

THE TWELFTH REYNOLDS HISTORICAL LECTURE

"Camelot-in-Bethesda: The Roots of the Magic Kingdom"

Presented on February 15, 1991

by

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Introduction by Wayne H. Finley, Ph.D., M.D., Chairman of the Reynolds Library Associates Steering Committee

I should like to welcome you to this the twelfth annual Reynolds Historical Lecture, particularly on behalf of the Reynolds Library Associates, the Lister Hill Library of the Health Sciences, and the Reynolds Library. I should especially like to welcome those guests from out of town who are here for the School of Medicine's Alumni weekend.

After the Lecture there will be some vans on the Eighteenth Street side of the auditorium to transport you to the reception at the Alumni Building, which is on Twentieth Street, across the street from the Radisson Hotel. The Caduceus Club of the School of Medicine sponsors this reception, and they certainly welcome you to this annual event.

In the lobby of this auditorium are displayed the books which have been donated to the Reynolds Library during the past year. We invite you to go by and inspect those additions to the Reynolds collection.

I should like to make one other announcement, and that is, that one of the projects of the Reynolds Library Associates will be in connection with the Festival of Arts here in May. As you know, this year Birmingham will honor Great Britain as the annual country of interest, and the Associates will bring the Wellcome history of medicine exhibit from London to the Alumni Building. It will be

open from May 2 to May 10, and consists of a series of photographs of outstanding events in the history of medicine. We invite you to attend this event during the Festival of Arts.

I am pleased this evening to introduce Dr. John Durant, Vice-President for Health Affairs and Director of the medical center, who will be our master of ceremonies.

Opening Remarks by John R. Durant, M.D., Vice-President for Health Affairs and Director of the Medical Center, The University of Alabama at Birmingham

Thank you, Dr. Finley, very much.

In all great institutions there are very special events which create the tradition of that institution, and the Reynolds Lecture is one of those important events in the life of this institution. We are very pleased to have Dr. Fredrickson here to give the twelfth of these Lectures, which are supported by the Reynolds Library and by all of the Schools of the Medical Center, and I wish to take this time to thank all of those people who have contributed to making this a very special event.

In your program there is a listing of the donors of books to this very important collection during the past year, and I should like to take this opportunity to thank the donors of these books which gradually add to this very special collection, which is a very important part of this institution.

We have some special guests with us this evening. Dr. Fredrickson brought along his wife, Mme. Fredrickson, and their son Rurik, and we welcome them to UAB. It gives me special pleasure to welcome Dr. McCallum here as our President. There is also a listing in your program of all of the Reynolds Library Associates, and we want to be sure we thank them for their interest in this event. We should also like to thank our librarian, Virginia Algermissen, and Mr. Marion McGuinn, the librarian for the Reynolds, and his staff. We should also like to thank the UAB Ambassadors for their help in this particular event.

At this time I should like to introduce one of my predecessors, Dr. Richardson Hill, who will introduce Dr. Fredrickson. Dr. Hill is well known to all of us. I will turn the meeting over to him.

Introduction of the Speaker by S. Richardson Hill, Jr., M.D., President Emeritus, The University of Alabama at Birmingham

Thank you very much, John.

This is really a special occasion for me, and we are all, I believe, very fortunate indeed to have Don Fredrickson as the 1991 annual Reynolds Historical Lecturer. By selecting Don we continue the tradition of having some of the most distinguished scholars in the world highlight this annual event at UAB. As an example, the last three lecturers were Dr. Michael DeBakey, Dr. Victor McKusick, and Dr. Roger Guillemin, and you will find other outstanding Lecturers listed in your program.

It is a real personal pleasure for me to introduce Don once again to a Birmingham audience, and to welcome not only Don but his wife, Priscilla, and their son Rurik as well to UAB and to Birmingham. It hardly seems possible, but Don and I have been friends for over forty years. I had been at the Peter Bent Brigham Hospital for two years as an intern and as an assistant resident when Don began his internship at that Hospital in 1949. In an unusual coincidence the summer before his internship started, my future wife Janet (whom I had not met at that time, and who was a student at Vassar College) and Don (who had just graduated from the University of Michigan Medical School) met each other while crossing the Atlantic on a converted troop ship with a group of other students. Janet was to tour Europe and to go to summer school at the University of Oslo in Norway, while Don was to bicycle through Europe, especially in the Netherlands. It was there that he met and fell in love with the beautiful young law student Priscilla, whom he married in 1950, the same year Janet and I got married, and we have all lived happily ever after. So much for the romantic interlude.

I should tell you that Don is from Colorado, and although he attended the University of Colorado, he obtained both his baccalaureate and his medical degree from the University of Michigan. In addition to training at the Peter Bent Brigham Hospital, he also trained at that other Harvard hospital, the Massachusetts General, before joining the staff of the Laboratory of Cellular Physiology and Metabolism at the National Heart and Lung Institute. In a ceremony honoring him, his colleagues at NIH had engraved, not in stone but in silver, "At NIH he wore every hat, but history will

know him as the king of fat."

Don subsequently became Director of the Institute, and Director of the Division of Intramural Research from 1968 to 1974. He next served for one year as the second President of the National Academy of Sciences' Institute of Medicine. He then took on the responsibility of being the eleventh Director of the National Institutes of Health. He served admirably in this position for six years under three United States presidents from 1975 to 1981. He next became a Scholar at the National Academy of Sciences for two years before becoming Vice-President, and then President, CEO, and Trustee of the Howard Hughes Medical Institute from 1983 to 1987. During his tenure the Institute became the world's largest philanthropy. Don is now a Scholar at the National Library of Medicine and the National Heart, Lung, and Blood Institute, a visiting lecturer at both George Washington and Georgetown universities, and is also President of a consulting firm, D.S.F. Associates.

Don is a member of many of the most prestigious and scholarly organizations in the country, including Phi Beta Kappa, A.O.A., the National Academy of Sciences, and the American Academy of Arts and Sciences, among many others. In addition, he has received numerous honorary degrees from distinguished universities, as well as numerous awards and citations from scores of prominent organizations throughout the world. He has also authored and edited some two hundred and fifty articles, books, and scientific papers.

Finally, Don is a highly intellectual and sophisticated scholar, not only in medicine and science but in the arts and literature as well. He has a better command of the English language than almost anyone I know, except perhaps for Margaret Thatcher. He is going to speak to us tonight on "Camelot in Bethesda: The Roots of the Magic Kingdom."

Please join me in welcoming Dr. Donald S. Fredrickson.

Dr. Fredrickson's Presentation

Thank you, Richard.

That is a welcome that makes me feel warm. I have been in Birmingham before and remember that I lectured then on "The Search for the Omphalos." We talked about how the earth was

formed and the movements of the tectonic plates. How in one era there existed the continent of Pangaea, which was Africa, North and South America all together, and we talked about how in Berkeley, and at Cal Tech in Pasadena, the kids used to wear T-shirts that said "Reunite Pangaea"—but there was no way to put it back together again.

I am delighted to be here this evening. I am going to give a lecture that I have never tried to synthesize before—including a short film lost to the public for forty years. Let me first tell you how this topic arose. I came back to NIH in 1987, and thought I would go back to my old laboratory, where they invited me to practice molecular biology in the wet. To be safe, however, I also staked out a bivouac in a study carrel at the National Library of Medicine. An invitation soon arrived there to address the Board of Regents at a dinner to be held a month or so later.

I thought I should pick out a topic that is historical, something easy, like the origins of the NIH Clinical Center. It was NIH centennial time and I looked at all the historical information that had been compiled. Curiously, the sources were mute on the subject of the roots of this great building, a paradigm of clinical research when it opened in 1953. Nobody could recall who had thought of it and how it was finally brought about. That started me digging in earnest. Ever since, my laboratory corner holds no cloning dish and operations have been mainly confined to the NLM carrel, now full of old, dusty archives, manuscripts and documents, and photographs of Public Health officers from the past. I finally have put together a story that is at least a partial answer to the question, and I should like this evening for the first time to sketch it for you.

It is not a deliberate injury to this audience that I shall not be dealing with an era involving Senator Lister Hill. His contributions to NIH cannot be praised too highly. If we examine the plot of NIH appropriations from 1950 to today, we are in awe of the pitch of the climb from 1955 to the late 1960s, the period when Lister Hill was chairman of both the authorizing and appropriation committees for NIH. I will be telling a story however that begins way to the left of this plot and starts in about 1930.

Many of you know that NIH is more than a hundred years old. Its lineage comes from the single-room Hygienic Laboratory at the

Staten Island Hospital created by the Marine Hospital Service to engage in serious science, particularly bacteriology. The laboratory was moved to Washington, D.C., a few years later, where it continued to be one of the earliest and best American medical research laboratories in its narrow field of specialization.

It was located in new quarters in a building at 25th and E streets in 1925 when Senator Joseph Ransdell of Louisiana was moved to convert it to the head of a grand search for the roots of disease. He introduced a bill in 1926 with the ambition of establishing a world center that would be swarming with life scientists of all disciplines who would work on the fundamental bases of all of the diseases of man. It would have a gigantic library and the largest fellowship program in the world.

When the bill was finally passed in 1930, it was only a shadow of its original self. The name of the Hygienic Laboratory was changed to the National Institute of Health, but its authority was expanded by only enough money for another building. It was given a fellowship program without a cent of support. As a sign of the times, the Congress did generously provide authority to accept any gifts given toward support of the new NIH. Ransdell went home in 1930, ran for the Senate, was defeated by Huey Long, and spent the next several years actually seeking gifts from industry and other private sources. His receipts were few, ranging from \$100,000 from the chemists' association that had first given him the idea, down to the sum of \$1 from Mr. Charles P. Wilder of Worcester, Massachusetts.

The reason for the disparity between the dream and the realization was that the bill had been passed in the beginning of the worst economic depression America had ever seen. Curiously enough, research and development in industry—by far the largest segment of such activity in that time—was doing reasonably well. The overall funds for research in universities and non-profit institutions, however, were actually lower in 1940 than in 1930, as their endowment and major foundation support weakened. The new NIH went hand-to-mouth in the same fashion during this decade. Even when, with a burst of rhetoric, the National Cancer Institute was established in 1937, the new institute's budget of \$400,000 remained about the same for the next ten years!

It was World War II that introduced a dramatic change in the

fortunes of science. Many of you know the name of Vannevar Bush, the distinguished engineer who was director of the Carnegie Institution of Washington in 1940. He and his friends went to see President Franklin D. Roosevelt and convinced him that the nation's scientific apparatus must be harnessed to meet the multiple needs of an actual war. At Bush's suggestion, the President established an Office of Scientific Research and Development (OSRD) with the authorities to organize and finance war-time R&D. The OSRD included a Committee on Medical Research (CMR). Soon scientists in the private sector were asked to sign on and received yearly contracts for war-related research. Many scientists, especially those in academic and non-profit laboratories, harbored a great fear that accepting government support imperiled academic freedom, but the demands of patriotism—and the desire to continue research—prevailed.

Medical research had a high priority at the time. The Germans had controlled the world market on quinine, and new antimalarials had to be found. Sulfa drugs were the only effective antibacterials; eventually the scale-up of penicillin production was also tackled. There were an endless list of other problems, from immersion foot to high altitude sickness in flyers, which needed solutions.

The CMR was headed by a highly regarded member of the faculty of the University of Pennsylvania, Professor A.N. Richards, the man who had first put a micropipette in a kidney tubule. Four other medical scientists joined the three Surgeons General in making up the CMR. The latter usually sent their chief scientists as members. Thus Rolla Eugene Dyer, the NIH Director, became the Public Health Service representative in 1942.

The acceleration of medical research in that time was remarkable and an indication of what serious federal support of science could achieve. Some projects accomplished more than others. Between undergraduate work as a U. S. Army private and medical school at the University of Michigan I was assigned to an OSRD-CMR project being carried out in a secret laboratory in the chemistry building. Passes were required to get through the barricades. Our assignment was to make protective clothing for fliers then perishing at an alarming rate in the North Atlantic. The task was to keep the cold water out, but allow perspiration to pass through during the long hours the suits had to be worn. Daily we bonded

nylon sheets with secret ingredients. One component was latex, a product in great demand which I once had to bring back from Dayton, Ohio to Ann Arbor, carrying the precious container with me throughout the railroad journey. The latex was mixed mainly with a mysterious "LD 50" (later discovered to be methyl cellulose). When we had fabricated enough cloth we would make up a suit and watch the research project leader promptly sink to the bottom of the swimming pool of the Men's Union. Years later I learned that "Aerobond" had ended up as sheeting to protect the mattress from enuretic patients. This poor example does not do justice to the success of many other projects, including work on atomic fission proceeding under the football stadium in Chicago, which was said also to be under OSRD supervision.

When victory seemed certain to Bush in 1944, he made plans to wind down the OSRD. He had never considered that it would extend beyond the war. In November of that year, however, Bush received a letter from President Roosevelt in which several questions were posed. The first was, what can the government do to aid scientific research by private and public institutions after the war? Bush put together a blue-ribbon committee of scientists to answer this question. It was chaired by Isaiah Bowen, a geologist and member of the scientific elite, who was also Vice-President of the National Academy of Sciences. Bowen surrounded himself with a committee of his own kind. There were no biological scientists, because medical research, the subject of the second question, would be examined by another committee. Bowen's group began by deciding that social science research was not their proper domain and set it aside. The kernel of their conclusions was that centralized control of research by a small number of persons, buttressed by the power and prestige of the federal government, would likely end disastrously. Support for science from the private sector was so bad, however, that they decided it should go ahead, provided two conditions were met. One was that a very small committee—with no powerful director who could be corrupted by politics—would manage the distribution of the funds. The second condition was that most of the money would go to universities in a lump sum, provided the school had a committee overseeing local disbursements.

Many people thought the risk of federal support to the ethic of

basic research was very great. Frank Jewett, the head of Bell Laboratories and then President of the National Academy of Sciences, certainly felt so, and additionally wrote Bush his opinion that he would leave medical research out altogether, because, in his view, as soon as one medical problem was conquered, another would crop up. The Committee on Medical Research was bypassed by Bush in selecting a group to answer the President's question about the advisability of continuing medical research. The head of this newly established Medical Advisory Committee was Walter Palmer, the Bard professor of medicine at Columbia's College of Physicians and Surgeons. The remainder of the cast included such powerful opinion-makers as Homer Smith, William Castle, Alton Ochsner, and last but not least, Linus Pauling. This group concluded its deliberations with an opinion that federal support of medical research was indeed essential, but that it would be undesirable to have it administered by the same foundation proposed by Bush for the other sciences. The medical committee favored an arrangement like Britain's MRC. One thing all agreed upon was that none of the existing government agencies should be involved. Apparently neither the military services nor Public Health Service, very few of whom had sophisticated scientific training or Harvard or Columbia degrees, could be capable of handling what Bush was proposing.

Bush ignored the separatist views of his Medical Advisory Committee, and a bill for a National Science Foundation to cover both the natural and biomedical sciences was introduced by Senator Warren Magnuson in 1945. It had the design favored by Bowen's committee, with a weak directorate and a Board responsible for strategy and implementation. Serious opposition soon loomed, however, in Senator Harley Kilgore's opposite proposal to harness the great American science machine to government control along Kilgore's more populist or socialist views. Kilgore thought there would be a *really* powerful directorate, four executives for parts of the country, who would manage all the resources. The Foundation would include on-site inspection of the research and transfer of the money to another institution if they could do it better. All the patents of any invention supported would go to the government. There would be geographical balance in funding. Finally—the last straw for the Bowenites—social sciences would be sup-

ported, too.

The hearings on these bills resulted in a Grand Guignol of protest and dispute. There was a flood of letters, including a petition to President Truman, who was said to favor some of Kilgore's proposals. Bush was completely opposed to Kilgore's formulation, and so was Surgeon General Thomas Parran. In the archives where many of the petitions are available, it is interesting to note that the predominant opponents of Kilgore were members of the faculties of medicine. As is true today, many of the biological scientists did not join the medical cause. Witness this statement of Willem Demerec, head of Cold Spring Harbor, to the Magnuson Committee: "It has been found in practice that placing fundamental research of agencies that anticipate practical applications seriously limits and restrains the freedom of thought necessary for basic advances." A bit stark? Not really; molecular biologists are currently saying the same thing today, abandoning the integral nature of research with conquest of disease as its purpose and forgetting the radical improvement in support for biological science deriving from the outcome of this period of turmoil.

Before these quarrels over the shape of future funding for research had begun, Surgeon General Parran of the Public Health Service made a move that was little noticed by the scientific community. He joined forces with a North Carolina Congressman, with the unlikely name of Bulwinkle, in a rewriting of the chaotic public health statutes which had been accumulating over the years. Parran urged the inclusion of a brief section containing a powerful new authority. The language (Section 301 of P.L. 78-410) constituted the broadest authority ever given to a federal agency for the conduct and support of research. All that was needed for an otherwise unlimited program was the appropriation of funds to use these broad and unrestricted powers. It was the first of several moves that would lead eventually to the realization of Ransdell's original dream. (I have a special feeling for Section 301, for near the end of my term as director a young Congressman, Henry Waxman, decided he would remove this vital portion of the midbrain of the NIH. The struggle lasted a year, but 301 remained intact, at least for then.) This key recodification became law in March, 1944. The letter from the President to Bush and the quarrel it precipitated was half a year away. At the end of 1944, Dyer wrote the chairman of the

MRC, reminding him that the NIH now had authority to take over some of the research that would still be ongoing at the time OSRD might close its doors in the next year. But there came no answer from Chairman Richards. As we have seen so far, the public dialogue in 1944–1945 made it clear that the Public Health Service was the favorite agency of few if any of the protagonists for new federal funding of science. Indeed, there were scientists in the NIH who were opposed to its expansion. The academic communities were ambivalent and it was well known that the Bureau of the Budget, the National Academy of Sciences, the American Medical Association, and Vannevar Bush himself, were adamantly opposed to NIH's becoming the agency which would take over those expiring research contracts.

Time was on the side of the angels, however. In 1945 the CMR met for the last time. OSRD was definitely closing down. Chairman Richards offered, one by one, the expiring contracts to the members of the committees. Only "Gene" Dyer raised his hand. At the end of the meeting he went home with the whole bundle.

The NIH extramural program thus began in 1946, a cold historical statement—of enormous implications—for the American university system as we know it now, and for medical research throughout the world; but not so simply achieved, for the money had to be obtained from Congress to begin. Dyer, and the Surgeon General, sitting side by side in the Appropriations Subcommittee chamber, had yet to pass the biggest hurdle. For any government agency nothing is real until it is bartered to the Congressional overseers. A reading of the dry record of the hearings in which Dyer procured the money to begin a grants program—the second of the three great coups of my story—deceptively suggests it was simple.

Testifying before Congress, as I have had opportunity to do so many times, convinces you that it is a unique art form. Much of the effort is spent in "creating the record." It seems a game, but if it is, there is no question but that the "game" is for "keeps." Here I show you the colloquy between Dyer and Mr. Ingles, a member of the House Labor and Health Appropriations Subcommittee.

Mr. Ingle: "What is this supplemental request for?"

Dr. Dyer: "Well, \$817,000 is for research grants."

Mr. Ingle: "You mean grants-in-aid for the states?"

Dr. Dyer: "No, not grants-in-aid, but research grants. We took over some research grants from OSRD."

Mr. Ingle: "What's the OSRD?"

[Was this a rhetorical question? Dyer explains that this was the office which made the atomic bomb and conducted wartime medical research.]

Mr. Ingle: "What was the date you took over this work?"

Dr. Dyer: "On the first of January, 1946."

End of conversation. Stale staple of Congressional business. But to me it reads like a passage from Homer. By switching contracts to grants and avoiding any prediction of what might come of all this, Dyer has brought his little vessel through the straits, without arousing a single one of the Furies. He has presided quietly over the metamorphosis of a small government laboratory into a future research colossus.

This might have been enough for one day. Not quite, however, for Dyer has one more colloquy with yet another member of the committee. Dyer expresses regret that last year's failure of an appropriations bill (Congress often cannot pass such bills on time and the government survives on a "continuing resolution") had prevented NIH from bringing up its need for building a facility for clinical research on the campus in Bethesda. The Bureau of the Budget had given NIH permission to bring this to the committee's attention at this hearing on a supplement to the annual appropriation. This prepared the way for the Surgeon General, at the next annual appropriations hearing, to confess that the building would probably cost about \$40,000,000, an amount twice that spent for all medical research by the CMR during the war. The massive hospital, five hundred beds wrapped in a thousand laboratories, far larger than any similar research facility in the world, was Parran's idea of the necessary complement to the extramural program. This would constitute the physical center of Ransdell's world research facility.

The idea of placing such a mammoth research hospital in the woods outside Washington did not appeal to many members of the academic medical community. Some of them told it to their congressmen. At the Senate Appropriations Subcommittee hearings on the Clinical Center money, Senator Pat McCarran roared at Dr. Norman Topping of NIH: "Do you know that there is a feeling present that concentration of this vast amount of federal funds in this center is discouraging private research institutions? . . . You may not just sit smugly there and say that we have it all and think you are going to get it so easily." The Chairman of the Subcommittee, Senator William Knowland, a solid and somewhat feared Republican, visited the NIH, and came away determined to support the project. The House was far more receptive. Congressman Frank Keefe of Wisconsin had liked the idea from the first, received Dyer and a small delegation in his home in Oshkosh, and offered a bottle of whiskey so that ambitions and deliberations could mix properly. Keefe was determined to hold to Parran's estimate of the cost and, with assurances, saw the appropriation through.

Witness to the academic resistance to the Clinical Center excludes from memoirs of some of the PHS leaders of the time. I remember vividly an encounter in the hall of the Massachusetts General Hospital which I had with Walter Bauer, the Chief of Medicine. Bauer, who could be a moody man, said, "Well Fredrickson, I hear you are going to this thing in Bethesda. It's going to be a great big federal backwater, and nothing more." Ten years later, Dr. Bauer was down at the "backwater" to recruit the coming generation of Harvard faculty. I was in the first group of eight Clinical Associates in the Heart Institute who arrived when the Clinical Center was opened in 1953, with a dedication by Oveta Culp Hobby, first Secretary of the new Department of Health and Welfare.

If one plots the course of events from Ransdell's creation of the NIH in 1930 to the year 1950, it is easy to see the critical years. They surrounded the times of operation of the OSRD and CMR. In 1944, Parran had obtained the crucial authorizations, permitting the extramural program and the creation of a Clinical Center, purely for research. In 1945 the expiring CMR contracts came home with Dyer, the money to convert them into grants came in 1946, and in 1947 came the money to start the Clinical Center. The pluralization

of NIH began in 1948, with the creation of four institutes, and by 1950 six in all existed. Bush's foundation, finally named the National Science Foundation, was created in 1950.

It is worth reflecting on the backgrounds and character of the three principal figures who were more responsible than any other persons for the transformation of Ransdell's ambition into the modern reality. They are virtually unknown by the present generations of researchers at NIH or, indeed, in the universe of academic science most affected by their endeavor. There is no picture of Thomas Parran displayed in any campus building at NIH. Portraits of Dyer and of Lewis Thompson hang in Building One, for they were Directors of NIH, but time blurs their identities. All three of these men were of modest families, and went into the Public Health Service within a year after their leaving medical school where they went into field work. Dyer, who received most of his training in the service, became an accomplished laboratory scientist. It was not the sophistication of their training in R&D, however, that accounted for the services for which we are in their debt. It was certain other qualities that determined their achievements as a group.

The oldest of the three was L.R. Thompson, whom everyone called "Jimmie." After going into the service, he became a quarantine officer and also had a year or two of study of pollution on the Ohio River. When he came to Washington in the 1920s he was soon made head of Industrial Hygiene in the Public Health Service, and in 1930 he became Chief Scientific Officer, during the time of Ransdell's effort to transform the Hygienic Laboratory. In the portrait of Thompson at NIH, he is standing before a laboratory desk, but there is only one instrument visible, a telephone. Thompson had qualities outside science that made him extraordinarily effective. He was a remarkable builder of places where science might be done, and he was a genius at tapping the reservoirs of the Executive Branch, the Congress, and the rest of official Washington in the interests of his beloved Public Health Service. Thompson had been working the halls of the committee rooms where Ransdell's bill was finally put to bed (1930). In the days afterward, when Ransdell was seeking contributions for his impoverished progeny, Thompson too went from door to door and—if one counts an authorization of two million dollars in the Social Security Act—he was more successful than Ransdell. In 1937, Surgeon General

Parran handed Thompson a suggestion from two other physicians for the creation of a National Cancer Institute, and told him to draft such an act. He went one better; he also drafted the report of the House on its passage of the Bill. It was Thompson who was available to help Congressman Bulwinkle revise the Public Health Service Act in 1944.

Thompson was responsible for the movement of NIH from its two small buildings in Washington to the campus in Bethesda. In 1935 he somehow learned that the Luke Wilsons out on Rockville Pike were interested in giving 45 acres of their valuable estate, "Tree Tops," for some good government purpose. Some say that Thompson even drafted the reply from the President accepting the land for the Public Health Service. At first Thompson sold the family the idea of creating an animal farm for the Institutes there. The Chamber of Commerce and the Planning Authority rejected the proposal to build a "pig farm" in Bethesda, but several influential people, including Gilbert Grosvenor, publisher of the *National Geographic*, and neighbor Canon Peter of the Washington Cathedral, overturned the objections. The family eventually made a gift of 95 acres, and Thompson moved the whole NIH to Bethesda in 1938. Some of the money in the Social Security Bill went for the first buildings.

Those familiar with the Georgian-style mansard roofs of the first three-story buildings will recognize the influence of the architecture of the earliest structures put up at the Pasteur Institute. Jimmie Thompson had been sent abroad to survey laboratory construction, before the NIH construction began.

Let us turn to Thomas Parran, who I think will prove to be one of the greatest of the Surgeons General in the history of the Public Health Service. He was born in rural Maryland and was tutored by an aunt until he was ready to enter St. Johns University in Annapolis. He then proceeded to Georgetown University Medical School, and after a year's internship at Sibley Hospital in Washington, entered the Public Health Service. After several years of field work he came to Washington as Head of the Venereal Disease Division. He caught the attention of Franklin Roosevelt when the latter was governor of New York, and he arranged for Parran to be seconded to Albany to become the State Health Officer. Parran soon declared all-out war on venereal disease. At that time "syphilis" was not a

word uttered in public, but on an NBC network Parran said the word and was promptly replaced by a piano. During these same years, Parran became greatly concerned about the lack of attention to chronic disease, including a vigorous research program within academic medicine.

When Parran returned to Washington as Surgeon General at the start of F.D.R.'s second term, one of his first acts was to make Thompson Director of NIH, for the two of them had a shared vision about the importance of a scientific war on cancer, cardiovascular disease, and mental disorders. Parran first indicated his interest in including a place for clinical investigation within the NIH when he presided over the National Advisory Health Council and the National Advisory Cancer Council. Indeed, the NCI council became so enthusiastic they campaigned for a hospital of their own. He found too that the psychiatrists did not want to mingle with somatic diseases for their research. Parran and Dyer had to wage a continuing, patient struggle against Balkanization of the research effort. They shared a strong belief that an all-out attack on diseases meant a unification of basic and clinical research, "under one roof."

Parran had placed construction of a Clinical Center at the NIH at the top of his priorities in his ten-year plan issued in 1944. Victory for integration of clinical research was won at the last moment, for the authorization for the Clinical Center arrived at the President's desk in 1947, along with authorization for a separate psychiatric hospital obtained by the Mental Health Division from another act of Congress. The Director of the Bureau of the Budget, James Webb, urged the President to agree to only one facility. On November 15, Harry Truman made it final. There would be one hospital of five hundred beds.

Now I should like to show you these personalities in action in a short film that was uncovered as a result of my search of the archives. This exhibit, a copy of which was edited with the insertion of still photographs from the times, permits one to see the NIH as it was in 1946, including the temporary buildings erected for wartime research and which were now being hastily renovated as the first home of the extramural grants program just getting under way that year. Here is a plot of the existing ground and the surrounding two-hundred-and-some acres. Acquired for expansion of the NIH through authority of the appropriations acts for the

Clinical Center were the home and grounds belonging to Canon Peter (the house that became known as Stone House). The adjacent Woodmont Golf Course, which had opened there in 1920, were also acquired. Finally, all but eleven acres of the dairy farm belonging to the Sisters of the Visitation were acquired. The sisters had established their cloister for sequestered nuns in 1923. (The NIH acquired the land when the nuns sold out in 1983, and it is today the home of the "Cloister" program operated by Howard Hughes Medical Institute as a residence and center for medical students undergoing research training at NIH.)

Now the movie switches to 1949. "Masur's Mountain" is rising behind Building One and the great hole where the foundation of the Clinical Center is being laid. In Building One, Institute Directors are seen greeting Thomas Parran and Eugene Dyer, still Director of NIH. Others about them are the planning committee for the Clinical Center appointed by Parran: Norman Topping, Associate Director of NIH; Leonard Scheele; and Mark Hollis (later the first director of CDC). Actually this scene is only a reenactment. Dr. Parran never sat down with the model that we now see in his hands. In April, 1948 he had been abruptly told he would not be reappointed by President Truman. Dr. Scheele was his successor. One of the figures standing about the model is Jack Masur, the physician and hospital executive from New York, who had been chosen by Parran to see the Clinical Center through to completion. He must be credited with the "blood-and-guts" effort to bring this unique hospital to completion for its opening in 1953.

The film gives us some painful pictures of the ancient trees on the old Wilson estate and the convent grounds being sacrificed for progress. Amid them is also glimpsed "Top Cottage," the house of the caretaker of the Wilson farm, which became the meeting place and social center of the NIH until the mid-sixties.

The proper beginning of this film is at the end. We see—in August, 1948—the excavators, McCloskey and Company from Philadelphia, arrive with the earth movers. I want you to focus on this figure over here, a lone man wearing one of those old-style white lab gowns tied in the back. As he hunches forward, he seems defiant, for he knows that when this earth has been moved, the NIH will never be the same again. Among the scientists who were sure that, with the coming of a hospital and the categorical institutes,

NIH would no longer be a haven of basic science, was Arthur Kornberg, who left shortly afterward for Washington University. In a recent book, he describes this feeling about the new NIH as one of the two greatest mistakes he ever made.

The final shot shows a group of officers in 1944. At Parran's elbow, like Cardinal Richelieu attending Louis XIII, is Jimmie Thompson. Dyer sits across from them. I suppose that the prime qualities of these men were their understanding of the integral nature of biomedical research, the sense that it was time for some new paradigm to emphasize this inherent truth, and an insistence upon excellence as the standard. From the turmoil that at this moment was rising about the desirable successor to the OSRD, they also had ample awareness of the fear of scientists that the power to distribute resources for scientific inquiry was corruptible and therefore a special public trust.

As a brief denouement, I will offer an answer to obvious questions about the fate of Thomas Parran. Why had he been so abruptly removed that April night in 1948 when his committee was preparing to report their vision for the future NIH? He had been then only fifty-seven, was at the height of his productivity and on the eve of seeing his boldest conception rise. He was, to be sure, at the end of his third four-year term, but his predecessor had served four such terms. If we search the columns of the *New York Times* in the spring of 1948, we see that the paper's editorial staff too were concerned. Why had this consummate public servant been dealt with so summarily?

Searching further for proof of suggestions from other observers, we at last came across a 1950 copy of *Colliers* magazine. The principal subject of the main article was Jack Ewing, the administrator of the Federal Security Agency, which had been the home of the Public Health Service before the creation of the Department of HEW.

The story offers, with a bitter tone, a survey of Mr. Ewing's obvious talents as a lawyer, his eminence as a New Deal politician, friend of Clark Clifford, and position at the poker table of President Harry Truman. Embedded in the body is this vignette. The article states that Mr. Ewing had found that the NIH had refused to recommend a research grant sought by Mrs. Ewing's personal physician. Ewing had allegedly sent for Dr. Thomas Parran, Sur-

geon General of the United States and a world figure in public medicine, and said, "I demand that the recommendation be made." Parran was said to be outraged and to have told his superior that this was the first time in the long existence of the Service that a nonprofessional had brought pressure, and he refused. I have explored the roots of this story more deeply and am certain that such a grant was eventually awarded after Parran's departure.

I end on this note of irony—perhaps I should say, of martyrdom; but I know that such a conclusion can be an unprofessional one for a historian. I wish to thank you for your great patience and for giving me the pleasure of telling this story.

Response Spoken by James A. Pittman, Jr., M.D., Dean, the University of Alabama School of Medicine

I first wish to thank Dr. Fredrickson very much for a great Reynolds Lecture. It was very revealing, and I hope it has been, or will be, published; if not, we will offer to publish it for you. I think there was probably more in that last little anecdote than was discussed. The NIH is subject to terrific political pressures, and if there is any one message in the whole talk which Dr. Fredrickson has given, from Vannevar Bush right to the last, it is that freedom of inquiry must be free for inquiry.

Dr. Fredrickson was there when NIH was built. He was in the first group, as he said, of eight people who went there as clinical associates of the National Heart Institute in 1953. I see Dr. Branscomb here; Dr. Branscomb was not very far behind. I was there the next year, and took care of the first patient ever admitted to NIH. Although he had been admitted a year before I arrived, he was still around when I got there. Great unit number, 00001!

In one way or another Dr. Fredrickson has been associated with the NIH ever since the very early days, when the Clinical Center he described opened in 1953. He was Director during difficult times in 1975 to 1978. It may have seemed less difficult based on the fact that it was more difficult to sustain when the budget was not growing so much and there was an oil shock and all that sort of thing. He did not say so, but his portrait is right up there beside Dyer's and Thompson's and those of the others.

Dr. Durant and I were discussing this afternoon the response to

this talk, and I said, "Well, you know he discovered and described Tangier disease, a lipid disease which is not encountered every afternoon, but an interesting disease, which revealed a lot about the nature of lipid diseases; and he developed a classification for hyperlipidemias." John said, "No, his greatest contribution was that he has always been in the forefront of promoting, for the last forty years, fundamental research in biomedical areas." He has been associated with three great American institutions which have been in the forefront of biomedical research: the National Institutes of Health, the Institute of Medicine of the National Academy of Sciences, and the Howard Hughes Medical Institute, but especially the NIH. You notice, he went back to the NIH recently.

I think one must remember, no matter what one thinks about the parent, Emerson's quote: "The reward of a thing well done, is to have done it."* As Dr. Fredrickson said, nobody at NIH knows who those people were any more.

One observation: there is no Soviet NIH. They did not have to have it. I think Ransdell is another person who is forgotten all the time. There is a National Institute of Medical Research in Mill Hill, England. I do not know the history of that; Don probably would though, he has been around a long time. There is INSERM in France, and the Istituto Superiore di Sanità in Rome, but none of them compare even remotely with the NIH. The Japanese have been increasing their contributions to cancer research much more rapidly, on a percentage basis, than we have in recent years, but nobody is doing what the NIH is doing.

One question I wanted to ask is this: how many Soviets have ever won a Nobel Prize in medicine? There have been probably seventy or eighty prizes in medicine awarded by now. They have never won a single one. Pavlov won a Nobel Prize in medicine, and so Metchnikoff did. Metchnikoff is often placed in the French list because he did a lot of work in France. These Nobel laureates were pre-1917, however. The Soviets have *never* won a Nobel prize in

*Ralph Waldo Emerson, "New England Reformers," in *Essays: Second Series* (Boston: James Munroe and Company, 1845 [c1844]), p. 311. Emerson is here paraphrasing Seneca, who wrote in his *Epistulae ad Lucilium* (Epis. 81, sec. 20): "Recte facti fecisse merces est."—*Ed.*

medicine, although they have won in every other category, especially chemistry and in categories like that, because those are politically useful. The fact is, they do not give medicine any priority. One can win a prize for literature working in an attic all by oneself; or one can win a peace prize, as Sakharov did for his protests, all by oneself, but one cannot do modern biomedical research without government support. The NIH is really one of the prongs in American civilization, and biomedical research and medicine are taught in English around the world, using American textbooks, because of the NIH. If that goes down in the next decade, I think we will have lost a major contribution to civilization.

We thank Dr. Fredrickson for the story he has told; we congratulate him and thank him for being here this evening.

I should like to announce the formation of the Alabama Chapter of the NIH Alumni Association, which will have its first meeting probably on April 5. The date is not yet totally settled: Ed Rawl, the Deputy Director of NIH, said he would come down here on April 5, when we will kick the organization off. The meeting will be in the Alabama Medical Alumni Building. It seems only appropriate to announce this today, for the Alabama Rural Health Association was also founded today, and we have to maintain balance here!

Dr. Durant: Thank you very much, Dr. Pittman and Dr. Fredrickson.

Before we go, we should like to give Dr. Fredrickson a special memento of his visit here in appreciation of the things he has taught us and told us about the organization of the NIH. We thank you very much, and hope you can hang that in a prominent place in your office.

Dr. Fredrickson: Thank you very much.

Dr. Durant: You are welcome.

Every year there is a Lawrence Reynolds Historical Award which is given for an essay. This year Ms. Pat Greenup has been selected as the winner for her essay entitled "Continuous Quality Improvement: A Decade of a New Paradigm of Management Science for Healthcare Organizations." Ms. Greenup, we have here a plaque for you which you can hang proudly on

your wall for your accomplishment.

Before we go, I should like to say one more thing. About ten years ago, at a dinner here in Birmingham Mr. Walter Cronkite said almost the same thing Dr. Pittman said about the NIH, that it was the single most successful thing that our government had ever done in the way of a democratic experiment, and we have just heard that same sentiment expressed. I should like to take this opportunity to thank Dr. Fredrickson personally for his major role in making that modern description of the NIH come true. We are truly honored to have had him with us today.

Thank you.

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1980-1991**

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