CHAPTER 8

The National Foundation for Infantile Paralysis: Early Research Programs—Part 2

The Foundation is a holding company for the People. It has several justifications—as critic, as originator, as catalyzer.

Dr. Alfred E. Cohn, *Minerva's Progress*

Q: Dr. Rivers, late in 1939 the post of medical director was created in the National Foundation. Could you tell me what events led to the creation of this post?

Rivers: Up to the time a medical director was appointed in the Foundation, most of the applications for grants were received by a secretary in Mr. O'Connor's law office. This girl, although very competent, had no medical experience, and consequently had no idea whether these applications were worth looking at, much less supporting, and as a result dumped all the applications, the good as well as the bad, into the laps of the various committees. For instance, during the very early days of the Foundation, the Virus Research Committee had to spend a hell of a lot of time just sorting grant applications before they could settle down to discuss those worth discussing. It was a time-consuming job and I can tell you that I and other committee members didn't like it too well. There were other problems. When the various chapters of the Foundation began to give direct medical assistance to polio patients, they quickly found that in many cases they needed expert medical advice, and that the lay people at headquarters were just in no position to give them the advice that they needed. When such questions came up, Mr. O'Connor would turn them over to Paul de Kruif, or if they were particularly pressing he might call me at the Rockefeller Hospital. It was an awkward way of doing business and I think that it was quickly recognized that the Foundation would eventually need the services of a medical person to handle such problems at headquarters.

My knowledge of the appointment of the first medical director of the Foundation stems from a visit that Paul de Kruif made to my office in the Rockefeller Hospital late in the fall of 1939—I don't remember the exact date. The point of Paul's visit was that the National Foundation had great need of a medical director, and that in his view Dr. Donald Gudakunst was the best man available for the job. He was sounding me out. That was all right. Hell, I knew about Dr. Gudakunst long before Paul spoke to me about him. Dr. Gudakunst had received his medical degree at the University of Michigan, and prior to 1939 had had a first-rate career as a public health officer. During the early thirties he had been commissioner of health of the city of Detroit and in 1937 became the commissioner of health for the entire state of Michigan. In addition to his public office, he was also a professor of preventive medicine and public health at Wayne University. There was never any doubt about Gudakunst's ability; however, in 1938 he had a knockdown dragout fight with the governor of Michigan and left his job and joined the U.S. Public Health Service.

I don't know what Gudakunst did for the Public Health Service but it was clear from de Kruif's talk that he was unhappy in his post and was looking for a job more suitable to his abilities. Paul could always add and so could I: it was plain that the Foundation needed a medical director who could sort out grant applications, visit laboratories, and evaluate physicians and their work for the medical committees of the Foundation, and it was just as plain that Gudakunst needed a job. Paul pushed his appointment and in the end he got the job. It just so happened that Dr. Gudakunst made a first-rate medical director.

Q: Did Dr. Gudakunst have any training as a virologist?

Rivers: No. Dr. Gudakunst was primarily a public health man, but he knew a little bit about laboratory work. He was smart and he learned very rapidly. Actually the man did the work of at least two people and, if you want my private opinion, I think that he killed himself working for the Foundation. He worked night and day, and drove himself and others. He never rested and in the end he died of a heart attack. He was one of the hardest workers I ever saw and Paul de Kruif made no mistake recommending him.

Q: Dr. Rivers, how large a department did Dr. Gudakunst administer initially?

Rivers: In the beginning the medical department wasn't very large. However, as the Foundation's interest in medical research, professional education, and medical care grew, new people were added to the department. Gudakunst was an excellent public health man and knew a great deal about problems like epidemiology; however, research and education were just a little bit outside his ken. To help him, a department of research was added to the medical department sometime late in 1942. The first director of this new department was a former big league ball player named John Lavan. Unfortunately, Dr. Lavan didn't work out, and after the war Dr. Harry Weaver was given the job. (I'll talk about Dr. Weaver later.)

Early in 1944 another division was added to the medical department to care for the Foundation's expanded program in physical therapy and nursing education. I forget what its original name was, but in time it became known as the Division of Professional Education. From the beginning it was administered by Dr. Catherine Worthingham, who formerly was a professor of physical education at Stanford University and a past president of the American Physical Therapy Association. I didn't know her very well until I came to the Foundation full time in 1955, but let me tell you she is quite a gal. In the past eighteen years since she has been with the Foundation, she has been responsible for developing one of the finest programs in medical education anywhere. I don't mind admitting that I am very fond of her—but don't you think for one minute that this means I go around patting her on the back. I have fought with her on many occasions, and I want to tell you, she gives as good as she takes.

Q: Would it be fair to say that, when Dr. Gudakunst became medical director, the Virus Research Committee leaned on his initial judgment about applications for grants?

Rivers: If you mean did we trust him to weed out the obviously sorry applications, the answer is yes, and for that matter we still depend on the members of our medical department—Dr. Clark, Dr. Boyd, Dr. Apgar, and Dr. Worthingham—to do that job. I should mention here that Dr. Gudakunst had another important job, as far as grant applications were concerned, and that was to advise prospective applicants on the merits of their applications. For instance, if a fellow submitted an application which Gudakunst knew had no chance of passing the Virus Research Committee, it was his job to advise the applicant that he was not likely to receive a grant and that it might be better to go elsewhere. Now in no case could Gudakunst tell them definitely that they would get no money. He could only tell them that it was unwise to make an application for a grant. If the applicant persisted, Gudakunst was honor bound to put the application through to the committee. The final decision to make a grant or reject an application was always in the hands of the Research Committee, and as far as I know Gudakunst never fudged on these obligations toward a grantee.

Q: Dr. Rivers, was there ever any conflict between Dr. Gudakunst and the Virus Research Committee? I raise this question because early in 1944, several months after long-term grants were made to Dr. Francis and Dr. Paul, Dr. Gudakunst prepared a memorandum that questioned whether the Foundation program in epidemiology was progressing as well as it might.[1]

[1] Proposals for a Meeting of Epidemiologists. Memorandum, Donald Gudakunst to Basil O'Connor, January 7, 1944. See also correspondence, Henry Vaughan to Donald Gudakunst, August 2, 1944; H. J. Shaughnessy to Donald Gudakunst, February 8, 1944; Joseph Melnick to Donald Gudakunst, February 7, 1944; Thomas Francis to Donald Gudakunst, February 21, 1944 (folder, Epidemiologists, General, 1944, National Foundation Archives).

Rivers: During the winter of 1944, Dr. Gudakunst wrote such a memorandum and circulated it both inside and outside the Foundation. Later he called a meeting of all of the Foundation's grantees who were interested in epidemiology to discuss the question of the Foundation's epidemiological program. I never attended that meeting although I subsequently saw some of the correspondence that was written as a result of the meeting. In a word, nothing much came of it. What Dr. Gudakunst did was understandable; you must remember that he was essentially a public health officer, and he undoubtedly felt that too little attention was being paid to the strict meaning of the word epidemiology by Foundation grantees. I myself never felt that people like Dr. Francis or Dr. Paul were spending their money incorrectly. Actually it didn't make much difference to me if in their grant application they said they wanted to study an "epidemiological problem" and wound up studying a straight problem in virology. This didn't happen; I am just giving it as an example. That's the way research is done. You start out to do something and if you run into a snag you do something else. If you didn't, you would never get anywhere, and all you would have to show for your effort is a bruised head from butting your head against a stone wall. That's not very smart. In research you make a start, and if that doesn't pan out you try the wall in a different place until you get a foothold. Other people widen that foothold, and sooner or later the wall disappears, and you discover that you can proceed down the path you wanted to take two or three years previously.

Q: Dr. Rivers, how much pressure was put on grantees for quick results?

Rivers: So far as the Virus Research Committee was concerned, I can say unequivocally no pressure at all. However, I really don't know whether Dr. Gudakunst believed that he should get results quickly as you do when you administer a public health program in a state or a city. In such cases you at least get something done. Whether it amounts to anything or not doesn't matter; at least you get something that you can write a report on. Most of the men on the Virus Research Committee knew something about research, and the committee, as I say, never put any pressure on the boys. Now, on one or two occasions, we did cancel some grants. But we didn't cancel because we thought that they were doing something wrong; we canceled because they weren't doing anything at all, and in such cases I feel that we had every right to cancel. It is true, however, that Dr. Gudakunst felt differently about research and how to handle research problems than I did. For instance, for a long time it was difficult to persuade him about the necessity for giving money to research projects that had no immediate relevance to polio. However, just before he died in 1946 he saw the validity of that approach and even urged the Virus Research Committee to support a grant to William Robbins at the New York Botanical Gardens to do research on phage and a grant to Linus Pauling of the California Institute of Technology to study the structure and nature of proteins and nucleic acid. Unfortunately, Dr. Gudakunst was never able to carry through on those grants because he died. However, his successor Dr. Harry Weaver did.

I want to tell you that Dr. Pauling was not even remotely interested in polio, but in the end his work helped us immeasurably in gaining a better general understanding of the biochemical nature of viruses, polio included. It is a matter of some pride to the Virus Research Committee that Dr. Pauling won a Nobel prize for his work in chemistry while under grant of the Foundation. I should add here that, while the work of Dr. Robbins' laboratory on phage was less fruitful than we had initially hoped for, it fortified us in our belief that such studies would in future prove of enormous value in developing virology as a discipline. Some years later, several Foundation grantees, among them Earl Evans of Chicago and Max Delbrück of the California Institute of Technology, amply confirmed our beliefs of the usefulness of working on phage, and during the last decade such work has truly helped revolutionize our understanding of viruses.

Q: Did Mr. O'Connor ever try to put pressure on the committees?

Rivers: Hell, no. Mr. O'Connor is smart and he had better sense than to try that. He knew that if he tried to put pressure on the committees they would have told him to go to hell and packed up and gone home. They were just as busy in their own work as he was in his.

He valued their independence and their judgment, and if you ask me he depended on it.

Q: Dr. Rivers, from time to time you have spoken of the importance of Paul de Kruif in counseling on early Foundation policy, but it is clear from material in the Foundation files that Mr. O'Connor frequently called upon you and Dr. Karl Meyer for advice.

Rivers: Well, I worked at the Rockefeller Hospital, and if Mr. O'Connor needed anything in a hurry all he had to do was pick up the telephone. He could get me at any time. I think that that accounts some for my being used so often. Karl Meyer stood well in virology and I think that Mr. O'Connor was impressed by Karl. Karl is no dumbbell and, like myself, doesn't like to pussyfoot. If you asked him a question, he would say yes or no, he wouldn't say maybe. Now that kind of an answer was very helpful to Mr. O'Connor. Remember that he knew very little about medical research and nothing about virology, and if he was to function efficiently as an administrator he had to have people who could say yes or no to him. It's pretty difficult to operate on maybes. I could say yes or no, and Karl could too. Karl still can and I still can. I think that this is the reason that Mr. O'Connor called on us. I am not saying that we were always right. Hell, no. But right or wrong, Mr. O'Connor would be able to get his yes or no.

Q: Dr. Rivers, I will return later to problems of epidemiology in another context. Now I would like to ask you about another early administrative officer in the Foundation named Peter Cusack.

Rivers: I knew Peter Cusack slightly. Originally he was associated with Keith Morgan in the administration of the President's Birthday Ball Commission. Later he joined the Foundation as executive secretary, and in that position played a key role in the day-to-day administration of the Foundation. I didn't see much of him, because his function within the Foundation had little to do with science, although it is true that he signed the minutes of the Virus Research Committee.

The main thing that I remember about Peter was that he thought the world and all of Sister Kenny, and I couldn't stand her. Originally she came to this country from Australia, and half of the provinces of Australia liked Sister Kenny and the other half would have nothing to do with her. The virologists that I knew in England told me all about her, and so I was well acquainted with her work before she ever came to America. When she landed in this country, she had introductions to two people; she had a letter of introduction to Mr. O'Connor and one to me. I was sitting in my office at the Rockefeller Institute one day when Mr. O'Connor called me. He said, "Tom, there's a person here who would like to come up and see you." I said, "Who is the person, Mr. O'Connor?" He said, "Well, it's Sister Kenny." I said to Mr. O'Connor, "Would you answer this question, yes or no? First, is she sitting there?" He said, "Yes." I said, "Please answer the next question, yes or no. Have you bought her?" He said, "Yes." I said, "If you have bought her, and if you and I want to remain friends, it's best that I not see her. You can make whatever explanations you wish of my refusing to see her. Because of the friendship between you and me, I'm not getting mixed up with that lady. Sometime I'll tell you why." And that was that.

Well, Mr. O'Connor bought her and so did Morris Fishbein of the AMA. They gave her money and she set up shop under University of Minnesota Medical School auspices. Some years later, she wrote a book on infantile paralysis and the Kenny treatment, and in the preface of that book claimed that the National Foundation and the AMA had asked her to come to this country to demonstrate her treatment for polio. That claim was just a plain lie, because the fact is, the National Foundation and the AMA did not ask her to come here.[2] Within a very brief period of time, the Foundation became critical of the way Sister Kenny set up her courses of instruction at the University of Minnesota and her increasing demands to put everything aside in the Foundation polio research program except the Kenny treatment.

In 1943 things came to a head during the course of a polio epidemic in Argentina. At that time, the Argentine Republic requested President Roosevelt to send Sister Kenny and her team to Argentina

[2] Dr. Rivers is mistaken here: Sister Kenny made no such claims. Cf. E. Kenny, *Treatment of Infantile Paralysis in the Acute Stage*. Bruce Publishing, Minneapolis, 1941, pp. 1–2.

to aid doctors in that country fight the epidemic. Mr. O'Connor called me and asked if Kenny should be sent. I said, "Yes, I think it would be a nice gesture, but I think you ought to send a good doc along with those physical therapists. I wouldn't send them down there alone." He agreed and got Dr. Rutherford I. John, an orthopedist from Philadelphia, to accompany Sister Kenny's team of physical therapists. Sister Kenny's niece was on that team—she was a good physical therapist and a doggone good-looking girl.

Well, that trip was a disaster. What happened was that all the rich people in Argentina gobbled up these therapists to care for their own children, and the purpose of the trip—to instruct the doctors in Argentina in the Kenny method—went by the board. Dr. John got sore as a boil and came home and reported to Mr. O'Connor what had happened, and Mr. O'Connor got sore. He and Sister Kenny had a blow-up, and from that time forward they hated each other. Before the blow-up came, the Foundation gave a luncheon in honor of Sister Kenny and Cusack called me and asked if I would attend. I said, "Yes, I'll come to your luncheon but under one condition, and that is that there will be no reporters present and that nobody will put my name in the newspaper as having had lunch with Sister Kenny." Cusack said, "I can guarantee that," and so I went.

When I arrived, Mr. O'Connor took me over and introduced me to Sister Kenny, and I stuck my hand out. She reared back, put her hands behind her back, and looked at me. She was a big woman, bigger than I was, and she said, "You were the man who wouldn't see me when I first came to this country." I looked at her. "Madame," I said, "I am that man." I then turned around and walked off.

After the blow-up with Mr. O'Connor, the Foundation stopped inviting her to its scientific functions. During the First International Poliomyelitis Conference in New York, she asked to take part in the conferences but was refused. She thereupon got herself appointed as a newspaper woman and attended the conferences in that capacity, but of course couldn't speak. When the Second International Poliomyelitis Conference was held in Copenhagen, she again wrote and asked to be put on the program and was again refused. She came anyway. By this time her health had broken, she had a myocarditis and the beginning of a fairly marked paralysis agitans—you know, the shakes. Prior to the opening of the conference, Mr. O'Connor gave a reception and she attended. I was in the receiving line when in came Sister Kenny—she could hardly walk—when she came to me in the receiving line she stuck out her hand, and I shook it. I am sure she didn't recognize me. Hell, it made no difference to me what she was —the poor old soul was damned near dead—and she was still a person. Shortly after the conference she died.

I think that all of this is rather a sad story, because you know Sister Kenny did do some good. Before she came to America, doctors put patients who had infantile paralysis in splints. Treatment at that time called for the immobilization of the paralyzed limb. I think that it is fair to say that many doctors and therapists on their own were at that time ready to change that practice, but I believe that Sister Kenny got us away from this much faster than we would have in the natural course of events. Kenny favored moving the paralyzed limbs and educating the patient to make that movement. Her big problem was explaining how she got the results she got. She had no notion of the nature of poliomyelitis as a virus disease and certainly knew nothing about its pathology. For example, she thought it was a disease of the muscle. The kindest thing I can say about her ideas of physiology and anatomy is that they best be forgotten. There is no denying, however, that she got effects, and I think that on the whole she did some good.

Q: Dr. Rivers, I would like now to examine your relationships with some of the other medical advisory committees in the Foundation.

Rivers: I don't know how helpful I can be to you here. At best, I will only be able to tell you about the purposes of these committees, some of the people I knew who served on them, and maybe a hassle or two, but not much more.

Q: That will be quite all right. Could you, for example, tell me about your relations with the General Advisory Committee? [3]

Rivers: This committee was one of the oldest committees within the Foundation and if I am not mistaken existed from the early for-

[3] The General Advisory Committee was organized May 15, 1941, and was dissolved on October 8, 1958.

ties until the reorganization of the committee system in the Foundation in the late fifties. Briefly, the General Advisory Committee was responsible for finally approving or disapproving the grants made by the other medical committees. As such, it served as a court of last resort. However, I don't actually remember whether it ever disapproved anything that had previously been approved. Its membership was composed of the chairmen of the various medical advisory committees—the Virus Research Committee, the Committee on Prevention and After Care, and so forth—and distinguished physicians from outside the Foundation, people like Dr. Irvin Abell, who was at one time chairman of the Board of Regents of the American College of Surgeons, and Dr. James Paullin, who had been president of the American Medical Association.

Generally the outsiders on this committee were chosen for their standing within the medical profession and not necessarily because they knew a great deal about polio. They might know something about polio, but it wasn't a prerequisite for being on this particular committee. Take Jimmy Paullin. Jimmy was an Atlanta boy and I had known him since 1909. He was a damn good doc who knew a great deal about pneumonia but not a hell of a lot about polio. Jimmy, however, was the kind of a person who could make a sound medical judgment when presented with the facts, and was very helpful on more than one occassion to the committee.

If anybody on the General Advisory Committee wanted to talk on what the Virus Committee had approved or disapproved, they had to argue with me. That was also true of the chairmen of other committees. Now if some one on the General Advisory Committee raised a question about a grant that had been approved by the Virus Committee, it didn't always follow that I would defend the action of my committee. I might defend it if I believed in it, but if I didn't I would tell them what I thought. You know on more than one occasion I was outvoted by the Virus Committee. The General Advisory Committee no longer exists—it has been done away with. Jimmy Paullin is dead, Irvin Abell is dead, Frank Ober is dead, the old boys are all gone. I don't know that that committee was ever needed, but while it did exist it was an ornament to the Foundation.

Q: Dr. Rivers, did your work for the Virus Research Committee ever bring you into contact with the members of the Committee on Prevention and Treatment of After-Effects? [4]

Rivers: By and large I didn't mix in their affairs unless they impinged on the work of the Virus Research Committee, and that I assure you was not very often. This committee, as its name implies, was largely concerned with clinical investigation and evaluation of various methods of treatment of aftereffects of polio. They dealt with such problems of physical medicine as muscle testing, muscle physiology, and orthopedic repair, as well as with the problems of respirators and splints. A great many orthopedists served on that committee, among them such distinguished practitioners as Dr. Frank Ober of Boston, Dr. George Eli Bennett of Johns Hopkins and Dr. Phil Lewin of Chicago. During the early days of the Foundation, it also had the services of two of America's most distinguished physiologists, Old "Ajax" Carlson of Chicago, and Walter B. Cannon of Harvard. I am not telling any secrets when I say that these two fellows didn't like each other very much—but you would never have known it from their work with the Foundation. When they worked on Foundation matters, they worked closely and in harmony.

The fellow on the Committee on Prevention and Treatment of After-Effects that I knew best was Dr. Phil Lewin, who during the early years was the chairman of the committee. Phil Lewin is now dead. He was slightly older than I. He was a great friend of Paul de Kruif's, and I don't think that it is an exaggeration to say that in his time he was one of the leading orthopedic surgeons in Chicago. He was a rather unique individual. I would say that he was one of the best-hearted men I ever knew, one of the nicest persons I ever knew, but in some respects one of the biggest numbskulls I ever knew. Phil was a damn good orthopedic surgeon, but that didn't prevent him from suggesting—I believe it was sometime in 1941—that the Foundation publish 10 volumes covering every aspect of polio. What we knew about polio at that time could have been put in a small box.

[4] The Committee on Prevention and Treatment of After-Effects was organized on October 6, 1938. It subsequently underwent a reorganization, becoming the Committee on Research for the Prevention and Treatment of After-Effects, on January 11, 1940.

Hell, you don't publish books when you don't know anything, not in my book you don't. I had a hell of a time talking him down—if we had published at that time we surely would have regretted it.[5]

As I said before, in the beginning Phil was the chairman of the committee. Mr. O'Connor liked him personally very much, but at one time it was decided at the Foundation to drop certain people from various committees, and I was delegated to write the letter of notification. Phil Lewin was one of those to be dropped. I thought I wrote a good letter, but I want to tell you that in reply Phil sent Mr. O'Connor one of the hottest telegrams I have ever read. He just didn't want to be dropped. He wondered out loud why in the hell he was being dropped, and he didn't mind telling the Foundation that he was the best person they had ever had, and so on. The funny thing about all this is that neither Phil Lewin nor, for that matter, any other member of the committees of the Foundation ever received a red cent for the work they did, outside of the expenses they incurred attending meetings.

Well, when Mr. O'Connor received that telegram, he came over to see me and said, "Tom, for God's sake, please get in touch with Phil and tell him we have made a mistake." I said, "Yes, Mr. O'Connor," and promptly sat down to write Phil. It was perfectly all right. Phil came to all the meetings after that and acted as if nothing had ever happened. He served on the committee until the day he died.

Phil did one of the nicest things I think I ever saw done. He was married to a wonderful woman who, I believe, was an actor's agent. They were well off financially, but they had no children and later in life they adopted a child who was terribly crippled by polio. I don't know how many times Phil operated on that little boy—I think it was

[5] The debate between Dr. Rivers and Dr. Lewin on the question of publication was one of long standing and apparently erupted in 1939 when Dr. Lewin made the suggestion that the National Foundation prepare a pamphlet on its research programs. Dr. Rivers opposed that proposal, because the suggestion was made that he be the author. When Basil O'Connor informed Dr. Lewin at that time of Dr. Rivers' objections to publication, Dr. Lewin replied, ". . . My immediate reaction to Tom Rivers' answer to you was—first, that it was discourteous, insolent, and dumb; second, it was typical of the full-time worker who recently had his picture in Time."—Philip Lewin to Basil O'Connor, October 7, 1939 (folder, Philip Lewin, Public Relations Files, National Foundation Archives); Thomas Rivers to Basil O'Connor, September 25, 1939 (folder, Thomas Rivers, Public Relations Files, National Foundation Archives). Later, Rivers and Lewin became friends.

seven or eight times. Today that boy can walk, use his arms fairly well, and even ride a horse. Just before he died, Phil and his wife came to one of the annual meetings of the Foundation in Florida. I saw that little boy swimming in a pool with Phil watching over him like a mother hen. He was wild about that little boy, and the little boy was wild about him even though Phil had put him through all those operations. That's the kind of a guy Phil Lewin was. A wonderful person, a damn good orthopedic surgeon—but when it came to science he just wasn't there.

Q: Dr. Rivers, you indicated earlier that the Virus Research Committee supported epidemiological studies of polio. I wonder if the consideration of such grant applications ever brought you into conflict with the Committee on Epidemics and Public Health? [6]

Rivers: Broadly speaking, epidemiology is the study of a disease as it attacks a human population; and while it does have laboratory work connected with it, it is actually a special kind of study which, by its nature, can't be contained within the four walls of a laboratory and has to be done in field. It is true that the Virus Research Committee and the Committee on Epidemics and Public Health were both interested in problems of epidemiology. However, I will say that we didn't tread on each others toes, because I suppose we were different kinds of folks and had different interests and training. With the exception of John Paul, most of the original members of the Committee on Epidemics and Public Health were people who were trained in public health. For example, Dr. Kenneth Maxcy, whom I spoke of earlier, was a professor of epidemiology at the School of Public Health and Hygiene at Johns Hopkins. Another member, Dr. George Ramsay, was commissioner of health of Westchester County, New York. Dr. Herman Bundesen was commissioner of health for the city of Chicago, and Dr. Thomas Parran was Surgeon General of the United States. Earlier in his career, however, Dr. Parran had served as commissioner of health of the State of New York.

[6] The Committee on Epidemics and Public Health was organized on June 6, 1939, and reorganized on September 30, 1947, as the Committee on Virus Research and Epidemiology.

Dr. Parran was and is quite a boy and I think that I ought to say a few words about him. I don't think that history will remember him for his contributions to polio, but it will remember him for his general accomplishments in public health administration, and as the man who had the guts to bring the word syphilis into the sitting rooms and parlors of the homes of people in the United States. You might say, what kind of a wonderful trick is that? Wasn't the disease in the bedrooms of those same homes? It was, but I want to tell you that at one time you didn't use the word syphilis in public, much less in the home. It was a forbidden word, and even doctors in talking about the disease used the word luetic or lues. Parran brought the word into the home and, calling a spade a spade, was able to use it as the opening gun in a national venereal disease campaign. Syphilis is a treacherous disease, and knowing it by name has helped bring it under control. When antibiotics came in, there was hope that we might be able to wipe out the disease entirely, but we haven't, and it is distressing to read about the rise of the disease, particularly among teenagers. However, Dr. Parran's work has had one lasting effect. Today people know about the disease, and when they get it they seek treatment. To be sure, we still have a hell of a lot of primaries and secondaries. However, because of early treatment, we no longer see the tertiary paretics and tabes that I saw when I was a young doctor. That's on the credit side of the ledger and we owe that to Tom Parran.

I don't want you to get the idea that everybody on this committee was of Dr. Parran's caliber. I can tell you now that Dr. Herman Bundesen wasn't. Dr. Bundesen was the commissioner of health for the city of Chicago and one of Paul de Kruif's friends. Like so many of Paul's friends, he too was invited to join the Foundation in an advisory capacity. I suppose that initially Mr. O'Connor was impressed with the importance of Dr. Bundesen's position and assumed that, as commissioner of health of the city of Chicago, he ought to know something. It was a fair assumption, but, as far as competence in epidemiological research was concerned, Dr. Bundesen just didn't have it. The only knack he did have was the knack of getting along with people. He was a doggone good politician, and he got along well with the politicians of Chicago, particularly Mayor Kelly. Come to think of it, being a politician was not an unimportant talent, and I'll have to admit that Dr. Bundesen probably accomplished as much for public health in Chicago with those politicians as anybody could have.

Early in 1941 Dr. Bundesen presented a plan to the Foundation for a national epidemiological research program.[7] In brief, his plan was for the Foundation to hire its own epidemiologist to do epidemiological research in the field during the course of epidemic periods. It was the most detailed and all-inclusive program I have ever seen and just about covered everything. It called for an expert epidemiologist, an expert diagnostician in polio, an expert virologist, an expert this, and an expert that. The only thing that Dr. Bundesen seemed to forget was, where in the hell he was going to get all of these experts without emptying medical schools and the state and federal public health services of their people. Believe me, when I tell you that it was impractical not only from the point of view of acquiring personnel. The purpose of doing epidemiological research just seemed to elude Dr. Bundesen, and he could never really differentiate between trying to control an epidemic and doing epidemiological research. Some of the all-inclusiveness of the program was downright silly and a dead giveaway that Dr. Bundesen just didn't know what the hell he was talking about. For instance, he made a great fuss about wanting to study the possible relationship of milk pasteurization and postpasteurization protection to the spread of polio. By 1941 it was clear to anyone who had spent any time at all on the problem of polio that milk played an insignificant role in the spread of polio. There were only two very small authenticated epidemics on record that could at all be traced back to contaminated milk. One of these had occurred in New York State and was investigated very thoroughly by Lloyd Aycock of Harvard, who traced the origin of this particular epidemic to a milker who was infected with polio.[8] But it was a unique case, and most virologists and epidemiologists realized that studying milk would not be a fruitful way of spending research time and money.

Eventually Dr. Bundesen sent this plan to virologists and epi-

[7] Herman Bundesen, Suggested Programme for the Study of the Epidemiology of Poliomyelitis, presented to the Committee on Epidemics and Public Health of The National Foundation for Infantile Paralysis (no date, probably late 1940). (Folder, Medical Meetings, 1940, National Foundation Archives.)

[8] W. L. Aycock, "The epidemiology of poliomyelitis with reference to its mode of spread," *J. Amer. Med. Assoc.*, vol. 87:75 (1926).

demiologists all over the country for comment, among them people like Dr. John Paul of Yale, Dr. John A. Ferrell of the International Health Board of the Rockefeller Foundation, and Dr. Charles Armstrong of the U.S. Public Health Service. The replies were all very polite and spoke very well of the importance of doing epidemiological research as a principle and then ripped hell out of the plan. I don't remember that anyone had a good thing to say about Dr. Bundesen's specific research proposals. You can believe me when I say that I wasn't the only one who made critical comment. I don't know whether Dr. Bundesen ever really expected to get such frank analysis of his proposals, but I assure you that he got them.[9]

However, that didn't end the matter. Dr. Gudakunst and others in the Foundation were convinced that the Foundation should have its own epidemiologist, and tried to cut Dr. Bundesen's proposals down to size. In the end, the Foundation held a special meeting in Chicago to discuss the feasibility of the modified proposals. I was invited but I didn't go. However, other virologists and epidemiologists that I know did attend. I am sorry now that I didn't go, because I understand that there was a knockout teardown battle. The upshot was that Bundesen's proposals were finally buried, and the Foundation decided to pursue epidemiological research by making grants to people like Kenneth Maxcy, John Paul, and Tom Francis, which I might add was a hell-of-a-lot smarter way of attacking the problem.

The Bundesen affair caused a commotion in the Committee on Epidemics and Public Health and some committee members began to wonder about the usefulness of the committee and resigned. After World War II, when it became clear that a special committee was not needed to deal with problems of epidemic aid, and that the problems relating to research in epidemiology were best handled by the Virus Research Committee, the two committees were merged. Today

[9] Typical of the many sharply critical letters Dr. Bundesen received are L. L. Lumsden to Herman Bundesen, February 19, 1941; C. C. Dauer to Herman Bundesen, February 20, 1941; Abel Wolman to Herman Bundesen, February 24, 1941; Roy Feemster to Herman Bundesen, February 25, 1941. Thomas Rivers to Herman Bundesen, February 26, 1941; George M. McCoy to Herman Bundesen, February 27, 1941; Charles W. Armstrong to Basil O'Connor, February 27, 1941; W. Lloyd Aycock to Basil O'Connor, March 3, 1941; Edward S. Godfrey to Herman Bundesen, March 3, 1941; W. G. Smillie to Herman Bundesen, March 3, 1941 (folder, Herman Bundesen, Public Relations Files, National Foundation Archives).

the Virus Research Committee is still officially known as the Committee on Virus Research and Epidemiology. Actually, epidemiology was never the principal concern of the Committee on Epidemics and Public Health. I would say that basically they were concerned with such problems as what the Foundation could best do to render assistance to stricken areas in times of epidemics, and what form such assistance should take.

Q: Dr. Rivers, what kind of public health advice could the Committee on Epidemics and Public Health give to a community faced with an epidemic, let us say, in 1943?

Rivers: It is interesting that you should ask that question, because I remember that, at the very first meeting ever held of the Virus Research Committee, we were asked whether we knew enough about the epidemiology of polio to recommend shutting down public schools in times of epidemics. The members of the committee were polled, and half of those present advised closing the schools, while the other half advised keeping them open. The only time that the committee achieved some semblance of unanimity was when we were asked whether kids should be kept out of swimming pools and crowds. We all agreed that children should be kept out of pools and crowds, but, for heaven's sake, don't ask me on what basis we reached such a conclusion. It's my feeling that we gave that advice as a reflex action, because we certainly had no hard scientific evidence on which to base such advice. Even knowing as much as we do about polio today, and it's a hell of a lot more than we knew in 1943, I still wouldn't know what to tell a public health officer to do during an epidemic. All I could possibly do which would be rational or effective would be to advise him to vaccinate the population in his area. But I wouldn't know enough to tell him to keep people out of swimming pools or ball parks.

I think that the lack of good hard advice was precisely the thing which led Dr. Gudakunst to circulate the memorandum you mentioned before on epidemiology. In 1942 the incidence of polio in the United States happened to be at a very low level, but in subsequent years it began to rise once more. When that happened, Dr. Gudakunst, as an old public health man, began to reach for something concrete he could tell the public. Lo and behold, when he went to people like Dr. Francis and Dr. Paul, who were working on problems of the epidemiology of polio, they could not give him the hard and fast advice and rules that he wanted. The funny thing about all of this is that their epidemiological work at the same time was giving us greater insights than we had ever had before into the nature of polio. Sure, they were groping on the role of the fly in polio epidemics, sewage, and water-borne epidemics, but, hell, that's what scientific research is—it's a search. As far as public health advice on what to do during a polio epidemic went in 1943, the committee couldn't give any better advice than the course which was followed by Dr. Haven Emerson during the great polio epidemic of 1916.

Today we don't actually know how many cases of polio occurred in the United States during that epidemic. At that time it was not compulsory to report polio cases, and many states didn't keep figures on the incidence of the disease. The cases that we do have a record of are, of course, far below the actual number which occurred. In New York City alone, over 9000 cases were reported during the summer and fall of 1916.[10] The epidemic actually began in mid-June but was still raging in September, when the mayor and other city officials called on Dr. Emerson, who was then commissioner of health, to decide whether to open the public schools for the fall term. Many physicians in the city urged that the schools be kept shut, but Dr. Emerson laid his head on the chopping block and fought to open them. In the end he won out, the schools were opened, and to everyone's happy surprise the incidence of polio in the city began to decline.

Today, looking back, we can say that it was coming to that time of year when the incidence of polio would drop anyway. But what was probably more important was the fact that when children went to school they came under school discipline—they didn't run around

[10] There are two excellent contemporary analyses of the polio epidemic of 1916: New York City Department of Health, *A Monograph on the Epidemic of Poliomyelitis in New York in 1916*. New York, 1917; and C. H. Lavinder, A. W. Freeman, and W. H. Frost, *Epidemiological Studies of Poliomyelitis in New York City and Northeastern United States during the Year 1916*. Public Health Bulletin 91. Washington, D.C., 1918.

putting their hands in each others mouths, and they were more careful in their personal hygiene going to and from the bathroom. That's hindsight. Actually, Dr. Emerson was very courageous to give that advice. He certainly had no way of knowing what would happen, because our experience with polio at that time was extraordinarily limited. Hell, if that epidemic had continued, they probably would have nailed him to the door.

When I came to serve on the New York City Board of Health in 1937 I got to know Dr. Emerson very well. He was a member of the New England Emerson family and, like his forebears, was of an independent turn of mind. Physically he was tall and rangy, and in the days that I knew him he wore a kind of rabbity moustache. I don't know how old he lived to be, but I believe that he was in his eighties when he died. He was vigorous even in old age, and I remember going out to see him at his home, a year or two before he died, and finding him chopping wood and doing other household chores. He was a tower of strength to the Board of Health of the City of New York, even after he retired. He attended meetings faithfully and invariably sat to the left of the commissioner of health. I used to sit beside him, and I would always get a charge to hear someone raise a problem as if it were brand new, only to have Dr. Emerson tell him how the board handled it twenty or thirty years before, and what mistakes had been made in setting up policy. Dr. Emerson's memory was extraordinarily important for the board, because frequently there was no record of the debates attending earlier decisions, and I want to tell you that the same problems came up over and over. Dr. Emerson helped keep things in historical perspective for the board and in so doing kept it from becoming stultified.

Q: Dr. Rivers, since you had this favorable opinion of Dr. Emerson, why did you later turn down his application to the Foundation for a grant to do research on problems of the epidemiology of polio? [11]

Rivers: The fact that I liked Dr. Emerson had nothing to do with the case. I was very fond of Dr. Emerson, but I didn't think that he could do research. That doesn't mean that I didn't think he was wise

[11] Haven Emerson, Application for a grant to the National Foundation for Infantile Paralysis, April 18, 1939 (folder, Haven Emerson, National Foundation Archives).

or could be helpful. During the 1940's the American Public Health Association published recommendations on what to do in times of polio epidemics. Dr. Emerson played an important role in keeping these recommendations up to date and advising the people charged with publishing them. I'd give him money for that kind of work any time, because he was a master at it and wiser than anyone I ever knew. Make no mistake, he was one of the pioneers in American public health—a great figure—but, damn it, he wasn't a research man, and I would never give him any money for research.[12] That doesn't mean that he couldn't think. I believe that I could tell you a story right now that would indicate the breadth of Dr. Emerson's views.

In the middle of the 1916 polio epidemic, Dr. Emerson asked the Rockefeller Foundation for a grant to help fight the epidemic. They listened and later made a rather substantial grant to the City of New York. However, on receiving the check, Dr. Emerson wrote the Foundation and said that he hoped that this grant wouldn't prejudice his application for a future grant to fight alcoholism, because he felt that the latter was a more important public health problem. You would have thought that the polio epidemic would have absorbed all his thought and energy. It didn't. So far as I know, his dislike for alcohol persisted right up until his death. It was a phobia with him. I personally could never see why he got so wrought up about alcohol. There is nothing sinful about getting drunk anymore than it is sinful to eat green apples and get a bellyache. Hell, when I was younger I used to drink; to be sure I later quit. But I didn't quit because it was sinful; I quit because it bothered my ulcer, not because it was wrong.

Q: Dr. Rivers, an examination of the grants made by the Virus Research Committee reveals that all such grants were made to investigators who were attached to universities and medical schools. Were any grants ever made to investigators who might have had "good ideas" but no institutional attachment?

Rivers: The Virus Committee rarely made such grants. In fact it was our policy to discourage such applications, if at all possible. On its face, such a policy might seem harsh and discriminatory to the lay-

[12] For a measure of Emerson as a public health thinker, cf. H. Emerson, *Selected Papers*. W. K. Kellogg Foundation, Battle Creek, Michigan, 1949.

man. Actually there was good reason for it. Virus research requires good laboratory facilities. It goes without saying that the doctor who was not connected with a university, medical school, or hospital would have had to expend considerable funds to build and equip such a laboratory. If he wanted to work on polio, it would have also been necessary to construct animal houses to keep his experimental monkeys, rabbits, and mice. I know of no private investigator who had the wherewithal or interest to do this. It is true that on occasion we got applications from young doctors who had interesting research ideas and at such times we did try to encourage them.

I remember that once a young doctor in the Bronx named Benjamin Sandler wanted to study the effect of disturbance in carbohydrate metabolism as a factor altering susceptibility to poliovirus. For the time it was a nice idea; however, the problem that the committee faced was that Dr. Sandler had had no extensive experience in handling viruses. We kicked it around awhile and finally told him that we would be willing to assist him, if he could get help from a virologist approved by the committee. I guess he felt that he could do it by himself, because he never did present a virologist for the committee's approval, and in the end we rescinded our approval for the project. I don't know how long this dragged on, but it went on long enough for me to get sore at the guy.[13]

I don't want you to think that, because an applicant was a doctor, it necessarily followed that he had to have good research ideas. Often the reverse was true. Once we got an application from a doctor in California for a study of the relationships of areas bounded by high tension wires and the incidence of polio. It was his unique notion that the electrical currents given off by high tension wires made cells susceptible to poliovirus. Another time considerable pressure was put on the Virus Research Committee to try rabies vaccine as a prophylactic against polio. Well, that idea didn't appeal to me very much because it meant giving rabbit brain and cord to people. It was well known,

[13] Rivers later became disenchanted with Sandler's idea and opposed it, although originally he had supported him. See Thomas Rivers to Gilbert Dalldorf, June 21, 1940; Thomas Rivers to Donald Gudakunst, February 21, 1941 (folder, Benjamin Sandler, Public Relations Files, National Foundation Archives). In 1952 Sandler published a volume called *Diet Prevents Polio* (privately published), which argued that low blood sugar or a diet of high sugar content enhanced susceptibility to poliomyelitis.

even at that time, that a certain number of people developed a demyelinating encephalitis after receiving antirabic treatment. Furthermore, I had produced such an encephalitis experimentally by injecting monkeys repeatedly with rabbit brain tissue. The applicant would have had to kill me before I supported such a fool notion.

Now, the bald fact is that the Foundation received a great many such crackpot applications and letters and for that matter still does. The Virus Research Committee always answers such letters and applications in a friendly way, indicating that the ideas are of interest but that the Foundation cannot support them because they are not likely to yield significant results. It never pays to make such a crackpot mad, and you can't cure one so far as I know, so we brush them off gently.

Q: Dr. Rivers, you earlier made the point that in 1940 there was a paucity of trained virologists in the United States. As a matter of fact one of the purposes in setting up the virus laboratory at the School of Public Health at the University of Michigan was to train young virologists. I wonder if you would speak to the point of the pool of virologists that existed in the United States about 1940.

Rivers: I think that it would be fair to say that in 1940 there were a small number of virologists in the United States who were capable of doing competent research in the field of polio or, for that matter, other virus diseases. I am not going to say how many, because I don't want to go around upsetting people—just let me emphasize that it was a small number and let it go at that. It was pretty obvious to the Foundation that, if it was to get any work done in poliomyelitis, it would have to train some youngsters to carry on when senior investigators went to their reward. Although Mr. O'Connor agreed that it was proper for the Foundation to undertake the training of such young investigators, there was no machinery then within the Foundation to get such a program under way.

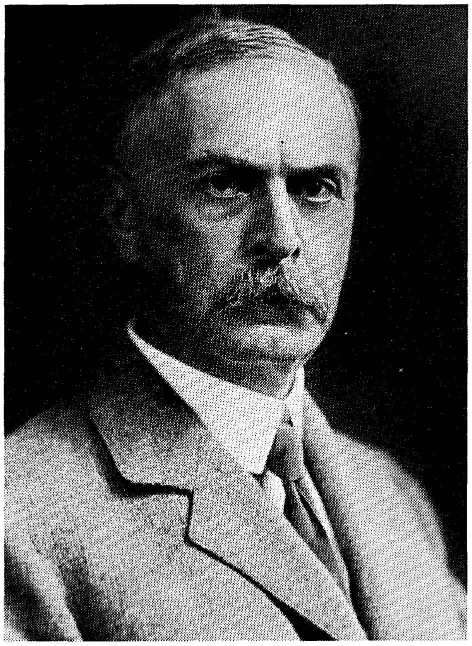
Sometime in 1941 the Rockefeller Foundation gave a grant to the National Research Council to set up a fellowship program in medicine and closely related fields, and I thought that if we approached the Research Council properly it might also possibly be persuaded to administer such a program for the National Foundation. Mr. O'Connor approved the idea, and I got in touch with Dr. Milton Winternitz and Dr. Louis Weed of the National Research Council and asked if they would administer such a program if we gave them the money to carry it out. They agreed and in 1941 it got under way.

I conceived of the fellowship program in broad terms. For example, I didn't think that fellowships should be restricted to training people to work on polio alone. I wanted to train broad-gauged virologists and, if necessary, to give them solid backgrounds in the basic sciences, such as physics and biochemistry. Mr. O'Connor wanted an even broader program and urged that fellowships be granted to train orthopedic surgeons as well. In the beginning, fellowships were offered to orthopedists. Unfortunately, however, only one or two were able to take advantage of the opportunity because the war intervened. In the end, the fellowships were awarded to those who wanted virological training and experience. I must say that it was a very successful program, and many of the boys who were trained under its auspices later went on to distinguished careers in virology. For instance Dr. Joseph Melnick, now a professor of virology at Baylor University Medical School, and Dr. Herbert Wenner, a research professor of pediatrics at the University of Kansas Medical School, got their start in virology under a fellowship grant which allowed them to work with John Paul in the Yale polio unit. I think that almost everybody in America today knows that Dr. Jonas Salk developed the first effective polio vaccine with the aid of grants from the National Foundation, but few know that he was trained in virology by Tommy Francis at the University of Michigan under the Foundation fellowship program. One fellowship holder, Dr. Fred Robbins won a Nobel prize while working with John Enders at Harvard, and today is a professor of pediatrics at Western Reserve Medical School. You know, there is a story about that Nobel prize.

It is said that, when Dr. Enders was informed that he had won the Nobel prize for growing poliovirus in nonnervous tissue, he indicated to the awards committee that he would not accept the prize unless Dr. Thomas Weller and Dr. Fred Robbins, his two young assistants, shared in the honors, and that in the end, because of his stand, the award was given to all three. Some claim that the story is apocryphal,

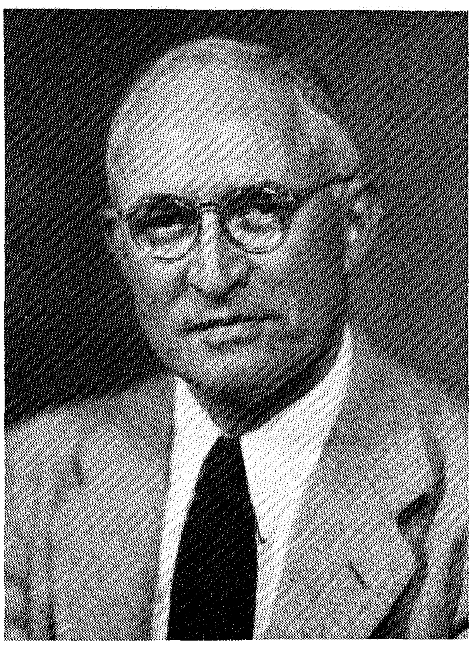
Thomas Rivers

(1) As a member of the Rockefeller Institute, 1927; (2) on receipt of the Sc.D. (hon.) from the University of Chicago, 1941; (3) in his laboratory at the Rockefeller Hospital, ca. 1949; (4) in his office at The National Foundation, December 1961.



Karl Landsteiner
ca. 1930

Simon Flexner
ca. 1920

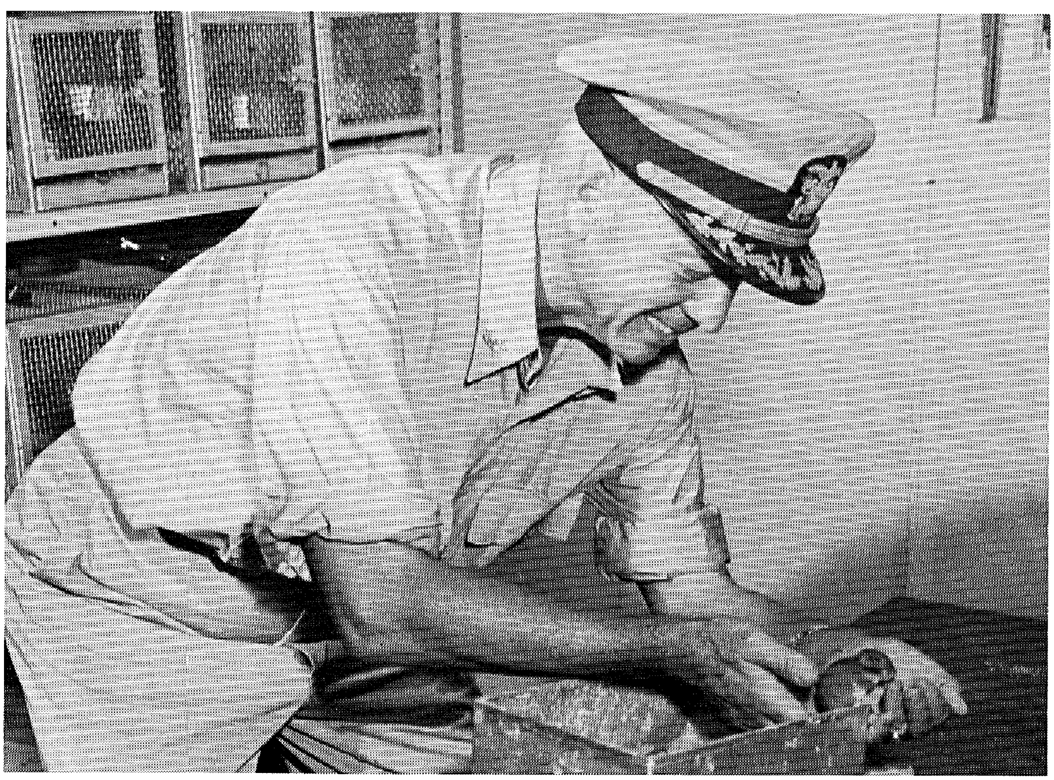


Peter K. Olitsky
in the 1950's

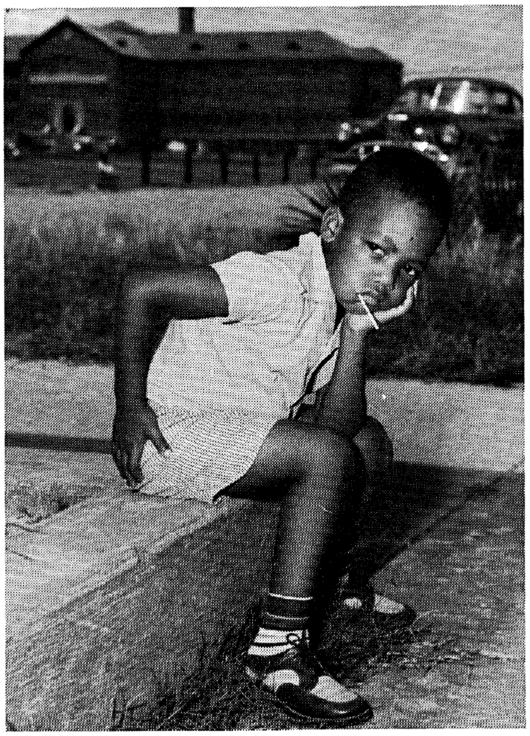
Associates of Thomas Rivers in virus research and immunochemistry at the Rockefeller Institute

(photographs courtesy of Simon Flexner Papers and Peter K. Olitsky Papers, American Philosophical Society)

Mayor Fiorello La Guardia and Thomas Rivers
at the Health Exhibit, New York World's Fair, 1939



Thomas Rivers with experimental animal, Guam, 1945
(photograph by U.S. Navy)

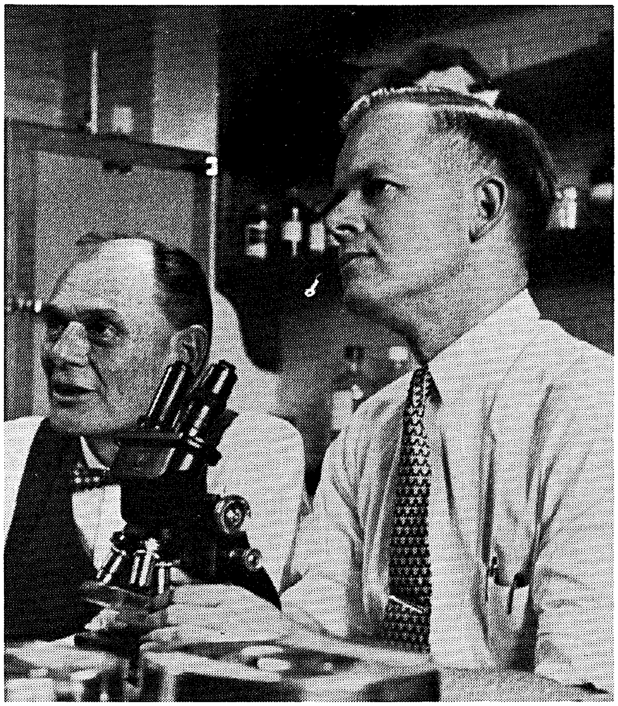


Thaddeus Vinson of Montgomery, Alabama, following inoculation with gamma globulin, July 1953

The process of immunization
(photographs courtesy of The National Foundation)

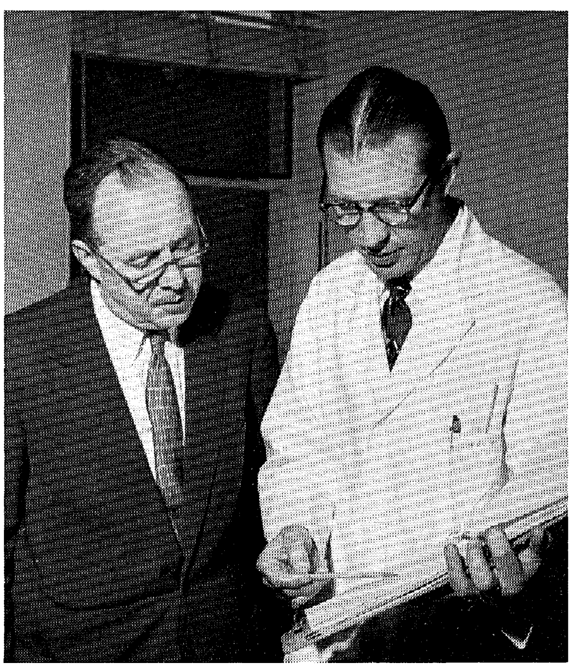
Jonas E. Salk administers Salk vaccine to his son Jonathan, September 1953, while Mrs. Salk and a nurse look on

Some polio pioneers

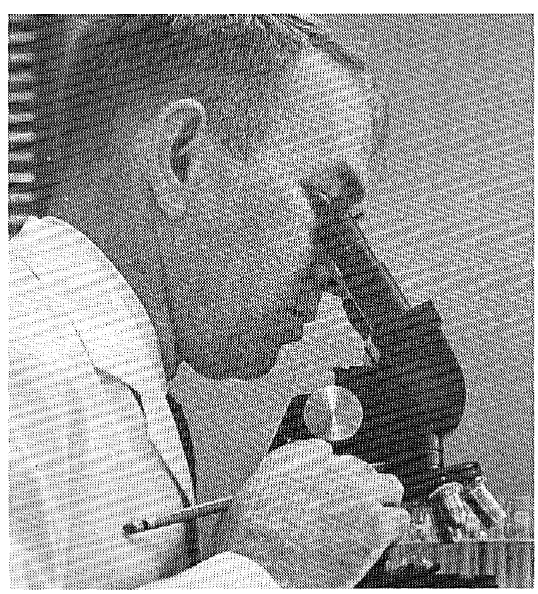


John Enders and Thomas Weller,
Harvard Medical School, 1954
(photograph by John B. Loengard)

Albert B. Sabin and Jonas E. Salk at the Third International Poliomyelitis Conference, Rome, 1954, with Basil O'Connor in background *(courtesy of The National Foundation)*



Thomas Francis, Jr., and Gordon C. Brown at the University of Michigan, 1954 *(courtesy of The National Foundation)*



Herbert Wenner at the University of Kansas, 1958 *(courtesy of The National Foundation)*

15

BULLETIN

ADVANCE FOR USE AT 10:20 A.M. TODAY

POLIO (TOPS 3)

(ADVANCE) ANN ARBOR, MICH., (AP)-THE SALK POLIO VACCINE IS SAFE, EFFECTIVE AND POTENT, IT WAS OFFICIALLY ANNOUNCED TODAY.

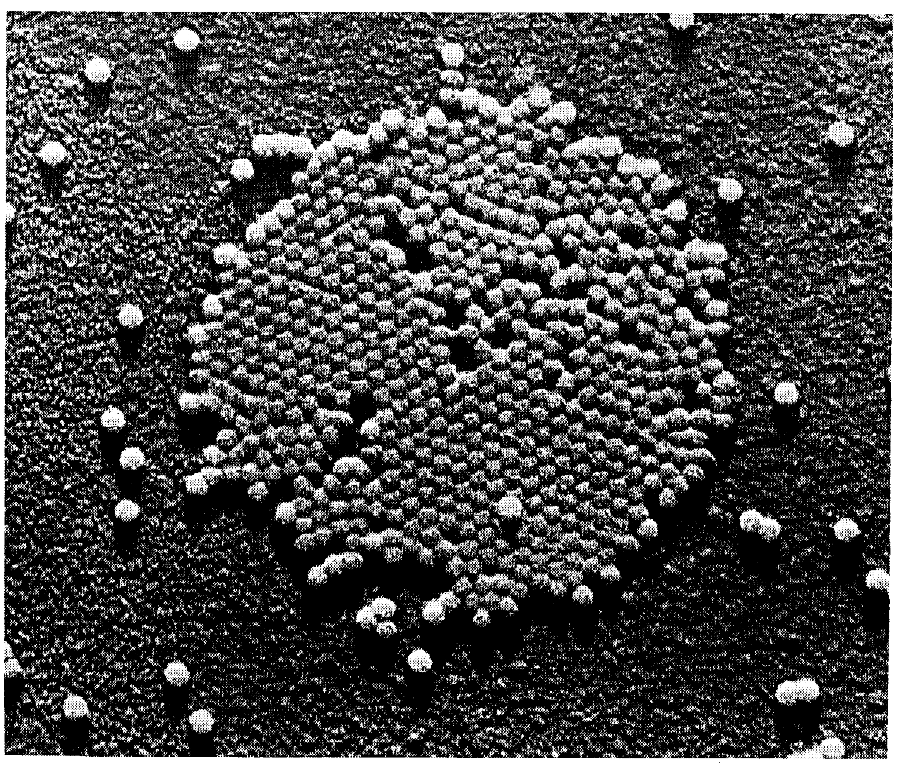
END ADVANCE
JC919A 4/12

Associated Press dispatch, April 12, 1955

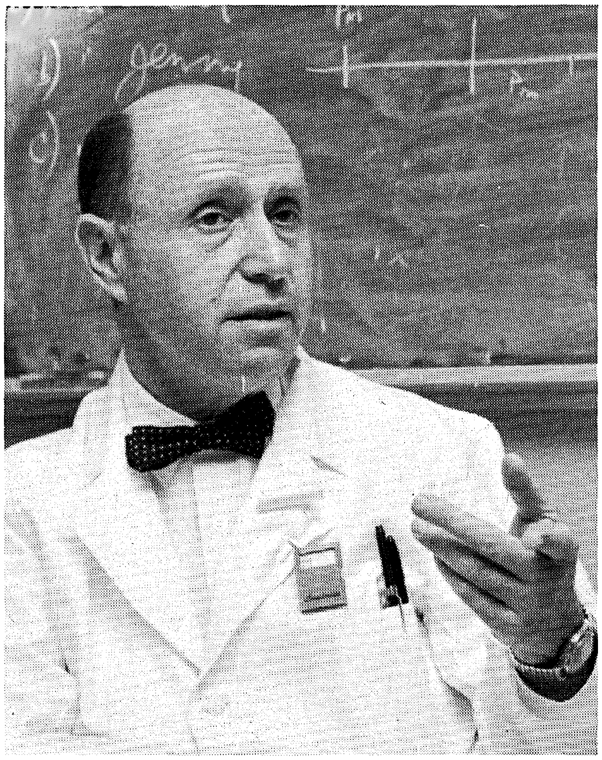
At the dedication of the Polio Hall of Fame, Georgia Warm Springs Foundation, January 1958
(courtesy of The National Foundation)

Thomas Rivers, Charles Armstrong, John R. Paul, Thomas Francis, Jr., Albert B. Sabin, Joseph L. Melnick, Isabel Morgan, Howard A. Howe, David Bodian, Jonas E. Salk, Mrs. Franklin D. Roosevelt, and Basil O'Connor

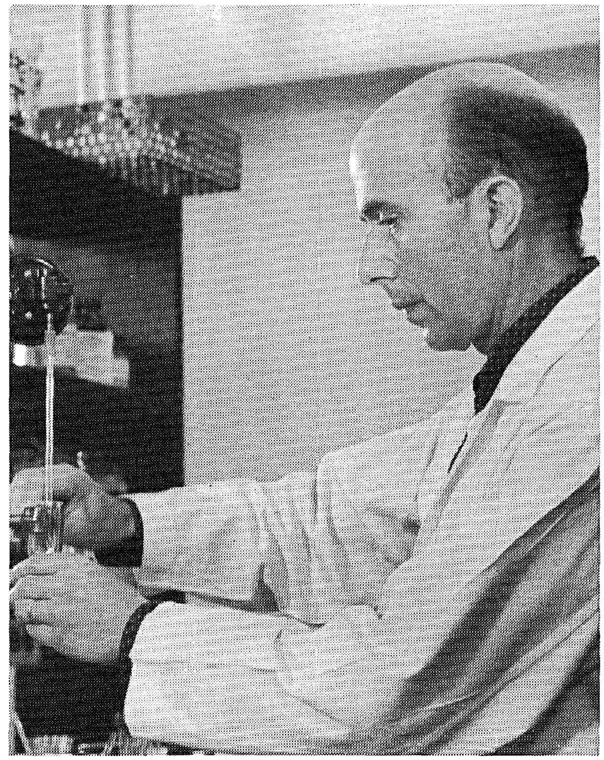
Basil O'Connor (left) and Harry Weaver
of The National Foundation
(courtesy of The National Foundation)



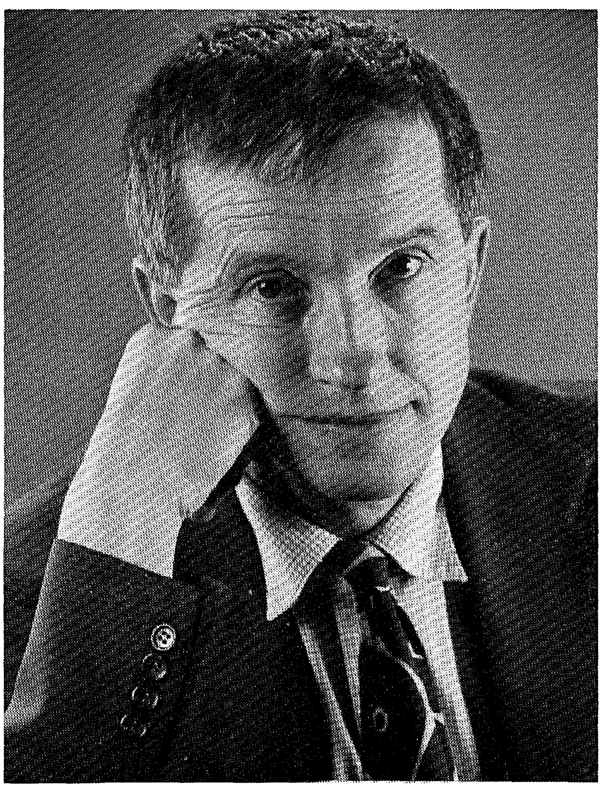
The MEF[1] strain of type 2 polio, magnified approximately
90,000 times
(photograph by Robley C. Williams)



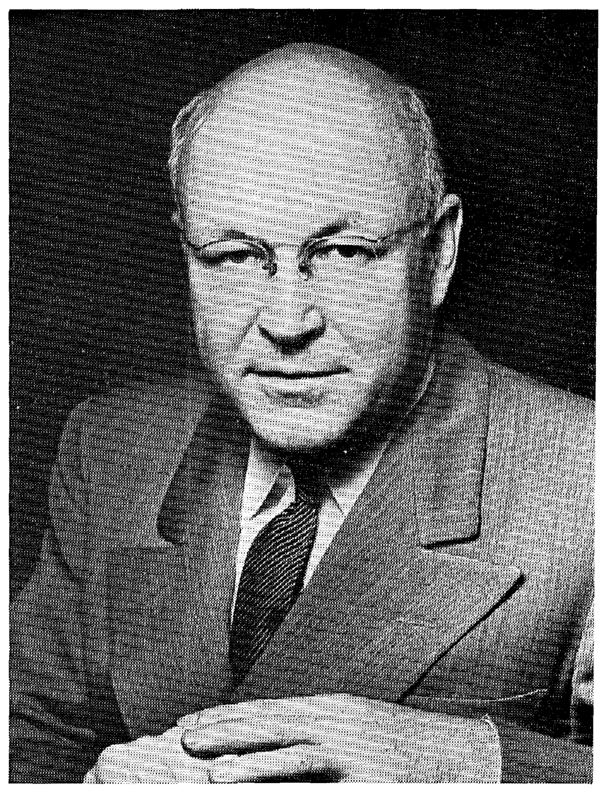
Theodore Puck



Renato Dulbecco



Max Delbrück



Wendell Stanley

Some contemporary virologists
(courtesy of The National Foundation)

but I believe it, because I know John Enders, and it sounds like something he would do. I'll tell you why: In September 1961, just after Dr. Enders announced the perfection of a vaccine against measles, *The New York Times* wrote a very laudatory editorial about Dr. Enders' accomplishment in developing such a vaccine. It was a good editorial, but John didn't thank them for it. Instead he wrote the *Times* a letter explaining the importance of collaboration in research. I have kept that letter in my desk since it appeared, and I show it to anybody who gives me a chance. I think that it should be framed and hung in every laboratory in the country. It's that kind of a letter, and I would like to insert it here:

COLLABORATION IN RESEARCH

To the Editor of the New York Times:

Editorial reference was made to our work on measles and poliomyelitis in your edition of Sept. 17. I wish to express my deep appreciation of these favorable comments on our work.

For the sake of accuracy, however, I would emphasize the fact that whatever may have been accomplished represents the joint product of many co-workers supported by several institutions. In the studies on measles virus and vaccine, essential contributions were made by Thomas C. Peebles, Milan V. Milovanovic, Samuel L. Katz and Ann Holloway. In the researches on the growth of polio virus the role of Thomas H. Weller and Frederick C. Robbins was as important or more important than my own.

Without the generous provision of financial aid and physical facilities not only by Harvard University but also by The Children's Hospital Medical Center, Boston, The National Foundation, The Armed Forces Epidemiological Board, The United States Public Health Service and the Children's Cancer Research Foundation, in which a large part of our laboratory is situated, nothing could have been done.

To me it seems most desirable that the collaborative character of these investigations should be understood, not solely for personal reasons but because much of all modern medical research is conducted in this way.

JOHN F. ENDERS

Professor of Bacteriology and Immunology
at the Children's Hospital,
Harvard Medical School
Boston, Sept. 20, 1961.

[Letter to *The New York Times,* October 1, 1961]

After World War II the fellowship program was broadened still further to include training for pediatricians. Several young doctors took advantage of these new training opportunities, among them Edward H. Ahrens of the Rockefeller Hospital, who used his fellowship to study the biochemistry of fats with Lyman Craig. Later he got a one-year extension of the fellowship to do clinical work with Rustin McIntosh at the Babies Hospital. Well, that particular training caused me some anxious moments. Rusty knows a good fellow when he sees him and he immediately offered Dr. Ahrens a position on the staff of the Babies Hospital. I topped that offer and got him back to the Rockefeller Hospital. Today the entire fifth floor of the hospital has been turned into a laboratory for Dr. Ahrens and his associates. He is extraordinarily productive and don't think that people don't know it. Just last year [1960] three chairs of pediatrics at Columbia, Cornell, and N.Y.U. became vacant at the same time—as a result of the retirement of Rustin McIntosh, Sam Levine and Emmet Holt, Jr.—and Dr. Ahrens was offered all three posts. It was unusual to say the least. In part I credit the Foundation for Dr. Ahrens' achievement, because, without the support given by the Foundation's fellowship program, it is doubtful that Dr. Ahrens could have gotten the training that has made him the superb investigator that he is today.

Q: How long did this fellowship program with the National Research Council last?

Rivers: I think that the program continued for about ten years. In time we became disenchanted with the way the National Research Council administered the grants. Let me amend that and say that I became disenchanted. I thought that the grants committee of the National Research Council was becoming stuffy in the way it made its awards. On several occasions it amended what I thought were perfectly valid requests for a three-year study program to a one-year program. One of these grants involved Dr. Ahrens, and I got damned hot under the collar. I don't think that the way I felt was the prime factor that led to the termination of the Foundation fellowship program under auspices of the National Research Council; actually it was a combination of factors. Many people at the Foundation thought that the Foundation did not receive adequate recognition of its role in granting the fellowships—the fellowships were known as National Research Council Fellowships—and they urged that the Foundation administer its own grants. By this time, the Foundation had already established its Division of Professional Education under Dr. Catherine Worthingham, and the responsibility for giving Foundation fellowship grants was eventually transferred to her division.

Q: Dr. Rivers, before the National Foundation fellowship program was begun, did the Virus Research Committee ever contemplate giving individual fellowship grants?

Rivers: Yes. In 1939 I persuaded the Virus Research Committee to consider giving Albert Sabin a fellowship to study with Thé Svedberg in Sweden. Dr. Svedberg's lab at that time was a leader in techniques of ultracentrifugation, and I thought that such training would be useful to Albert in his virus work. But you know, the rascal turned me down. I thought I had it all set, but he turned me down. The circumstances were these.

Sometime in 1938 a number of people at the Rockefeller Institute began turning up a number of curious organisms. They were filterable, but their characteristics were such that you couldn't classify them as viruses; for example, they could live and multiply outside of the living cell. Actually, they belonged to a group of organisms which are called PPLO or pleuropneumonia-like organisms. This was not the first time that such organisms had turned up in research, and scientists generally held them responsible for the pleuropneumonia affecting the lungs of cattle and another disease involving the udders of sheep and goats. Today some scientists believe that such organisms cause a disease known as Reiter's disease, which is characterized by a purulent discharge from the penis without gonococci. Now PPLO have turned up in cases of Reiter's disease, but I personally don't think that they cause the condition. I don't think that anybody knows what causes Reiter's disease, but I think that everyone is agreed that it is not a venereal disease like gonorrhea.

To get back, one of the people who had turned up these organisms was a boy by the name of Tom Brown. Today Dr. Brown is a professor of medicine at the Georgetown Medical School in Washington, but at that time he was a part of Homer Swift's setup at the Rockefeller Hospital working on rheumatic fever. One other person who turned up these organisms was Albert Sabin. Dr. Sabin, in the course of routine passage and maintenance of toxoplasma in mice, observed a nervous syndrome that he had never seen before and upon examination turned up these organisms. He found that, when he injected mice intravenously and intraperitoneally with cultures of these organisms, the mice developed an experimental polyarthritis that bore a marked clinical and pathological resemblance to human rheumatoid arthritis. Later he found another strain of PPLO that in the process of attacking certain cells lining the inside of the chest and abdomen of experimental mice gave off a toxin which damaged parts of the brain and led to symptoms occurring in rheumatic fever in man. Albert thought he was on to something and, of course, was unwilling to give it up to go to Sweden. I didn't blame him—later it turned out to be a blind alley, and I believe that he dropped it.

Q: Dr. Rivers, were members of the medical advisory committees of the Foundation allowed to make applications for grants?

Rivers: For a number of years after the Foundation was formed, members of the medical advisory committees were allowed to put in applications for grants. For instance, Dr. Karl Meyer, who served on the Virus Research Committee, received any number of grants during his tenure on the committee. The procedure which we followed in such cases required the committeeman requesting a grant to leave the committee meeting when his application came up for consideration. It was awkward, and I for one was relieved when the Foundation decided that, if you were on a medical advisory committee, you just couldn't put in for a grant. It just seemed to be the better part of wisdom to do so. I am not saying that the Foundation discovered that some guys said, "You vote for me and I'll vote for you." So far as I know, that kind of log rolling did not go on. But, hell, you didn't have to be a wizard to know that, if it wasn't going on that given time, it would. Today, if someone on the Virus Research Committee wants to get a grant, he just can't get it. The only way he can make an application is to resign from the committee.

Q: Dr. Rivers, were there set rules or criteria by which the Virus Research Committee judged grant applications?

Rivers: I don't know how it would be possible for the committee to have any set rules. The only rule that we actually had was in the manner of making the application. The applicant had to state clearly what he wanted to do, how he was going to do it, and what he hoped to get out of his work. He had to put all this down in sufficient detail so that members of the Virus Committee could follow it. He also had to include a meaningful budget. I don't see how we could have operated without such a procedure. In the committee, each man had to make up his own mind and no guide was given to him on how to do this. We assumed that, at the minimum, committee members would have this talent, and they did.

This doesn't mean that all the grants that we made turned out to be profitable or wise. For instance, about 1941 Dr. Harold Faber of Stanford University put in an application to study active and passive immunization of poliomyelitis. Well, the Virus Committee just wasn't smart enough to realize that it was premature to undertake studies of active immunization—we didn't know for sure at that time that there were even three types of poliovirus. I might add that we knew just as little about passive immunization; however, there was a strong feeling that Dr. Faber, an experienced investigator, might make a contribution. It makes no difference how good a researcher a man is; the fact is that not every piece of research a man undertakes necessarily turns out to be profitable. In my own experience, certainly more than half the research I did went down the drain. In fundamental research you sometimes have little more at your disposal than something that induces you to ask an intelligent question. Often much of that research is fruitless, and it's bound to be. When we gave money for research on fundamental problems, the committee could make mistakes and we did. The scientists who did the research made mistakes. This does not discredit the committee or the scientists.

Q: Dr. Rivers, what were the mechanics of approving or disapproving an application for a grant?

Rivers: In the beginning, research grants were made (and for that matter still are made) by the Board of Trustees of the Foundation, following a recommendation of approval by the medical advisory committees at the Foundation's semiannual meetings. Now, if a fellow came to the Foundation after one of the semiannual meetings and didn't want to wait six months to have his application considered, he could always ask the medical director to send his application and other pertinent material to the particular medical advisory committee concerned for an immediate mail vote. There were two differences between a vote taken when a committee was in semiannual session and a mail vote. At the semiannual meeting there would be discussion among committee members and a majority vote would be sufficient for approving a grant; but in a mail vote there was no discussion among committee members and the vote had to be unanimous before a grant was approved.

Generally speaking, the mail vote was very unpopular because it took only one guy to block approval, and in the Virus Research Committee you could always count on one son-of-a-gun who would be willing to toss a monkey wrench into the works. Finally we had one other way of making a grant. If a member of the medical department thought it wise to make a grant to a particular scientist whose work was promising and who was in a hurry for a decision, he could bring it before the medical director for discussion and ask that an administrative grant be made. If the director approved the application, it would then be sent to Mr. O'Connor for final approval or disapproval, and if he approved the grant would be made. These were merely ways of proceeding. I would say that in general most applicants waited for the semiannual meetings to submit their applications.

Q: Dr. Rivers, in a sense, approval or disapproval of an application can be considered a judgment of a man's work by a committee of his peers. Was the Virus Research Committee aware of the impact of disapproving a grant application?

Rivers: Please keep in mind that there were boundaries to the kind of work the Virus Research Committee could support. Sometimes, people were turned down not because their ideas or work were poor, but because what they wanted to do was on the fringe of the work that the Foundation was interested in, and less important than the work of another investigator whom the Foundation thought it ought to support. The committee always had to make that kind of choice. I want to reiterate that, if an applicant was turned down, it didn't mean that his work was necessarily scientifically unimportant; it just meant that it didn't fit in with the Foundation's research program. Of course, we also turned down applications that were no good at all. Now we never told a guy why we turned him down. You asked whether it was a judgment. Sure it was a judgment, but that kind of judgment also has a silver lining, at least I think so. It is true that the guy that gets turned down gets a kick in the pants; to the extent that he wonders why, it may do him some good.

Q: Dr. Rivers, I wonder if you will examine with me some of the applications that were turned down during the early years of the Foundation, so that I might form some notion of the research ideas that were discarded during that period.

Rivers: I take it you have some specific cases in mind?

Q: Yes. Dr. Rivers, among the many applications that came into the Foundation during its early years, were a series of applications from Dr. George Draper who at one time served in the Hospital of the Rockefeller Institute. Was Dr. Draper a virologist? [14]

Rivers: I knew Dr. Draper a long time, but I don't think that I would call him a virologist. Dr. Draper was a distinguished clinician and had a long experience dealing with polio from the clinical point of view. That experience began in 1912 when he was serving at the Rockefeller Hospital, and as I believe I mentioned earlier, together

[14] George Draper, Application for grant to The National Foundation for Infantile Paralysis, January 12, 1942 (CRBS #37, Columbia University, 1942, National Foundation Archives).

with Francis Peabody and Alphonse Dochez, wrote one of the best monographs dealing with the clinical aspects of polio that has ever been written. When Franklin Roosevelt returned to New York from Campobello Island stricken with polio, Dr. Draper became his physician and took care of him for a very long time. Later he wrote a textbook devoted to problems of polio, and in time became an associate professor of clinical medicine at the College of Physicians and Surgeons at Columbia. During the twenties—I don't know exactly when—Dr. Draper got the idea that people of a certain constitutional type were more susceptible to polio than others. Incidentally, he believed this of other diseases as well. He used to claim that he could look at a person and tell if he was likely to have duodenal ulcers. Incidentally, he once told me that I was the type that would never get a duodenal ulcer—I want to tell you that I later had the granddaddy of all duodenal ulcers.

Although I am poking fun at Dr. Draper now, please keep in mind that I mentioned the relation of constitution to susceptibility of polio as a possible research topic in the eleven-point program I presented to the Virus Research Committee. Actually it was an intriguing idea, and Dr. Draper could always get support to make such studies.[15] In 1935, for example, Dr. Flexner asked me if I would support an application from Dr. Draper to the President's Birthday Ball Commission for a research grant to study this problem, and I agreed. However I should add that I also told them not to give him the whopping sum he asked for. Later he got some modest support from the Milbank Fund to carry on his research, and when that gave out he came to the National Foundation.

If he had asked for a large sum, I don't think that he would have gotten to first base with the Virus Research Committee, but he asked for a very modest amount and we thought it worthwhile to take a flyer—not everybody—but a majority. Well, he set to work, to compare the buffy coats of bloods taken from patients who had polio with

[15] F. W. Peabody, G. Draper, and A. R. Dochez, *A Clinical Study of Acute Poliomyelitis*. Monographs of the Rockefeller Institute for Medical Research No. 4, New York, 1912. One of the earliest expressions of Draper's views on the significance of the human constitution to medicine was given in the Beaumont Foundation Lectures in 1928 (see also G. Draper, *The Human Constitution and Other Lectures*. Williams & Wilkins, Baltimore, 1928).

those of patients who had other diseases and set up control groups with persons of various sizes and shapes and racial extraction. He sent in progress reports with interesting pictures of various kinds of blood cells, but for the life of me I didn't know what it all meant, and for that matter neither did anybody else.

When he asked for a renewal of his grant, we turned him down. I want to tell you that all hell broke loose. He threatened to go here and he threatened to go there, he wrote to the newspapers and finally appealed to Mrs. Roosevelt. He had taken care of the President and he was an old family friend, and, as you might suspect, Mrs. Roosevelt listened sympathetically. She was a very wise woman and wrote a letter of inquiry to Mr. O'Connor asking for the facts in the case. It was nothing more than an inquiry. She didn't ask the Foundation to do anything, nor would she, because she was that kind of a lady, but the letter was a pressure. Well Mr. O'Connor just isn't the kind of a man you pressure, even indirectly. If you ask me, I think that it gets his back up a little. I do know that he sat down and wrote Mrs. Roosevelt a long letter explaining the facts in the Draper case, and that was that. Dr. Draper's application never came before the Virus Research Committee again.

Q: Dr. Rivers, another person who frequently asked the Foundation for support during the early years was Dr. John Toomey.[16]

Rivers: John Toomey was a professor of experimental pediatrics and contagious diseases at Western Reserve Medical School in Cleveland. Dr. Toomey was a good clinician and during the thirties was very active in the laboratory on problems of polio. He was full of ideas, and during the early years of the Foundation we supported him in any number of projects. He never asked for very great sums of money, but that wasn't the consideration; when he had a good project we were happy to support him. For instance, in 1940 he approached us for a modest grant to produce polio in lab animals other than the monkey. It was a good project for the time, and we were happy to give him the money.

[16] John Toomey, Applications for grants to The National Foundation for Infantile Paralysis, 1939–1942 (Grant Application file, John Toomey, 1939, 1940, 1941, National Foundation Archives).

The trouble with Dr. Toomey was that he thought it was mandatory for us to support all of his ideas, and the plain fact is that he had some that were not worth supporting. Toomey had a certain tune that he played for years and, while it was interesting in part, I and other members of the Virus Research Committee didn't think that it was necessary for the Foundation to sing along with him. Toomey, like John Paul and James Trask of Yale, recovered poliovirus from the stools of polio patients and in the sewage in epidemic areas. He wasn't dumb by any means, and, like several other investigators at the time, began to question the view that the nose was the portal of entry for the virus. He fixed his attention on the gastrointestinal tract. For some reason that I still can't understand, he assumed that some of the Gram-negative organisms which were found in the gut gave off a toxin which facilitated the passage of the poliovirus through the mucosal wall of the intestines to the gray sympathetic nerve fibers, and from there to the vagus nerve and eventually to the bulb of the brain. At one point Toomey wanted the Foundation to support him in a research program in which he would vaccinate monkeys with colon paratyphoid organisms, to see if they would protect monkeys against poliovirus given by way of the gastrointestinal tract. No one that I knew thought much of that project. At the time, the work that Howard Howe and David Bodian were doing on like problems seemed much more promising, and I should add here that it wasn't until Howe and Bodian published their classical experiments on the neural mechanisms in polio that we got a clear picture of how poliovirus invaded the central nervous system.

Dr. Toomey never had an experimental program the way Dr. Howe or Dr. Bodian did; it was helter skelter, like his ideas. I remember that the Virus Research Committee once got an application from him that began with a request to investigate whether vegetables or fruit carried poliovirus, ran to a project to determine whether polio could be produced in vitamin deficient rats, and ended with a series of experiments to determine whether you could immunize monkeys against polio with mouse virus. There were about nine different projects in all in the one application, and he got sore as hell when we indicated that we thought that only one or two were worth pursuing. Dr. Toomey, you can say, presented the problem of the clinician who worked in the lab but was not in any sense a full-time virus investigator. He had interest and ideas and is to be commended for those traits, but he really wasn't an experimenter, although he did do a hell of a lot of experiments.

The other side of the coin for the Virus Research Committee was the problem presented by full-time virus investigators who had projects which on the surface seemed worth pursuing, but which the committee in the end turned down because other virologists couldn't repeat the work they had reported, or because there was a mix-up in their results that was apparent to everybody but themselves. Now that happens to a lot of us—even the best of us. The fellow who gave us the most trouble along these lines during the early years of the Foundation was Dr. Claus Jungeblut.

Q: Dr. Rivers, could you give me a specific example of what you mean?

Rivers: I will give you two examples. Before I do, let me say that, at the time Dr. Jungeblut made his applications to the Virus Research Committee, he was a professor of bacteriology at the College of Physicians and Surgeons at Columbia and had had extensive experience in polio research. As a matter of fact, before he approached the Foundation for help, he had been supported by other foundations and had even received a grant from the President's Birthday Ball Commission, so he was not unknown to us.

Sometime around 1939 or 1940, Dr. Jungeblut obtained a strain of poliovirus from John Paul at Yale—the Yale SK strain—and began to play around with it in his lab. Soon afterward, he reported that he had adapted this human strain to mice and that he could pass it from mouse to mouse.[17] Well, that made us sit up, because in 1939 Charley Armstrong demonstrated that he was able to pass the Lansing type 2 polio to cotton rats, and Dr. Jungeblut's reports suggested that he had made a successful repetition of Armstrong's experiments and had, in fact, made a material extension of its experimental basis. That

[17] C. W. Jungeblut and M. Sanders, "Isolation of a murine neurotropic virus by passage of monkey poliomyelitis virus to cotton rats and white mice," *Proc. Soc. Exptl. Biol. Med.*, vol. 44:375 (1940).

work on its face promised a great deal. However, on closer examination, I became suspicious of it. For instance, I found that Jungeblut had actually passed the SK virus successfully to cotton rats only once, and that the neutralization tests he performed were too few to be significant. When other investigators could not successfully repeat his work, I suspected that he had inadvertently picked up a mouse virus and was confusing it with the SK strain he originally got from John Paul. You know, he could never see the flaw in these experiments, and I can tell you that he didn't like the fact that the Foundation turned him down.

Now, that particular work didn't bother me personally as much as his request for a grant to study the relationship of vitamin C and poliomyelitis. Jungeblut believed that vitamin C could protect against neurotropic agents of disease—this included the paralysis caused by poliovirus as well as the paralysis caused by diphtheria toxin or the paralysis of tetanus intoxication. In 1937 he published results which purported to show that, if you administered vitamin C during the incubation period of experimental polio, you could not only modify the course of the disease but that in some cases you could even prevent polio in monkeys.[18] Albert Sabin tried to repeat that work and couldn't. When Jungeblut later submitted that particular work as a

[18] C. W. Jungeblut, "Vitamin C therapy and prophylaxis in experimental poliomyelitis," *J. Exptl. Med.*, vol. 65:127 (1937).

Dr. Peter Olitsky adds this note to Dr. Rivers' remarks:

I regret very much that it fell to my laboratory to show different results from those obtained by Dr. Jungeblut and which are mentioned by Dr. Rivers here.

Such work as confirmation, or not, of results obtained by others is a horrid chore: it does not advance science very far: it takes time and uses up budgets and ends in bitter feelings. In this instance, Dr. Claus Jungeblut being an amiable and attractive person, one would have preferred to pass the buck to others. However, because of his repeated statements of his findings, we were compelled to clear the field.

The ineffectiveness of vitamin C for prevention or treatment of experimental poliomyelitis in monkeys was proved by Dr. Sabin. He used over 100 monkeys in this test. Although his findings were negative, some good did come out of his work, namely, a study of scurvy in monkeys, including symptoms, pathogenesis, and treatment.

The experiments on SK virus were carried out by Col. Yager and me. We demonstrated that the Jungeblut MM virus (also thought by him to be, like SK, a poliovirus), and mengo and encephalomycarditis viruses, all had similar and cross antigenic and antibody reactions as tested by hemagglutination and its inhibition. Thus the four agents could be regarded as four strains of the same virus. All were active in certain rodents which served also as reservoirs of the virus. An occasional human being was attacked by them. The important point is, of course, that neither the SK nor the MM were polioviruses (private communication).

basis for a grant, I had him turned down. I was willing to risk Foundation funds when there was a chance that it would advance our basic knowledge of poliovirus or the epidemiology of polio, but I'll be damned if I would put it on something where the door had already been closed.

Q: Dr. Rivers, your rejection of Dr. Jungeblut's work on vitamin C raises a deeper question, namely, your attitude toward work relating nutrition to viral infections in general. I would particularly like to pursue this point in relation to the short-lived Nutrition Committee in the Foundation.[19]

Rivers: Go ahead. I don't mind telling you what I know.

Q: Dr. Rivers, how did the Nutrition Committee originate?

Rivers: During the thirties Paul de Kruif was much influenced by the work of Dr. Tom Spies on vitamin B_1 and became convinced that undernourished or badly nourished children were more likely to be susceptible to polio than others. It was de Kruif who urged Mr. O'Connor to create a committee in the Foundation that would devote itself to an examination of the relationships between nutritional status and polio. If you ask me, I think that it would be fairer to say that the committee was formed to take care of Dr. Tom Spies. I know that sometime earlier Paul had approached the Annheuser Busch people for a grant on Dr. Spies's behalf, so that he could settle at Washington University in St. Louis, and when they turned him down Paul thought that the National Foundation should support Spies. I don't mind telling you that I fought the formation of the Nutrition Committee. I didn't give a damn about Spies, and I was firm in my belief that nutrition had little to do with susceptibility to polio or, for that matter, any other virus disease. I based this belief on what I had observed in army camps during World War I and what I had observed in the laboratory.

[19] The Nutrition Committee was organized on September 23, 1940, and disbanded May 15, 1941. Cf. Minutes of the Committee on Nutritional Research (folder, Organizational Meetings, 1941, National Foundation Archives).

For instance, during World War I the big strapping farm boys from the midwest and south were more susceptible to influenza virus—and a hell of a lot sicker—than the fellows who were born on the east side of New York or who came out of the slums of Chicago and never grew very big and never got enough to eat. Of course, what had happened was that the boys who came from the big cities had previously been exposed to all sorts of bacterial and viral infections and had built up resistance, while the farm boys, although initially healthy, had never been exposed and were, of course, more susceptible. Later I found the same thing to be true in the laboratory. When I first started to work with viruses, I discovered that sickly or undernourished rabbits were much less susceptible to vaccinia virus than healthy rabbits. Peyton Rous, for example, had even earlier noted that he had a great deal of difficulty passing the Rous sarcoma virus to unhealthy or sick chickens. Now this happens to be almost universally true so far as virus diseases are concerned. I will grant that an exception can be found here and there—there are always such exceptions—but I think you can see from what I have told you why I thought we didn't need a committee to study the relationship of nutrition to polio. However, since the Foundation was going to have one, I thought it would be best for me to be on it—as a watch dog.

Q: Who else was on the committee?

Rivers: Actually it was a very good committee. In addition to de Kruif, Spies and myself, James MacLester, Conrad Elvehjem and Robert R. Williams served on that committee. The latter members had much experience with problems of nutrition. Dr. MacLester, for example, was the author of a standard textbook on diseases of metabolism; Dr. Williams who was then the chief chemist of the Bell Telephone Labs, had discovered vitamin B_1 which, as you know, is the key chemical necessary to the integrity of the nervous system, and Dr. Elvehjem, as I indicated previously, had already made quite a reputation through his discovery of nicotinic acid as a preventative of pellagra in dogs. You might even say that it was an excellent committee for its purposes. Later Charles G. King, a chemist at the University of Pittsburg who was interested in nutrition, also joined the committee.

Q: Dr. Rivers did the committee devise any research program?

Rivers: No. Although we spoke a great deal about the necessity of creating such a program, nothing was ever done. Actually the committee only made three grants during its short existence. One went to Tom Spies at the University of Texas Medical School, another went to Conrad Elvehjem and Paul Clark at the University of Wisconsin, and a third went to Leslie Webster at the Rockefeller Institute.[20]

Q: Dr. Rivers, correspondence in the files of the Foundation indicates that you were not very consistent in your beliefs about nutrition and virus disease; because, so far as I can make out, although you opposed grants to Dr. Spies for such purposes, you supported grants to Dr. Elvehjem and Dr. Clark and actually initiated the grant to Dr. Webster.

Rivers: Perhaps I was inconsistent. I am not perfect, and I have never claimed to be right all of the time. I can tell you why I did what I did, and perhaps it will act as explanation, but I ain't promising that it will be one. First, if you read carefully, you will find that I supported the first grant that the Foundation made to Dr. Spies. I opposed a renewal of that grant and that's a horse of a different color. During the late thirties Dr. Spies proselytized a great deal on behalf of nutritional studies. At the time he was an associate professor of medicine at the University of Cincinnati Medical School and a professor at the special branch of the University of Texas Medical School maintained at Galveston. He was well thought of, and the Foundation gave him a grant to study the relationship between infectious disease and nutrition. It was a broad-gauged grant; as a matter of fact, nothing more than the relationship between infectious diseases and nutrition was specified in it. Dr. Spies, who had connections at the Hillman Hospital in Birmingham where he had done some previous nutritional research, decided that he would begin his research by

[20] *Ibid.*, September 23, 1940; November 8, 1940. See especially Thomas Rivers to Donald Gudakunst, July 14, 1941 (CRBS #2, University of Texas, 1940); Memorandum, Donald Gudakunst to Peter Cusack, November 11, 1940 (CRBS #44, University of Wisconsin, 1940, National Foundation Archives).

studying the relationship between staphylococcus and streptococcus infections and nutritional deficiencies. He did that for a year and never went near a virus disease, much less polio. He kept promising to apply what he was learning to polio but never did.

Now, I never minded broad-gauged grants if they in any way furthered our knowledge of virus disease, because I always felt that such knowledge could always be applied later to the study of polio. I supported Dr. Karl Meyer's work on western equine encephalitis for just such a reason. Dr. Spies's work, unfortunately, could not be so construed, and after a year it became apparent that his major concern was only with dietary deficiency and bacterial disease. I didn't mind flanking attacks, but a flanking attack has to have purpose. Dr. Spies's work had as much future usefulness to polio as doily making has to football. It was useful in itself, sure, but that's all.

Q: Were the grants made to Dr. Elvehjem and Dr. Webster any different?

Rivers: Yes, both in purpose and personnel. For example, the grant that was made to Dr. Elvehjem was essentially made to a team, one member of which had long and extensive experience with polio. I am speaking here of Dr. Paul Clark. Dr. Clark's work in polio began with Dr. Flexner at the Rockefeller Institute as far back as 1912. As a matter of fact Dr. Clark can be counted as one of the pioneer polio workers in this country. Also, at the time we made the grant, Dr. Elvehjem was one of the best damned nutrition chemists in the country. Even more important was the fact that Elvehjem and Clark had a program that was important for polio research. They wanted to study the influence of nutrition and its effects on metabolism as a factor in susceptibility to polio in monkeys and cotton rats. I can tell you now that the Foundation doesn't have to be ashamed of that grant. Dr. Elvehjem and Dr. Clark did very nice work. They not only worked out the nutritional requirements for the monkey, which was the chief experimental animal used at that time in polio research, but also demonstrated that cotton rats who were fed thiamine-deficient diets showed a lower incidence of infection to Lansing type 2 polio than those who had a sufficient thiamine diet. It was very useful work, and you might term it a building block in our knowledge of the nutrition of experimental animals used in polio research.[21]

Q: How about Dr. Webster's grant?

Rivers: First, let me say that it is true that I pushed Dr. Webster's grant in the Nutrition Committee; in fact it was I who urged Dr. Webster to make application to the Foundation in the first place. To be sure, I didn't think very much of working on problems that explored the relationship of nutrition and polio, but I did believe that it was necessary to develop an experimental program that was capable of giving us answers one way or another on this problem. I thought that Dr. Webster offered us the best hope for developing such a program. As I mentioned earlier, Dr. Webster had made giant strides in experimental epidemiology, using strains of mice that had been genetically bred for resistance and susceptibility to typhoid and encephalitis. Put another way he had at his disposal a group of laboratory animals, with a unique control of their inherited factors of resistance and susceptibility to both bacterial and viral infections. In addition to available standardized lab animals, Dr. Webster had the assistance of an excellent young nutritionist in the person of Dr. Howard Schneider and a team who had perfected the technique of reproducing infections as they occurred in nature. The purpose of the grant was likewise admirable—as I remember it, Dr. Webster wanted to assemble an experimental diet of purified materials, i.e., vitamin A, D, B_1, riboflavin, and so forth, and to test its effect on the resistance of several inbred strains of mice to both typhoid and encephalitis infections. I wasn't the only one who thought well of that project. It was unanimously supported by the committee, and a grant of $25,000 or more accurately $5,000 a year for five years was made.

I don't mind telling you that the grant made to Dr. Webster caused me a good deal of personal trouble and from a most unexpected source. When the first check for Dr. Webster was sent to the

[21] H. A. Waisman, A. F. Rasmussen, C. A. Elvehjem, and P. F. Clark, "Studies on the nutritional requirements of the rhesus monkey," *J. Nutrition*, vol. 26:205 (1943); A. F. Rasmussen, H. A. Waisman, C. A. Elvehjem, and P. F. Clark, "Inference of the level of thiamin intake on the susceptibility of mice to poliomyelitis virus," *J. Infect. Diseases*, vol. 71:41 (1944).

Rockefeller Institute by the National Foundation, Mr. John D. Rockefeller, Jr., called Dr. Gasser and myself at the Institute and told us that he wished to speak with us. We went down to see him at his office, and he very politely and very firmly spanked us. He made it clear that, as long as he had anything to do with the Institute, nobody would be allowed to put any money in the Institute, because he felt that it would impair the freedom of action that the Institute enjoyed. He then asked us to return the check to the Foundation. Well, that talk came as a shock and a surprise to both Dr. Gasser and me, because we just had no idea that Mr. Rockefeller felt that way. After the money was returned, the Board of Trustees of the Institute voted a like sum to Dr. Webster. So he didn't lose a thing.

Q: Dr. Rivers, although we have reviewed some of the work supported by the Nutrition Committee, you still have not explained the reasons for the demise of the committee.

Rivers: I don't know that there was any one reason for the abolition of the Nutrition Committee. So far as I remember, the trouble which led to its abolition began with consideration of an application made by Enrique Ecker of the Department of Pathology at Western Reserve Medical School. However, I think it is fair to say that if it hadn't been this application it would have been another, because by the time that Dr. Ecker's application came up for consideration, Mr. O'Connor had become disturbed by the operations of the Nutrition Committee. Mr. O'Connor is not a scientist, but he was perceptive enough to see that the committee had not devised a telling research program, and that there was some doubt among his scientific advisors whether nutrition was a good approach to problems of polio. On more than one occasion he mentioned to the committee that it was spending money too fast, and on several occasions I'll admit that I upset him by my criticisms of the nutrition work in progress. When I opposed the grant to Dr. Ecker, things came to a head.

I should say here that Dr. Ecker wanted to examine the changes which occurred in blood serum globulins and their immunological manifestations during the course of vitamin deficiencies. I believed then and I still believe, that Dr. Ecker was more concerned with the chemistry of complement than he was with problems of the relation of nutrition to polio. I opposed the grant. Now this didn't mean that I didn't think that it would be valuable to know about the chemistry of serum globulins. I just wasn't convinced that complement had anything to do with resistance to disease, or that the things that altered complement necessarily altered immunity. Besides I thought he was asking for too much money for what he wanted to do.

Q: Dr. Rivers, was the committee unanimous in its opposition to Dr. Ecker's application?

Rivers: No, definitely not. Dr. Ecker had substantial support in the committee. Paul de Kruif, Dr. Spies, Dr. King, and one or two others supported him. Some were neutral. I guess that I was the only one who opposed the grant outright. I'll tell you one thing, the committee didn't move hastily—that application was examined and reexamined. Dr. Gudakunst made several trips to talk with both Dr. Ecker and Dr. Howard Karsner who was then chairman of the Department of Pathology at Western Reserve Medical School. I suppose that if I hadn't been so stubborn Dr. Ecker would have gotten his grant, because at one point just about everybody on that committee had come around to support him. I persisted and finally, since Dr. Spies was also asking for a renewal of his original grant, the Nutrition Committee decided that it might be a good time to reexamine the general policy of the committee regarding the breadth of research to be undertaken by the Foundation in the field of nutrition. I don't now remember the exact date of that meeting but I remember the meeting—we had a hell of an argument, but when we got through I had convinced Dr. Williams and one or two others on the committee that the approach to problems of nutrition and polio was too damned broad to be meaningful.[22] A special committee composed of Ajax Carlson, Morris Fishbein, and Max Peet was appointed to go over Dr. Spies's previous work on the relation of nutrition to infectious disease and to consider his new grant application. By this time Paul de Kruif was good and sore, and when the special committee voted against a renewal of Dr. Spies's grant he resigned from the Foundation. Dr. Spies also re-

[22] Minutes, Committee on Nutritional Research, May 15, 1941.

signed, and a short time later the Nutrition Committee was dissolved. I didn't mind one bit, because nutrition was something I thought the Foundation should never have become involved with in the first place. I suppose that Paul de Kruif has never forgiven the Foundation for turning down Tom Spies.[23] I know that he has never forgiven me.

Q: Dr. Rivers, during the early years of the Foundation, was there ever any conflict between the Foundation and the U.S. Public Health Service on problems relating to research in polio?

Rivers: No. Actually the National Foundation and the Public Health Service cooperated with one another a great deal on such matters. Paul de Kruif, who was one of the powers in the Foundation during its early days, had a deep admiration for the medical research that went on under the auspices of the Public Health Service, particularly the work of Joseph Goldberger, George McCoy, and Charles Armstrong. He frequently held them up as models, and if you read his early works you will find that he wrote about them with much sympathy and understanding. I don't think that I am far from the mark when I say that it was through de Kruif that both Dr. McCoy and Dr. Armstrong came to serve on the Virus Research Committee. I would like to add that they served conscientiously and well, and during the early years Dr. Armstrong, in particular, could always be depended upon to give cogent advice on research problems.

Today, of course, the federal government supports medical research not only in the various divisions that go to make up the U.S. Public Health Service, but also in universities and medical schools through-

[23] Several years later de Kruif made a vigorous public defense of Spies's work on nutrition in his book *Life among the Doctors.* de Kruif presents the rejection by the National Foundation of Dr. Spies's application for a renewal of his research grant as an act of big committee men of organized medicine who, through prejudice and whim, did not understand or appreciate the ideas or implications of Spies's research. Nowhere, however, does de Kruif speak of the substance of the scientific debate attendant upon the rejection of Spies's application, or of the problems of the relation of nutrition and poliomyelitis. The weight of evidence in the Minutes of the Committee on Nutritional Research of The National Foundation for Infantile Paralysis and the correspondence in the grant application file of Dr. Spies do not support de Kruif's charges. See further P. de Kruif, *Life among the Doctors,* Harcourt Brace, New York, 1942, pp. 50–137; Folder, CRBS #2, University of Texas, 1941, National Foundation Archives; Minutes of the Committee on Nutritional Research, September 23, 1940; November 8, 1940; January 28, 1941; May 15, 1941, National Foundation Archives.

out the country. Last year, I believe that close to half a billion dollars was spent for just such purposes. That's quite a sum, and if you had told me in 1940 that this was going to happen I don't think that I would have believed you. For instance, in that year there was only one National Institute of Health in the U.S. Public Health Service, and the research funds allotted to it by the government were negligible. I don't remember what the exact sum was, but I can tell you that it wasn't very much. Research was not as admired as it is today.

You ask if there was any conflict between the National Foundation and the U.S. Public Health Service on problems relating to research in polio? I never saw any. It's been a long time, and I am sure that a lot of folks have forgotten, but, in 1939 and for several years thereafter, the National Foundation helped support some of the pathological research in polio that went on in the National Institute of Health. As I mentioned earlier, the first point of the eleven-point research program established by the Foundation was an examination of the pathology of polio in humans, and, when Dr. Ralph D. Lillie, who was then chief of the Division of Pathology of the National Institute of Health, asked for a grant to support a study in the pathology of polio, the Virus Research Committee gave it a good deal of attention. Dr. Lillie at that time was engaged in making detailed topographic studies of the distribution of polio lesions in the brains of human victims and wanted to do comparative studies of material taken from epidemics occurring in different geographic localities. As I remember, he also wanted to do some cytological and histological studies. It was a good program for the time, and the committee approved his grant. I believe that initially we gave him $10,000. Making the grant was easy enough but giving the money was hard as hell, because the federal government did not readily accept such funds. I don't know how many special governmental advisory committees had to approve this particular grant but in the end it was approved. Later the grant was renewed; however, the actual work was interrupted by the war and was not completed until 1947. To my knowledge, this is one of the few instances that I know of where a private voluntary health agency supported the research activities of the U.S. Public Health Service. I doubt very much whether that same thing could happen today.

CHAPTER 9

NAMRU 2—A Virologist at War

. . . The number of seamen in time of war who die by shipwreck, capture, famine, fire, or sword are but inconsiderable in respect of such as are destroyed by the ship diseases and by the usual maladies of intemperate climates.

Dr. James Lind, *An Essay on the Most Effectual Means of Preserving the Health of Seamen in the Royal Navy,* 1779

Q: Dr. Rivers, did the Rockefeller Hospital make any special preparations in anticipation of World War II?

Rivers: When I first came to the Rockefeller Hospital in 1922, I heard many stories of how World War I had disrupted the hospital, and I was determined that if another war did come, the hospital's work would not be curtailed because much important research was under way.[1] You know in 1939 not everybody was of the opinion that

[1] Peter Olitsky presents this portrait of the operations of the Rockefeller Institute during World War I.

In 1917 the Rockefeller Institute was militarized and became U.S. Army Auxiliary *Laboratory No. 1, and the members of its staff secured commissions as officers of the U.S. Army Reserves under the Commanding Officer, Colonel Simon Flexner. There were two exceptions, Drs. Avery and Kligler (apart from German citizens for whom special rules were made by Washington to retain them). Dr. Avery was a Canadian and hence could only serve as private; he later was commissioned as officer. Dr. Kligler was also a private for reasons unknown to me; he later became a sergeant. An adjutant of the army was detailed to the Institute to take care of the Army paper work and commanded the technicians and helpers, maintained an Army discipline among them, and drilled them at parades, etc., on York Avenue, with our lay neighbors looking on, I hope, with pride. A mobile, complete hospital unit (on wheels) consisting of several small buildings, wards, laboratories, laundry, kitchen, etc., was rolled into the front yard*