcript (NLM, History of Medicine Division) p. 3.
7. Interview with Dr. Donald S. Fredrickson, October 10, 1986. Transcript (NLM, History of Medicine Division) "Bibliography," p. 4.
8. "Statistical Data—Research Grants: Percentage of Indirect Costs to Total Grant Dollars Awarded." Department of Health and Human Services, NIH, Office of Associate Director for Administration, February 22, 1985.

XI
Contemporary Needs, Changing Numbers, Continuity of Spirit

1986 and 1987 were anniversary years for the National Institutes of Health and some of their component parts. The National Library of Medicine chose 1986 as its sesquicentennial anniversary, tracing its origins to a collection of journals and tracts begun in the office of the Surgeon General of the Army in 1836. NIH marked one hundred years of history in 1987, its lineage beginning with a bacteriological laboratory set up by a Public Health Service officer, Dr. Joseph J. Kinyoun, at the Marine Hospital on Staten Island in 1887. The Cancer Act and the Cancer Institute were fifty years old in 1987; the National Institute of Mental Health reached age forty in 1986. Additional anniversaries have since occurred: the National Heart Institute and the National Institute of Dental Research reached the age of forty in 1988.

Other dates could also be identified as landmarks in the U.S. Government's biomedical research endeavors. The Social Security Act of 1935, in its Title VI, had explicitly authorized the expenditure of up to $2 million for health grants to the states for "investigation of disease" and sanitation problems. The 1944 Public Health Service Act gave the NIH the legislative basis for its postwar program, with reemphasized and broader authority to conduct research. But of all the dates and events that could be singled out as critically important in the evolutionary expansion of a purposeful, concerted, government-supported strategy to defeat disease and enhance health, none surpasses that of the establishment, in 1946, of the Office of Research Grants.

Dr. Van Slyke was very proud of the fact that, within a year after the NIH grants program was launched by him and his colleagues, 250 eminent American scientists and physicians

were involved in advising NIH and, in twenty-one study sections, appraising the scores of grant applications coming in during those early postwar years. He would be astounded to know that, forty years later, more than 2,200 scientists were serving on NIH review groups, including ninety panels of sixty-seven formal study sections of the kind that he and Ernest Allen had set up in the first weeks of their new jobs. Van Slyke was by all accounts a visionary man, but he would surely be amazed to know that, in fiscal year 1987, the Division of Research Grants received 33,804 proposals from scientists and institutions across the country, of which almost 23,000 were reviewed in one or another of the ninety panels of its sixty-seven established study sections.* He would be stunned to learn the size and scope of the activity being served by the system he and his colleages had devised: In 1987, $4.6 billion of the total NIH appropriation of $6.2 billion was invested in almost 28,000 research and training grants in 1,300 institutions around the country. Over 90% of those were research grants, including some for projects led by 20,000 principal investigators.[1]

The nature and mix of health challenges and disease threats have changed considerably in forty years. Cancer still looms large in morbidity and mortality figures. Heart and cardiovascular diseases still rank second in their aggregate toll. Mental health remains a pervasive and complex challenge made more complex in the 1980s by a persistent and perverse problem of illicit drug use by tens of thousands of Americans, many, though by no means all, in low income groups.

In these categories, cancer remained the most frustrating health problem: while progress in detection, treatment and "cure"—the latter measured in five-year terms—improved overall and in some particular categories, the bigger killers such as lung cancer remained dominant over treatment and over education as to causes; a recent study suggests that ap-

(5,800 proposals were reviewed by other units of the NIH, and 4,900 by other review groups in the Public Health Service).

proximately 60,000 out of 82,000 of the recent annual deaths from lung cancer are specifically attributable to smoking.[2] While breast cancer treatment now produces survival rates of 75%, its incidence continues to go up. The National Heart Institute and NIH proudly point to a 43% reduction in the death rate from coronary heart disease since 1972, a staggering figure, attributable to both medical and surgical advances and lifestyle changes, including nutrition and exercise habits, also encouraged by recent research findings.[3] The National Institute of Neurological and Communicative Disorders and Stroke had reason to be gratified by an equally impressive figure: death rates from stroke were down more than 48% in the same fifteen-year period.[4] With respect to those disease problems identified by Dr. Parran in the 1940s as major national health problems, one had clearly been brought under control, if not totally vanquished. Based principally on research and on trials with fluoridation by the National Institute of Dental Research in its early years, dental caries had been reduced dramatically, particularly in younger generations, to the point that the closing of dental schools became, in the 1980s, a common phenomenon.

As always, new health threats arose, the most dramatic one being the frightening spectre of AIDS (Acquired Immune Deficiency Syndrome). Not identified as a specific disease until 1979, exact patterns of transmission were not understood for several more years, yet by 1982 it was clearly an urgent problem. In 1981, the Center for Disease Control had reported 200 cases and 177 deaths—a death rate of 88%. Within seven years, by June of 1988, over 60,000 cases had been identified and almost half of those had resulted in death.[5] The first cases were associated with sexual conduct of homosexual males; the deadly virus was isolated in blood. Later cases included some caused by contaminated blood used in transfusions; others stemmed from rare but dramatic instances of laboratory accidents. There were some women victims and a few newborn infants were afflicted. As the population profiles of those affected became more diverse, public alarm increased.

Even as the nation and the federal government attempted to

organize themselves, in typical disjointed and uneven ways, some actions and accomplishments were encouraging. By 1987, NIH was spending half a billion dollars per year on AIDS research, much of it in-house, and most of it targeted. A number of medical advances, almost all supported by NIH—particularly the National Cancer Institute and the National Institute of Allergy and Infectious Diseases—were already occurring. The drug AZT was proven to retard the spread of the disease in 90% of the patients to whom it was given. Other chemicals were rapidly being tested and tentative vaccines were being readied for clinical trials in the United States and Canada. This progress was heartening, even surprising, given the recency of the identification of the disease.

It helped greatly that the National Cancer Institute had for more than a decade, in the laboratory headed by Dr. Robert Gallo, been working on the viral theory of cancer. Dr. Gallo was thus able to identify a connection, first conceptually and then in laboratory experiments, between the slow viruses thought to produce certain kinds of cancers and the HIV virus (Human Immunodeficiency Virus) carrying AIDS. It added up to another dramatic example of the serendipity of scientific exploration: Work in one area is having beneficial effects in another area.

Still, no cure is in sight; no vaccine protection is expected soon. Great hurdles continue to exist not only on the research front, but also in public education and statistical projection. The Public Health Service published figures projecting an expanded base of infection—1.5 million persons in 1987—which yielded dramatic numbers of possible AIDS cases five years into the future: a cumulative total of 270,000 cases and 179,000 deaths by the end of 1991.[6] If these projections remain firm, an estimated 50,000 persons will die of AIDS in that year, a figure reminiscent of, but somehow more frightening than, 40,000 deaths from tuberculosis fifty years earlier. One thing that gave pause to a long-time observer of such predictions was that similar statistical extrapolations within the last fifteen years—especially those regarding swine flu and Legionnaires' disease—did not hold up. As the grim but essential business

careful counting and delineating continues, in the context of expanded research, treatment and education efforts, one can only hope that the projections might be revised downward.

At the root of the statistical and educational problem was a behavioral one. Sexual practices in the homosexual community changed rapidly from the point of the elucidation of the disease pattern. But intravenous drug use among a growing population of addicts did not change. A high proportion of the new AIDS cases identified in 1987 and 1988 were among poor blacks and Hispanics, and a high percentage of those cases correlated with drug addiction and the use of contaminated needles. Hence, a new concern was that if AIDS was identified as threatening only sub-groups of the population outside the mainstream—gays, Blacks, Hispanics and intravenous drug users—the effort to combat the disease might slacken.

Meanwhile, small wars against other disease problems also continue to go forward. The 30,000 Americans afflicted with cystic fibrosis took heart from the recent identification of the chromosome that carries the deadly gene, background work for which was sponsored by the National Cancer Institute and the Cystic Fibrosis Foundation. The 35 million Americans who suffer from allergies were encouraged by the identification of the antibody responsible for their allergic reactions, research supported by the Allergy and Infectious Diseases Institute. Further progress was reported against diabetes, gout, and even baldness. While no significant advances were made against another terrifying disease, Alzheimer's, the increasing number of older Americans could at least take heart that, in work sponsored by the National Institute on Aging, it had been proved demonstrably that Alzheimer's was not simply a by-product of aging, but a specific disease with specific elements toward which new scientific research could be applied.[7]

Treatment of diabetes became more sensitive, and effective, when research supported by the National Institute of Diabetes and Digestive and Kidney Disorders distinguished between two types of the disease. For the first time in medical history, an effective drug treatment for gout was developed based on research on metabolic defects supported by the National Insti-

tute of Arthritis and Musculoskeletal and Skin Diseases. (Less progress being made against arthritis prompted the creation in 1986 of a separate National Arthritis Institute.)[8]

As for the political and budgetary context of the 1980s, NIH probably benefited from President Reagan's concern about U.S.-Soviet rivalry in arms and technology and the technology-based economic challenge of friendly countries. The President therefore looked favorably on "R and D". Of all domestic programs, one of few not seriously affected in the first Reagan term's efforts to cut domestic expenditures was NIH. In fact, the agency had acquired such a reputation that the conservative Heritage Foundation, which had contributed analyses and individuals to the Reagan election and re-election campaigns, identified NIH as virtually the only domestic agency of government whose work was so important, so efficiently carried out, and of such high benefit-to-cost ratio, that no cuts should be made in it.

Nonetheless, just as shock waves in the stock market continue to reverberate in the psyches of investors, even after specific crises pass, so the perception of another period of negative political influences and declining budgets accompanied the election and even the re-election of President Reagan. Dr. James Wyngaarden who succeeded Dr. Fredrickson as NIH Director in 1982, found that one of his greatest challenges was reassuring the biomedical science community, institution by institution and in some cases department by department and association by association, that research funds were not drying up, indeed were more than keeping up with inflation and almost with highly rated applications. Within five years after he assumed the NIH directorship, the agency's budget climbed from $3.7 billion to more than $7 billion.

In another repetition of history, Dr. Wyngaarden and his colleagues also found themselves defending the peer review system against political challenges from the outside, in this instance, the Chairman of the Senate Appropriations Committee, Senator Mark Hatfield of Oregon. Troubled because the universities in his state received comparatively little biomedical research funds, the Senator asked the General Ac-

counting Office to review the patterns of health and science research funding and the role of peer review systems of NIH and NSF in making such awards. The GAO reports may not have assuaged Sen. Hatfield's concerns, but it did show that peer review was not the reason that biomedical and other science research funds went to particular regions, states, institutions or scientists.[9] Instead, it documented the existence of strong, vibrant health science research capacities in every region of the country, particularly in one hundred institutions that spanned the length and breadth of the land, even if it clustered strongly in the Northeast and on the West coast. Nor was this group precisely the same hundred that had been the top recipients of comparable kinds of research funds twenty years earlier. Instead, the picture emerging from the statistics and the charts of the GAO showed two definite trends that must make the pioneers of the grants programs smile with special satisfaction: first is the steady buildup of particular scientific capacities, built around individuals and departments in every part of the country. Second is the development of new kinds of scientific strengths, in new areas or newly clustered groups of established scientific disciplines, combining to show the dynamism of scientific interchange and the emergence of new individual and institution leaders.[10]

C. J. Van Slyke, Ernest Allen, Ralph Meader and their colleagues may or may not have had in mind building geographically dispersed constituencies as one reason for their concerted efforts to assure that NIH took geography into some account. But their early attention to this factor, and their successors' further manifestations of concern about it, had, in effect, resulted in a strong practical shield against political influence on the expert review process. Indeed, the only successful assaults on peer review were those that went around it, rather than against it. For example, when the Democratic Majority Leader of the Senate, Sen. Robert Byrd found himself unable to persuade Dr. Wyngaarden and his NIH colleagues that they should somehow ignore study section and advisory council judgments about support of cancer research and treatment in his state of West Virginia, he caused to be inserted in an

authorization and appropiration bill a specific line item for the development of such a center in the budget of the Secretary of Health and Human Services, not that of NIH.

In this continuously dynamic climate, NIH Director James Wyngaarden urged the reversal of a research support trend of the past two decades, an increasing "central direction" of the focus and scope of scientific initiative. Particularly when the big bulge in the Cancer budget occured in the early '70s, and with the assignment of a large fraction of the new funds to the contract mechanism, NIH began to see the balance of its research support changed in favor of initiatives coming from inside rather than outside, from central direction rather than individual investigators. In fact, says Dr. Wyngaarden, "We fell under 45% of our total budget in investigator-initiated research project grants."[11] In the seven years of his tenure to date, that trend has been reversed, and now approximately 63% of the extramural budget of NIH goes in support of investigator-initiated research, about 10% higher than was the case in 1982.

Dr. Van Slyke would be pleased with another development as well. In the last several years, NIH has cut back on the number of pages allowed in research grant applications and at least in that way, simplified the application and the review processes. The interchanges between scientists in the field and administrators at NIH is not nearly as informal and personal as it was in the old days, but there are new ways for would-be researchers to secure helpful information. One is the requirement of the Privacy Act that researchers be allowed to see their "pink sheets," those summaries of the study sections' appraisals of their proposals prepared by the executive secretaries. To be able to understand exactly what members of the study sections thought were the strengths and weaknesses of a proposal is to enable a stronger application in a subsequent round. And even though the numbers of actual review panels increased to approximately ninety, the cost of processing and reviewing a research grant has come down in real dollars, dropping from around $1,800 in 1972 to approximately $1,100 in 1987.[12] Thus new systems and procedures, from computers

to sunshine laws, have helped the NIH reinvigorate an old spirit, that of open communication and efficiency.

Similarly, new discoveries coming out of both the intramural and extramural programs have helped in the reassertion of an old conviction. Dr. Michael Zasloff of the National Institute of Child Health and Human Development, has worked for some years on biological elements in reproduction, using the African clawed frog in his experiments. This basic research led him to the observation that the frogs never got infections from "surgical procedures," and subsequently to the identification of elements in the frog's skin which constituted the protection. The exciting possibility is that artificially replicated "magainin" can become a new medical weapon against a wide spectrum of infectious agents in humans. Commenting on his discovery, Dr. Zasloff gave powerful reaffirmation of the theme sounded forth years before by Dr. Dyer and Dr. Van Slyke: "You never know the ways of research. Let science be free. . . We are not so smart as to know if what we do today is going to be important tomorrow."[13] Or, he might have added, if so, how.

Dr. Zasloff's work had its counterpart in dozens of other "intramural" and "extramural" examples. Dr. Ruth Kirschstein points out an equally unusual and beneficial one: work supported by grants from the Institute of General Medical Sciences on recombinant DNA technology has led to the ability genetically to engineer bacteria to clear up oil spills, or on a smaller but wider scale, to dissolve hair stuck in plumbing systems.

Equilibrium may never be maintained for long. But those equilibria which the NIH must maintain seem to be in good shape on the fortieth anniversary of the Division of Research Grants and the hundredth year of the institution itself. The intramural program continues to produce first-rate results and share first-rate people with the scientific world.

The extramural program remains by far the largest component. Yet it has been relieved of a major burden: where project research and program research grants once dominated the budgets of many medical schools, they no longer do; reimburse-

ments for patient care are now the larger source of income, though a troubling one. With another government push to reduce overhead costs, and with appropriated dollars continuing on a definite though not dramatic upward climb, funds for research, within those hundreds of institutions where it is performed, are a stable element and relatively secure commitment.

The NIH today is a large agency, encompassing twelve institutes, a clinical center and five other divisions—including the Division of Research Grants. The National Library of Medicine, one of those divisions, houses the world's largest repository of medical literature, including information on recent scientific and medical advances, immediately accessible by computer to physicians throughout the nation and much of the world. The NIH staff of more than 14,000 persons includes more than 3,183 scientists and physicians, most of whom work at the 300-acre Bethesda campus. For the hundreds of scientists and scores of lay persons who serve on its review committees and councils, such service is considered a professional honor, as well as a professional or civic obligation.

Continuous organizational changes that have occured at NIH through the years include: the addition of new institutes and divisions; the reformulation of their names and the alteration of their specific responsibilities; responsibility for overseeing trial regional medical programs and other medical technology delivery systems; the lodging and dislodging of particular programs (e.g., National Institute of Mental Health, Bureau of Health Manpower Education) within its scope of activities. These have occurred in the context of changing political alliances and shifting sands of executive and congressional politics. Yet none of these factors or forces, together or separately, has, in the end, seriously affected the central mission or the guiding principles. The enterprise is alive and well and, to an amazing extent, functioning along lines envisioned and hoped for by the NIH pioneers. The grants program can still boast of its bill of rights for scientists—freedom of inquiry, freedom of initiation. The administrators are still keeping the balance wheels aligned.

Surely one reason for the continuity in the orderly pursuit of scientific progress and medical accomplishment, despite the enormity of change, is related to the continuity of personal connections. Jim Wyngaarden came to NIH in 1953 where he worked in the laboratory of chemical pharmacology at the National Heart Institute when Jim Watt was Director. Dr. Fredrickson was an NIH colleague. Subsequently, Dr. Wyngaarden was a clinical associate at the National Institute of Arthritis and Metabolic Diseases when Jim Shannon was Director of NIH and Van Slyke was Deputy Director. Dr. Jerome Green had a thirty year association with the National Heart Institute before becoming Director of the Division of Research Grants for NIH in 1986. Indeed many of the current senior NIH figures knew and worked with some of the pioneers.

The circle of leaders and potential leaders has grown greatly; the networks begun by Van Slyke, Allen and company have multiplied exponentially. But the institution and its scientific principles are touchstones for virtually every biomedical scientist in the country, and many beyond our shores. Despite the vastness of its reach and the formalization of its bureaucratic systems, it remains a human place, almost a familial enterprise, with strong personal links between the past and the present. Those who lead it and work there today share firmly with their predecessors a devotion to science, a concern for human health, and an optimistic spirit.

NOTES TO CHAPTER 11

1. Basic figures from *A Century of Science for Health: National Institutes of Health,* NIH Office of Communications, 1987. Updated figures through October 1987 provided by Division of Research Grants, NIH.
2. Susan Okic, "Smoking's Contributions to U.S. Deaths", *Washington Post,* October 30, 1987, p. A23.
3. "Research Accomplishments", *A Century of Science for Health,* op. cit.
4. Ibid.

5. The figures on AIDS used here are from several immediate sources, including interviews and newspaper articles, but all are based on figures published by the Center for Disease Control, U.S. Public Health Service, 1988.
6. Dan Colban, "AIDS: The Growing Impact." *Washington Post* Weekly Health Journal, June 2, 1987, pp. 10–11; see also *Washington Post,* June 3, 1988, page one article, continued on p. 14A.
7. "Research Accomplishments," *A Century of Science for Health,* op. cit.
8. Ibid.
9. "University Funding: Information on the Role of Peer Review at NSF and NIH" (Washington, D.C.: General Accounting Office, March 1987).
10. "University Funding: Patterns of Distribution of Federal Research Funds to Universities" (Washington, D.C.: General Accounting Office, February 1987).
11. Interview with Dr. James Wyngaarden, August 13, 1987. Transcript (National Library of Medicine, History of Medicine Division) p. 6.
12. Id., p. 1.
13. Quoted in Peter Caws, "Nature: Preserve it for Science," *Wasington Post,* August 10, 1987, op. ed. page.

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