CHAPTER 10

The Public Health Research Institute of New York

. . . Bringing home to public opinion the fact that the community can buy its own health protection is laid upon all health officers, organizations, and individuals interested in public health movements. For the provision of more and better facilities for the protection of public health must come in the last analysis through the education of public opinion so that the community shall vividly realize both its needs and its powers.

Dr. Hermann Biggs, *Monthly Bulletin of the New York City Department of Health,* October 1911

Q: Dr. Rivers, since you were associated with the founding of the Public Health Research Institute of New York, I wonder if you could tell me something about the origins of this unique institution.

Rivers: The Public Health Research Institute of New York was founded during the summer of 1941 but if you are looking for origins I think that you can go way back to 1893 when Dr. Hermann Biggs, then the commissioner of health for the City of New York, asked his associate, Dr. William H. Park of the laboratories of the New York City Health Department, to do research on diphtheria. Less than a year after receiving these instructions, Dr. Park developed a laboratory method for the diagnosis of diphtheria, a method which I am happy to say has today become standard procedure throughout the world.[1] It is said that Dr. Park's success convinced the authorities of

[1] In one sense, Dr. Rivers here has given too much credit to Dr. Park. The credit for the test for the diagnosis of immunity in diphtheria should go to Dr. Bela Schick,

the City of New York that much was to be gained if the laboratories of the Health Department continued to do research. If they had such convictions they gave little evidence of it because Dr. Park as head of these laboratories received little or no funds from the city to develop a research program. In 1898, in addition to his job as director of the Department of Health laboratories, Dr. Park was appointed professor of bacteriology at the New York University Medical School. That was an event of some importance because it gave Dr. Park the opportunity to acquire research funds from the University, and what Dr. Park got with his right hand from the University, he dispensed with his left hand to the laboratories of the Health Department. Having two such eminent positions also gave him the opportunity to recruit researchers, and he would attract people by offering them posts as part-time instructors in the Department of Bacteriology as well as jobs in the laboratories of the Health Department.

Although Dr. Park personally did not need any salary—he came from the Park-Tilford family and had money of his own—he drew two salaries and utilized a sizable proportion of it for research programs that he was interested in. Now, drawing two salaries was contrary to the laws of the City of New York, and, although every politician in the city knew what Dr. Park was doing, they winked at it. Nobody said boo. Everybody recognized his outstanding ability and since he wanted to do research so badly they let him alone. Why shouldn't they? It didn't cost them a thing.[2]

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who in 1913 reported that a simple skin test employing a small amount of diphtheria toxin could demonstrate whether a subject had immunity or whether he needed artificial immunization. Dr. Park and his associates should be credited with preparation of diphtheria antitoxin and its use on a mass basis for treatment, and with studies proving the value of these procedures for large populations. See further: C. E. A. Winslow, *The Life of Hermann Biggs*. Lea and Febiger, Philadelphia, 1929, pp. 107–130; W. W. Oliver, *The Man who Lived for Tomorrow*. Dutton, New York, 1941, pp. 82–125; 161–189.

[2] Dr. Peter Olitsky, who early in his career worked in the laboratories of the New York City Health Department, gives this portrait of Dr. Park:

Dr. Park was so busy with extramural activities, especially the Board of Aldermen and New York University, and with administrative affairs of the laboratory, that he was generally inaccessible to laboratory personnel. In the year I was there (1912–1913) I saw him only once, on my first visit, when he took me on as a volunteer but full-time worker. His basic problem was to "sell" to members of the Board of Aldermen the value of medical, especially microbiological, research and public health activities. Generally, the trend of the board was to regard research as an unnecessary luxury; more often, In summary I think that it's fair to say that the laboratories of the New York City Health Department, during their early history, were able to engage in research because Dr. Park successfully combined the resources of the city and the resources of New York University. The former supplied quarters and laboratories, while the latter provided scientific personnel and finances. It was a very convenient marriage but it was no way to carry on sustained scientific research, and as time went on the pressures from both these institutions on Dr. Park became greater and greater. By 1930, for example, bacteriology had become so specialized a discipline that departmental duties which Dr. Park had earlier carried on a part-time basis now required his full-time attention. At the same time, the daily activities of the laboratories of the Health Department had proliferated to such an extent that they too required the full attention of Dr. Park. To add to these burdens the people who had previously shouldered the direction of research in the laboratories with Dr. Park began to die off or retire. In 1930 Dr. Charles Krumwiede, who for years was a stalwart in the research programs of the laboratories died of a malignancy. In 1936 Dr. Anna Williams, who was one of the pioneer researchers in this country on

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the city fathers did not always grasp what Dr. Park was talking about. In this regard a story went the rounds of the laboratory. A proposal was once made to the Board of Aldermen to acquire more gondolas to adorn the city's Central Park Lake. "More gondolas," roared an alderman, "Why not get a male gondola and a female gondola and I warrant within a year you will have more gondolas than you can use!"

In the end Dr. Park was compelled to sell "biologicals" (vaccines, culture media, etc.) to physicians to pay for part of his expenses. Laboratory gossip had it that Park, apart from sharing his two salaries with some of his group, dug into his jeans for his own money to pay salaries and expenses at the laboratory. It was a condition that led to great hardships for some. One of these was Abraham Zingher. Dr. Zingher was a quiet, thinking man, admirable, kind, generous, helpful, and a veritable Hercules for work. He probably contributed most of the labor of Park's famous, pioneering, successful diphtheria experiments. He was always busy always going somewhere, never relaxing or chatting or gossiping. His salaries from Dr. Park were not enough for the support of his family, so he was forced to practice medicine privately. Thus for seven days a week, morning, noon, and night, Zingher could be seen with an old, worn portfolio case which held papers on which were the names of children down with the choking, paralyzing, crippling diphtheria—those that he treated with antitoxin, those that were being or had been immunized, and those Schick-tested—and notes for epidemiological study, all of which was the meat for the series of papers to be published by W. H. Park and A. Zingher.

This humble and inspired man broke under the strain. . . . On a Sunday morning he came to the laboratory, sat down at a work table, laid his head on it, turned on the Bunsen burner gas, and thus made new problems for his family but solved his own (private communication).

problems of rabies and streptococcus infection and coauthor with Dr. Park of a textbook on bacteriology, retired. By the mid thirties there was no one in the laboratories of the Health Department who could shoulder the burden of directing research and the pressure of events was such that the problem could no longer even be evaded.

Let me give you an example of what I mean. In 1936 Dr. William Thalhimer of Mt. Sinai Hospital wanted to establish a convalescent serum division in the laboratories of the Health Department to investigate the use of such serums in the prophylaxis and therapy of measles and scarlet fever, and to study the comparative usefulness of human serum and citrated plasma as blood substitutes. It was an excellent plan but unfortunately the Department of Health was in no position to support such work. If it had been anybody else, the idea would have died there; but not Dr. Thalhimer—he just turned around and convinced several foundations to give him the money he needed. Foundations have always been reluctant to give money to individuals and to help Dr. Thalhimer administer these funds and to raise other funds for related research a number of us in the city got together and organized a nonprofit corporation called the Health Research Fund. If I remember correctly, Dr. Park, Dr. John Torrey, Dr. Currier McEwen, Dr. Michael Heidelberger, and myself served as the first board of directors. This occurred sometime in April 1936. To be sure it was a step forward, but I must add that it was a stopgap, because the Health Research Fund only had a limited amount of money, certainly not enough to support a full-time staff of scientists and not enough to carry on sustained research.

In 1937 Dr. Park retired as director of the laboratories, and the city began to scurry around for a replacement. It was a complicated matter: not that there weren't good candidates for the job, there were. It just so happened that the director of the laboratories was a civil service appointment and in order to qualify candidates were required to pass a special examination. I can tell you that that examination was pretty stiff—it was no cinch. I know, because I was on the examining board. I don't remember now how many passed or failed—it really doesn't matter because in the end the job went to the fellow with the highest grade and he turned out to be Dr. Ralph Muckenfuss. At the time Dr. Muckenfuss was an assistant professor of bacteriology at the Washington University Medical School in St. Louis and, as I mentioned before, earlier in his career had worked with both Dr. Jacques Bronfenbrenner and myself at the Rockefeller Institute. Dr. Muckenfuss was admirably qualified for the job, both as a bacteriologist and as a virologist, and while he got the job as director of the laboratories he did not get Dr. Park's other post as professor of bacteriology at the New York University Medical School. As a result, Ralph had no picnic, because while the city gave him enough money to carry on routine tasks at the laboratories they did not give him enough to carry on a sustained research program. And as far as I remember, nobody outside the city broke their backs to give him such funds.

On more than one occasion I spoke about this problem to David Heyman who was president of the New York Foundation. I knew Dave well, since we had both joined the Board of Health about the same time in 1937. Although Dave was no scientist, he was a kindred soul, and, without saying too much to me, he put the problem of doing research up to Dr. George Baehr. Dr. Baehr at the time was probably one of the best clinicians in the City of New York. He worked at the Mt. Sinai Hospital, but more important from the point of view of the story I am telling you, he was also Mayor Fiorello La Guardia's private physician.

Let me say that on problems of health Mayor La Guardia would do what George Baehr told him to do, and when George began to speak about how necessary it was for the City of New York to support medical research the mayor went along. Actually some weeks after Dr. Baehr spoke with him, the mayor appointed a committee composed of David Heyman, Dr. Ralph Muckenfuss, Dr. John Rice and myself—there may have been one or two others—to study the problem of city-supported medical research. We all felt that the Board of Estimate would never consistently vote sufficient funds to the laboratories of the Department of Health for research—it hadn't in the past—and that instead of trying to break a habit of not doing anything it might be better to create a new organization whose sole purpose would be to do medical research. In a memorandum we later prepared for Mayor La Guardia, we urged that the Health Research Fund, which was organized in 1936, be reorganized as a private research institute with the privilege of entering into contractual relations with the city for support of specific medical research programs. Mayor La Guardia accepted our proposals, and in June 1941 the Public Health Research Institute of New York was incorporated. About a month later, the mayor drew up a contract whereby the City of New York agreed to provide facilities for the new Institute at the laboratories of the Department of Health and to pay it a stipend of $100,000 a year for ten years in return for research into various problems of public health.

Q: Dr. Rivers, I am curious to know how you persuaded Mayor La Guardia to give the Public Health Research Institute $100,000 a year when the Board of Estimate during those years was generally speaking niggardly in providing funds for the laboratories of the Department of Health?

Rivers: I think that Mayor La Guardia outsmarted himself in this matter. Somehow he had learned that the laboratories were doing premarital Wassermanns and not charging anything for them. Since anyone who got married in the City of New York had to have such a test he figured he could pick up a $100,000 with ease by just charging a dollar for each test. I remember his telling us, "I don't have to go to the till to get your money, I can get it this way," and he signed the contract without a qualm.

When the Public Health Service in Washington heard about Mayor La Guardia's plan to charge a dollar for premarital Wassermanns all hell broke loose. They called him and told him, "Look, you can't charge anything for mandatory Wassermanns. If you do, we'll see to it that federal health funds are withheld from the City of New York." I want to tell you, they didn't have to call twice, because those funds totaled a hell of a lot more than the $100,000 that La Guardia hoped to raise by charging for premarital Wassermanns, and he quickly rescinded the order. In the end, La Guardia did have to go to the till; actually he had no choice in the matter, because by that time he had already signed a ten-year contract with the Public Health Research Institute. To La Guardia's credit, he never balked at fulfilling the terms of the contract. As a matter of fact, in 1945 he raised the annual stipend to the Institute from $100,000 to $200,000.

Q: Dr. Rivers, how was the Public Health Research Institute administered?

Rivers: Generally speaking, the responsibility for running the Institute fell on a Board of Directors and a Research Council. The first body was made up of prominent lay people and city officials, including the mayor, the comptroller, the commissioner of health and the commissioner of hospitals. I would say that the lay people were chosen for their business or legal ability, since the major job of the Board of Directors was to look after the financial and legal affairs of the Institute. At the very beginning David Heyman of the New York Foundation, and Edward F. Chinlund who was the vice president and treasurer of R. H. Macy and Company, served on the board as lay members. In time, they were joined by people of the caliber of Winthrop Rockefeller of Socony Vacuum, Rear Admiral Lewis H. Strauss, and Albert Simmonds, Jr., of the Bank of New York. The Research Council, on the other hand, was exclusively composed of scientists. In many respects it was very much like the Board of Scientific Directors of the Rockefeller Institute. Its major activity was to oversee and examine the progress of research at the Institute and to approve the hiring of scientific personnel. Initially, the Research Council was composed of Dr. George Baehr, Dr. Michael Heidelberger, Dr. Eugene Opie, Dr. Henry C. Sherman, and myself. Later Dr. Stanhope Bayne-Jones, Dr. Vincent DuVigneaud, Dr. Warren Weaver, and Dr. John Steffen joined the Council. I mention these names not because they were the most important, but because they happened just now to come to mind.

Q: Dr. Rivers, who directed the Institute in its day-to-day affairs?

Rivers: In the very beginning, Dr. Muckenfuss undertook the obligation of directing the Institute, since it was physically located in the laboratories of the Department of Health and was still quite small.

He continued in this post until he entered the service in 1942. At that time, instead of hiring a replacement, we asked Dr. Opie, who was then a member of the Research Council, to assume the burden of directing the Institute. He agreed. I don't know whether he really liked the job or not but he continued to serve until Dr. Muckenfuss returned from the army in 1946.

After the war we all expected Dr. Muckenfuss to pick up the reins again, and he did. Unfortunately, however, he did not long continue in the job. About a year after he returned, an administrative hassle began in the Institute. The root of the trouble lay in the delegation of administrative authority and the adherence of the scientists in the Institute to agreed administrative policies and procedures. In plain words, some of the boys began to go their own way on matters like making grant applications, purchasing supplies, and hiring technical personnel—not important things, but matters which were capable of giving an administrator ulcers. Dr. Muckenfuss at that time was still the director of the laboratories of the Department of Health, which was a full-time job. Administering the Institute was a part-time job he assumed for the Research Council. When administering the Institute began to give evidence that it had become an onerous burden for Dr. Muckenfuss, the Council decided that the time had come to seek a full-time director. Actually by 1947 the Institute had grown to the point where it required the attention of a full-time director.

At the suggestion of Dr. Michael Heidelberger, the Council decided to offer the directorship of the Institute to Dr. Walter Palmer, who at that time was chairman of the Department of Medicine at the College of Physicians and Surgeons of Columbia University. As I remember, there were no prolonged negotiations. We made the offer and he accepted. I might add that we were most fortunate that he could come. If we had made the offer several years before, I doubt very seriously if we could have moved him. When we approached him, Dr. Palmer was about to retire and, like most professors, didn't have much money. Even more important, I think that he was happy for the opportunity to continue in harness. I don't think that we have learned yet the wisdom of how to retire people. A guy can work vigorously and actively in his job to the age of 65 or 68, and the day after his birthday he will be told he is through, and yet he is no different from the day before. What the hell, Columbia's loss was our gain.

Q: Dr. Rivers, the first director of the Public Health Research Institute, Dr. Muckenfuss, was a working virologist. Dr. Opie, his replacement during the war, although an older man, was still active as an investigator. Why did the Research Council appoint a director who was a clinician?

Rivers: It's true that Walter Palmer was a clinician, although you might have added that he was an excellent clinician. You know, being a clinician does not mean that you can't understand or appreciate research. I think that if you examined Dr. Palmer's department at Columbia, you would find that he encouraged his young men to do research. As a matter of fact, clinical research was one of the hallmarks of that department and they did excellent work, Dr. Robert Loeb for one, Dr. Dana Atchley for another, and there were others, believe me.

You must keep in mind that we didn't hire Walter Palmer as an investigator; we hired him as an administrator. There was never any question in our minds that Dr. Palmer might not understand or appreciate the problems connected with doing research. We knew him to have such understanding in abundance. Besides, the divisional chiefs of the Public Health Research Institute did not need anyone to direct their research. They had plenty of ideas. The big problem was to keep them out of each other's hair and to administer the Institute in its day to day affairs. Dr. Palmer proved to be a superb administrator, he was just what we were looking for. The great pity was that, a little less than three years after he took the job, he suddenly died. I usually don't put great store by what is said about a man after he dies, but I do think that if you want to get at the substance of what Walter Palmer did for and meant to the Public Health Research Institute, it might be a good idea for me to read to you the resolution passed by the Research Council of the Institute in his memory.

Walter W. Palmer, Director of the Public Health Research Institute of the City of New York, Inc., Bard Professor of Medicine Emeritus of the

College of Physicians and Surgeons of Columbia University, and Consultant to the Presbyterian Hospital, died suddenly on his farm at Tyringham, Massachusetts, on October 28th, 1950.

Dr. Palmer, affectionately known as 'Bill' to a vast number of friends and colleagues despite the above resounding titles, was at the height of his powers and was keenly enjoying his work and his success as Director of the Public Health Research Institute. His retirement at Columbia had occasioned lively competition for the benefit of his calm, unruffled sagacity, his remarkable executive capacities, and his gift of leadership. The Public Health Research Institute was fortunate in persuading him that it had a mission to fulfill in research in preventive medicine and that his guidance was needed. He arrived at a time of internal upheaval and of uncertainties as to the future of the Institute. An air of calm descended upon the Institute almost at once, conflicts were quietly resolved, and the position of the Institute in relation to health problems became firmly established. The work of the Institute expanded in the same atmosphere of steady, contented effort that characterized Dr. Palmer's Department of Medicine at the College of Physicians and Surgeons.

In the painfully inadequate words of this minute, the Research Council of the Public Health Research Institute wishes to express its appreciation of and gratitude for the great benefits which Dr. Palmer's directorship, brief as it was, brought to the Institute; to record the deep personal sense of loss its members feel at the passing of their friend, and to express to Dr. Palmer's family their heartfelt and understanding sympathy.[3]

Q: Dr. Rivers, did you have much trouble finding a new director for the Institute?

Rivers: No. Soon after Dr. Palmer's death, the Research Council appointed a committee composed of Dr. Stanhope Bayne-Jones, Dr. John Kidd and Dr. George Baehr to seek a new director. That they had success in their task I think was in large measure due to Mr. Roger Elliott, the business manager of the Institute. I should explain that Mr. Elliott took over the chore of directing the Institute on an interim basis after Dr. Palmer's death and did such a good job that it took pressure off the committee to find an immediate successor. The committee, I believe, spent the better part of seven months surveying candidates and finally recommended Dr. L. Whittington Gorham for the job. Dr. Gorham was personally well known to the members of

[3] Minutes, Research Council, Public Health Research Institute of New York, December 1, 1950.

the committee. He was a graduate of the Hopkins and a professor of medicine at the Albany Medical College. He too was on the edge of retirement and, like Dr. Palmer was much interested in research. As a matter of fact throughout his career he had done considerable clinical research on cardiac problems. I ought to stress, however, that we hired him as an administrator, not an investigator. Although Dr. Gorham was not as forceful as Dr. Palmer, he too proved to be the right man for the job and kept things on an even keel at the Institute until his retirement in 1956.

When Dr. Gorham left the Institute, the Research Council offered the directorship to Dr. George Hirst. Now, that offer broke almost a decade of precedence. For one thing, Dr. Hirst at the time was still a relatively young man, and secondly, he was still active in the laboratory as a virologist. As a matter of fact, when he was offered the job, he was just entering his ninth year as chief of the Division of Infectious Diseases at the Institute. I will speak of Dr. Hirst at some length later; for now, let me say that, since he took over as director, he has been doing a bang-up job of running the Institute. So, if you are looking for a general rule about the usefulness of clinicians and investigators at directing a research institute, I guess what I have told you doesn't help you very much.

Q: Dr. Rivers, I would like to turn now to an examination of the scientists who carried on research at the Public Health Research Institute during its early days, and to learn something about the nature of their work. I think that you will agree with me that one of the most interesting scientists who was employed by the Institute during its early years was Dr. Jules Freund. Could you tell me how he came to the Institute?

Rivers: It's an involved story. Dr. Freund was a Hungarian by birth and received most of his medical training at the University of Budapest. I say most, because after World War I he embarked on a course of postgraduate work which took him first to Vienna and later to Hamburg, and finally, during the late twenties, to the United States where he took a post as a bacteriologist in the Von Ruch Laboratories in Asheville, North Carolina. About 1928, he went to the Phipps Institute at the University of Pennsylvania, where he stayed for several years. When I first came to know him—in 1937 or 1938—he was a member of the Department of Bacteriology at the Cornell Medical School and assistant director of the Department of Health of the City of New York. During those years, Dr. Freund worked very closely with Dr. Eugene Opie on problems of tuberculosis and, as a matter of fact, it was through Dr. Opie that I first came to know him.

He was an odd character. For example, although he lived in the United States for many years, he never learned to speak English very well. But I must say that this particular deficiency never interfered with his career as an investigator. He was as fine a bacteriologist and immunologist as I have ever met. When the question of obtaining a chief for the Division of Immunology at the Public Health Research Institute came up in 1942, Dr. Opie, who was a member of the Research Council, pushed Dr. Freund for the post. He certainly rated it. If he had any problem at all, it lay in the fact that, while he himself was a hard and devoted worker, he had great difficulty in communicating with the people who worked with him.

I would say that one of Dr. Freund's most important contributions while at the Public Health Research Institute came in the field of applied immunology or, put more concretely, in the development of a technique for preparing vaccines by the use of adjuvants. I should perhaps explain that without adjuvants the immunity in many immunized persons is not high enough to prevent disease, and because of the short duration of immunity the immunizing injections have to be repeated. What Jules did was to demonstrate that a high level of immunity with greatly prolonged duration could be achieved if vaccines were incorporated in an emulsion of oil. Perhaps the first important success in the use of this new technique came in the laboratory during World War II. At that time, the consensus among experts who worked in the field of malaria was that immunization against malaria was impossible. I remember that Dr. Freund made everybody sit up when he succeeded in immunizing monkeys against malaria, using a killed vaccine and adjuvants. That was one of the early demonstrations of the usefulness of adjuvants. However, Dr. Freund's first great practical success came when he showed that penicillin was more effective when it was administered in an emulsion of oil. I say practical success, because, when the results of this research were first published, the Department of Health of the City of New York began using penicillin prepared in this manner for the treatment of patients with gonorrhea and syphilis in all their clinics. When it became apparent that days and weeks were cut from therapy through the use of such specially prepared penicillin, drug companies began using this principle in the commercial preparation of penicillin, a procedure which they follow to this day.

Quite apart from these practical results, Dr. Freund's principle of adjuvants worked a revolution in biological research. It was, for example, Dr. Freund's adjuvant which helped revolutionize the experimental approach to allergic encephalomyelitis. I think I mentioned earlier that, when I first began to work on this problem, it took several months before I was able to produce a demyelinating encephalitis in monkeys by feeding them normal rabbit brain tissue. Several years later, when Dr. Kabat and Dr. Wolf at Columbia incorporated Freund's adjuvant in monkey brain tissue, they were able to produce an allergic encephalomyelitis in their experimental animals in a matter of days.

Another area where Freund's adjuvant proved to be extraordinarily important was in polio research. After World War II one of the great needs in polio research was an in vitro test for polio. In 1950 Dr. Freund, with the aid of Dr. Robert Ward of the Department of Pediatrics of New York University Medical School, provided such a test by adding killed *Mycobacterium butyricum* to virus in water in oil emulsion as an immunizing medium, a procedure which resulted in the production in monkeys of sera with unusually high neutralizing antibody titers.[4] The moment those results were publicized the method was utilized by Dr. Jonas Salk, who at that time was engaged in typing various strains of poliovirus. It was a great help. Dr. Freund continued to work for the Public Health Research Institute until his retirement in 1954. Subsequently he took a job as consultant to the National Institutes of Health on problems of applied immunology. I understand that he continued active in his work until his death al-

[4] R. Ward, D. Rader, M. M. Lipton, and J. Freund, "Formation of neutralizing antibody in monkeys injected with poliomyelitis virus and adjuvants," *Proc. Soc. Exptl. Biol. Med.*, vol. 74:536 (1950).

most a year ago. I can't tell you any more, because in later years I lost contact with him.

Q: Dr. Rivers, did you find it difficult to recruit scientific workers of the caliber of Dr. Freund for the Public Health Research Institute?

Rivers: During the very early years of the Institute, as a member of the Research Council I took a great deal of personal responsibility in hiring scientists, and I can honestly say that I did not have too great difficulty getting the people I wanted. Recruiting scientists for the Division of Nutrition is, I think, a case in point. During the late thirties there was a great deal of interest in the relationship of nutrition to disease, and many imaginative investigators were to be found in the field. I have previously mentioned people like Conrad Elvehjem and Robert Williams but there were others, among them Professor Charles G. King at the University of Pittsburgh. Professor King had served with me on the Nutrition Committee at The National Foundation for Infantile Paralysis and, while I fought with him on issues that came before that committee, I nevertheless was impressed with his ability as a chemist. When we began to look around in 1941 for someone to head the Division of Nutrition, I offered Dr. King the post. He thanked me for it but indicated that he really didn't want to leave his professorship at Pittsburgh and instead urged me to go after Dr. Otto Bessey. Dr. Bessey was a biochemist who at that time was working at Harvard with Burt Wolbach on the pathology of deficiency diseases, and with Baird Hastings on the chemistry of nutrition. He had done some nice work and had the respect of his colleagues, and after meeting with him several times I offered him the job. Dr. Bessey wasn't the only candidate for that job. Actually a number of older people had also applied, some of them with very impressive credentials. But in the end I decided to go with Dr. Bessey, because he was still a relatively young man and, as I say, had promise.

As chief of the division, Dr. Bessey had the privilege of recommending and bringing in his own assistants, and he made the happy suggestion that we go after Oliver Lowry. I had heard of Dr. Lowry before and had been told that he was a most unusual young man. Initially he had taken a Ph.D. in biochemistry at the University of

Chicago, and a year or two later took an M.D. at the Rush Medical College. However, he never took an interneship. At the time Dr. Bessey spoke to me, Dr. Lowry was working in the Department of Biochemistry at Harvard under Baird Hastings. When Baird learned that I was after Lowry, he put up a hell of a fight to keep him. Luckily I was able to outtalk Baird or maybe I had a little more money to offer, and in the end Lowry came to the Institute.

Q: Were Dr. Bessey and Dr. Lowry the only men in the Division of Nutrition?

Rivers: In the beginning, yes, but a year or so later in 1943, I hired a young Danish biochemist just out of the University of Copenhagen, named Herman Kalckar. I do not believe that Dr. Kalckar, during the time he was at the Institute, did any independent work outside of the work that was then going on in the department, but he too was an extraordinary young man. Unfortunately he did not stay very long at the Institute. I believe that he wanted to stay, but after the war his young wife set her mind on returning to Denmark, and in 1946 he went back to Copenhagen. Some years later, I believe it was 1953, he returned to the United States and took a job with the National Institutes of Health. I want to tell you that this time he quickly began to show the stuff he was made of. Dr. Kalckar is the man who discovered the cause of galactosemia. Galactosemia, as you know, is a hereditary disease in children caused by the absence of a gene, which leads to the malfunctioning of the enzyme galactose-1-phosphate uridyl transferase, which is necessary in the conversion of galactose to glucose. Without this enzyme, children can't metabolize galactose properly and, being unable to do so, live a miserable life. If they live long enough, they get cirrhosis and cataracts and, worst of all, become mentally retarded. All you have to do to cure these children, or see that they don't become idiots, is to take them off milk or any other food that has galactose in it, and feed them a proper diet. A number of such diets have been worked out, and, if they stick to it long enough, children eventually learn to tolerate galactose and lead perfectly normal lives. Dr. Kalckar is the man responsible for a good deal of our understanding of the problem of galactosemia. Today he no longer works with NIH, and I understand that he has a professorship at Johns Hopkins, but I always like to remember that he began his career at the Public Health Research Institute.

Q: Dr. Rivers, I wonder if you could tell me now something about the work which the Division of Nutrition engaged in during the early years of the Institute.

Rivers: When the Public Health Research Institute got under way in 1941, there was substantial agreement within the medical profession that a studied application of the science of nutrition to problems in the diagnosis and treatment of disease would be beneficial. There was no lack of interest in nutrition; yet, in spite of interest, there was little progress in nutritional research, simply because there were no adequate and practical methods for measuring the nutritional condition of individuals and large groups. Both Dr. Bessey and Dr. Lowry recognized this deficiency, and during their early tenure at the Institute concentrated their efforts on developing microchemical tests to measure nutritional status. By 1945 they had perfected a series of tests which made it possible, for the first time, to determine the nutritional status of an individual by taking a few drops of blood from a fingertip. For instance, with as little as three-tenths of a milliliter of blood they could successfully measure for nine nutritive essentials or their related tissue constituents, including vitamin A, carotene, ascorbic acid, serum protein, hemoglobin, riboflavin, phosphatase, iron, and nicotinic acid coenzymes. Let me say that prior to the development of these micromethods, such tests required between 30 and 40 milliliters of blood. The new methods not only cut the working time in making tests; it made possible, for the first time, large-scale surveys whereby nutritionists could locate those sections of the population most in need of better nutrition. Best of all, it facilitated nutritional studies with infants and small children. Before Lowry and Bessey perfected their micromethods of analysis, you couldn't study the nutritional status of children because getting a large blood sample was difficult and in many cases unwise.[5]

[5] For further details, see O. A. Bessey and O. H. Lowry, "Biochemical methods in nutritional surveys," *Amer. J. Public Health*, vol. 35:941 (1945); "Microchemical me-

These new methods of nutritional analysis attracted world-wide attention and in the years following the war many scientists visited the Public Health Research Institute to get first hand knowledge of the techniques developed by Dr. Lowry and Dr. Bessey. However, the Division of Nutrition did not content itself with a teaching role, and as interest grew it began to apply its methods in the field. In 1946 Lowry went to Germany and helped establish a nutritional laboratory for the army in Frankfort so they could effectively utilize micromethods of analysis to measure the nutritional status of the German people. In 1947 Bessey conducted a nutritional survey of 1200 selected school children in New York State in order to determine variables in the nutritional levels of school children in different parts of the state. As a direct result of this latter investigation, a hundred or more girls in the Yorkville High School of Women's Service Trades of New York City were treated for low hemoglobin levels.

By 1947 the Division of Nutrition was ready to go beyond the technical methods they had developed for nutritional surveys and to begin work on problems involving nutritional physiology. It was all very promising, when suddenly both Bessey and Lowry left the Institute. Suddenly is a poor word. Just let me say that after the war it became progressively more difficult to live with Dr. Bessey administratively. I don't remember the details of the various hassles that developed both within the department and with the director of the Institute except to say that they were annoying. When Dr. Lowry got an offer of a professorship at Washington University Medical School in St. Louis in 1947, he took it without batting an eyelash. Several months later, Dr. Bessey resigned to take a post as professor of biochemistry at the University of Illinois.

Q: Dr. Rivers, did the Institute suffer with two such important workers in one division leaving almost simultaneously?

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thods for nutritional surveys," *Fed. Proc.*, vol. 4:268 (1945); O. H. Lowry, J. A. Lopez, and O. A. Bessey, "Determination of ascorbic acid in small amounts of blood serum," *J. Biol. Chem.*, vol. 160:609 (1945); O. H. Lowry, M. J. Brock, and J. A. Lopez, "Determination of vitamin A and carotene in small quantities of blood serum," *J. Biol. Chem.*, vol. 166:177 (1946); O. A. Bessey, O. H. Lowry, and M. J. Bròck, "Quantitative determination of ascorbic acid in small amounts of white blood cells and platelets," *J. Biol. Chem.*, vol. 168:197 (1947).

Rivers: I don't mind saying here that I hated to lose Dr. Lowry, and in ordinary circumstances I think that the Institute would have received a setback in the loss of two such important workers as Dr. Lowry and Dr. Bessey, but no sooner did they leave than I was able to attract an extraordinary replacement. I am speaking here of DeWitt Stetten. Dr. Stetten again was one of those unusual fellows with both a Ph.D. in biochemistry and an M.D. He was originally trained at Columbia University but at the time he came to the Public Health Research Institute he had been an assistant professor in the Department of Biological Chemistry at the Harvard Medical School. The thing that attracted me to Stetten was that he was interested in problems of intermediary metabolism and had already mastered techniques of using isotopes as tracer substances. In other words, he was admirably equipped to tackle problems of nutritional physiology and thus give continuity to the work which had been begun by Dr. Bessey and Dr. Lowry.

Dr. Stetten did not disappoint me, and in the five years he spent at the Institute he and his coworkers made considerable contribution to our understanding of uric acid metabolism in cases of gout. I would like to add that they also helped chart the metabolic pathways of glucose utilization and cast much light on the biological oxidation of choline. Now that's quite a mouthful, but, believe me, it's hardly descriptive of a hell of a lot of good work. Dr. Stetten was like a breath of fresh air at the Institute. I liked the boy, and I was damn sorry when the NIH persuaded him to go to Bethesda. Damn sorry.[6]

Q: Dr. Rivers, while the Division of Nutrition and Physiology and the Division of Applied Immunology seemed to have drawn their workers from various institutions, the Division of Infectious Diseases almost seems like an extension of the Rockefeller Institute. I think that it's fair to say that in its early years that division was populated by Rockefeller Institute alumni.

[6] See also DeW. Stetten, Jr., "Studies in intermediary metabolism conducted with aid of isotopic tracers," *Bull. N.Y. Acad. Med.*, vol. 24:87 (1948); J. D. Benedict, P. H. Forsham, and DeW. Stetten, Jr., "Metabolism of uric acid in normal and gouty humans studied with aid of isotopic uric acid," *J. Biol. Chem.*, vol. 181:183 (1949); M. R. Stetten and DeW. Stetten, Jr., "Metabolism of gluconic acid," *J. Biol. Chem.*, vol. 187:241 (1950); DeW. Stetten, Jr., "Pool of miscible uric acid in normal and gouty man studied with aid of isotopic nitrogen," *J. Mt. Sinai Hosp.*, vol. 17:149 (1950).

Rivers: I only had one criterion for choosing people for posts at the Public Health Research Institute, and for that matter at the Rockefeller Institute, and that was their ability. You don't get very far in scientific research if you choose a guy by the way he parts his hair, or the cut of his suit, or the old school tie. It just so happens that many of the people with ability as researchers on problems of infectious disease, who were available as directors of laboratories during the early forties, received some of their scientific training at the Rockefeller Institute. I would like to add here that, through the years, there were plenty of people at the Rockefeller Institute to whom I would never give a job.

The first person I hired for the Division of Infectious Diseases was Louis Julianelle. At the time Dr. Julianelle was a professor at the Washington University Medical School in St. Louis. However, before he went out to St. Louis, he had worked in Dr. Avery's laboratory at the Rockefeller Hospital on problems relating to pneumonia. Dr. Julianelle was a first-rate bacteriologist who knew how to do research, and I was darned pleased that he accepted the post as director of the division. I never regretted bringing him in.

During the early days of chemotherapy, we knew very little about the prophylactic use of various sulfa compounds, and when Julianelle first came to the Institute, he began a series of studies in collaboration with Dr. Morris Siegel which were designed to test the use of sulfadiazine as a prophylactic agent against various types of bacterial infections. They found, for example, that some types of respiratory infections could be prevented by the prophylactic use of sulfadiazine, especially if bacterial secondary invaders were associated with the disease, while other infections, although similar in appearance were not affected at all. To give continuity to this work, they made an extraordinary survey of the effect of continued drug administration on both pathogenic and nonpathogenic organisms that are found in the throat—pneumococci, streptococci, and influenza bacilli, among others.[7] Today such work seems like old hat, but at the time it was of

[7] For further detail, see L. A. Julianelle and M. Siegel, "Epidemiology of acute respiratory infections conditioned by sulfonamides. 1. Gross alterations in nasopharyngeal flora associated with treatment; 2. Effects of treatment on organism and carrier of diphtheria; 3. Trends in pneumococcal types initiated by drug treatment," *Ann. Int. Med.*, vol. 22:10, 21, 29 (1945).

enormous help in learning about things like toxicity of drugs, carrier state, and the development of drug fastness. Much of Dr. Julianelle's work at the Institute had this cast of practicality to it, but I would be remiss if I didn't point out that he also had interest in certain theoretical questions, and in 1946 he instituted a number of studies designed to examine the reversal or dissociation of antibodies and disease-producing organisms. Unfortunately he never got very far in this work because soon afterward he got sick with a myeloma and, although the tumor was later removed, he had a malignancy of one of his ribs and died some weeks after the operation.

Q: Dr. Rivers, did you have much trouble in finding a replacement for Dr. Julianelle?

Rivers: No, because I was lucky enough to get George Hirst. I had known Dr. Hirst from the day he came to the Rockefeller Hospital in 1936. Before you jump to any conclusions, let me say that Dr. Hirst never worked in my laboratory; his job at the Rockefeller Hospital was with Homer Swift and Rebecca Lancefield, and during his tenure at the hospital he worked on problems relating to rheumatic fever and streptococci. I think it is only fair to say that, during his years at the Rockefeller Hospital, I had a down on George Hirst, because I didn't think that he was developing rapidly enough. At that time I don't think that I would have conceded that some people developed more slowly than others. I do know that I didn't make too much of a fuss when he left the Rockefeller Hospital in 1940 to join the laboratories of the International Health Division of the Rockefeller Foundation. In the beginning he worked with people like Frank Horsfall, and Edwin Lennette, studying various problems relating to the susceptibility of man to influenza virus, and testing the efficacy of complex vaccines against influenza A. Later he went off by himself and in 1942, about two years after he left the Rockefeller Hospital, he made everybody sit up when he demonstrated that influenza virus had the power to agglutinate certain red blood cells. In effect, what Hirst did was to make it possible to have a beautiful test-tube case for influenza.[8] Now,

[8] G. K. Hirst, "The agglutination of red cells by allantoic fluid of chick embryos infected with influenza virus," *Science*, vol. 94:22 (1941); "Adsorption of influenza hemagglutinins and virus by red blood cells," *J. Exptl. Med.*, vol. 76:195 (1942); "The

influenza virus is not the only virus that demonstrates this phenomenon of hemagglutination. There are others. I don't think it's important that I give you such a list now, but if you want such a list you can always consult the volume I edited on *Viral and Rickettsial Infections of Man.*

After Dr. Julianelle died, I offered the job as chief of the Division of Infectious Diseases to George Hirst. I knew him, Ralph Muckenfuss knew him, but, best of all, he knew something about virus disease. I was firmly convinced that, with a person like Dr. Hirst at the helm, it would be easier to extend the work of the Division of Infectious Diseases to include research into virus as well as bacterial disease. This is precisely what happened, although I will admit that it didn't happen in quite the manner that I thought it would happen. Almost the first thing that Dr. Hirst did when he took over the division was to set up a virus diagnostic service in collaboration with the Health Department of the City of New York. By 1949, a little less than two years after its formation, it was doing 40 to 50 diagnoses per month on such diseases as influenza, mumps, lymphocytic choriomeningitis and the various encephalitides. Today, it is one of several virus diagnostic laboratories that exist in the United States and certainly one of the pioneers in the field.

In addition to establishing a virus diagnostic service, Dr. Hirst continued his studies of the hemagglutination of influenza virus, developing the scope of such studies by 1949 to include investigations of the interaction between the virus and host cell. I don't think it is hyperbole on my part to claim that some of these studies helped break new ground in virus investigation. Let me illustrate what I mean. About 1950 Hirst and some of his coworkers succeeded in separating influenza A into two subtypes. They then discovered that, when these subtypes were used simultaneously to infect an egg, they gave rise to a third subtype which was new but had the antigenic components of both parent types. It was a grand discovery; the only trouble was that Hirst and his coworkers soon found that the new virus could not be maintained for more than one or two generations. In other words, the change was phenotypic rather than genotypic. I give this example, not

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quantitative determination of influenza virus and antibodies by means of red cells agglutination," *J. Exptl. Med.*, vol. 75:49 (1942).

to illustrate success or failure about a particular experiment, but to demonstrate the nature of the development of virological research in the Division of Infectious Diseases under Dr. Hirst's direction. In a word, it was forward-looking. I don't know how many laboratories and workers have since tried this trick of recombination with viruses. Today it's part and parcel of a whole new set of biochemical and genetic techniques which virologists use to study interference phenomena, techniques I might add, that have the most important implications for the immunization of man against virus disease.

Earlier you mentioned that the Division of Infectious Diseases was populated with alumni from the Rockefeller Institute, and I think that before I conclude my remarks I ought to say something about a Rockefeller Institute alumnus who contributed a great deal to the early success of the Public Health Research Institute's research program on virus disease. I am speaking of Dr. Walter Schlesinger. I mention him not only because he is an important virus investigator, but because I feel a great deal of responsibility for the development of his career, although I hasten to add that he too never worked in my laboratory.

I came to know Walter Schlesinger in 1938 when he first came to the United States. He was born in Hamburg, Germany, and I understand was attending the University of Hamburg Medical School when Hitler and the Nazis came to power. Because he was a Jew, he was forced to leave school and in the circumstances he might well have been lost to science, and that would have been a damn shame, believe me. At that, he was lucky, because circumstance very early forced him to make a decision about remaining in Germany, and in 1935 he left Germany and went to Switzerland, where he entered the University of Basel Medical School to complete his studies. I don't know what prompted him to go to Basel, but from the point of view of becoming a virologist he couldn't have made a better choice. At that time the professor of bacteriology at Basel was Robert Doerr. Dr. Doerr also happened to be a topnotch virologist and had at that time already done much pioneer work on herpes simplex virus. In the two years that Schlesinger spent at Basel, he worked in Dr. Doerr's laboratory as a guest investigator. As a matter of fact, Dr. Doerr was so impressed with Schlesinger that, when Schlesinger decided to migrate to America, Dr. Doerr gave him a very warm letter of introduction and asked him to deliver it to me at the Rockefeller Institute.

I liked Schlesinger from the moment he came into my office at the Rockefeller Hospital. I don't know how to explain it, except to say that he was a cultured gentleman. I remember that I talked to him at length and told him, "Dr. Schlesinger, if you want to stay in this country and get ahead in medicine, I think that the first thing you ought to do is to get an interneship. There is going to be a time pretty soon when foreign students will not get a chance to stand examination for a medical license unless they have had an interneship." He took my advice, but, by God, he did not have an easy time. As a stopgap, he first rode the ambulance at the Beekman Hospital for about three months. Anyone who has ever ridden an ambulance will tell you what a pain in the neck such a job is. But Schlesinger stuck with it and later got a rotating interneship at the Stamford Hospital in Stamford, Connecticut. He stayed with that job for almost two years, and when he had completed his interneship he came to see me once again.

"Dr. Rivers," he said, "I have finished my interneship. Now what can I do?" I said, "Are you interested in doing research on viruses?" When he indicated that he truly wanted to do research, I took him over to Flexner Hall and introduced him to Peter Olitsky. Bless Peter. He sized Schlesinger up then and there and took him on. For the next three years Dr. Schlesinger worked very closely with Dr. Olitsky and Dr. Isabel Morgan on problems relating to the encephalitides and poliovirus, and, I must say, did very handsome work. In 1943 he obtained his citizenship and about a year later joined the army, eventually becoming a member of the Army Neurotropic Virus Commission.

A good deal of the work that Schlesinger did during the war related to dengue fever and was done in collaboration with Albert Sabin. Together they propagated certain strains of dengue virus in mice and chick embryos, and I would like to point out that this marked the first time that dengue virus was propagated in a species other than man and monkeys. Later they used the mouse-adapted dengue virus for immunization of man. It was handsome and successful research, but unfortunately a quarrel later developed between Dr. Schlesinger and Dr. Sabin about credit for the work. I don't know yet the full ins and outs of this disagreement, but I do know that Dr. Sabin held up publication of the monograph that covered this work, because he didn't want Schlesinger's name on the monograph. My impression is that this work is still unpublished. Now I may be wrong about this; it could have been published without my knowing it, but I know that there was a big holdup for a while. If anyone ever tells you that scientists aren't like other people where their self-interest and egos are involved, just kick them in the pants, because the truth is, they are like everybody else, even more so. Most virologists that I know believe that Dr. Schlesinger did more than a fair share of the work on the dengue vaccine that was developed in mice. However, I am honor-bound to point out that my knowledge of this whole affair is secondhand, because I was on Guam when they actually did this work.[9]

[9] Albert Sabin notes: "I don't know where Rivers got this idea. This is completely untrue. The work was published jointly in *Science*; there was no "hold up" (private communication).

ED. NOTE: see A. B. Sabin and R. W. Schlesinger, "Production of immunity to dengue with virus modified by propagation in mice," *Science*, vol. 101:640 (1945).

Walter Schlesinger adds this observation on Dr. Rivers' account of his "quarrel" with Albert Sabin:

Concerning my relationship to Dr. Sabin and the question of publication of our joint work on dengue and other viruses, I should like to make the following comment: During the two wartime years, 1944 and 1945, we worked under tremendous pressure on the assignment of learning something about a group of viruses which were expected to plague our troops in the Pacific and in the Mediterranean theaters of war. These were dengue and sandfly fever viruses. In addition, we received for study a number of field specimens from cases of fevers of unknown origin suspected to be viral in nature. Our work involved characterization of the human pathogenicity of these agents, which we established by tests in human volunteers, and a large variety of attempts to "tame" at least some of these agents by adaptation to some suitable laboratory animal or tissue culture system. At that time, nothing was known about the fundamental biological properties of any of these viruses, except that they were transmitted by certain arthropods and that they produced debilitating disease in man. The practical aim of our work was, of course, to develop vaccines for military use.

Our most concentrated efforts were concerned with the study of the behavior of dengue viruses in man, studies which led to the recognition of at least two immunological types of this agent. Basic studies were also conducted on the mechanism of transmission and many other phases of the human disease, and the major success of our studies consisted of the adaptation of one strain of dengue virus to mice. This, as Dr. Rivers says, was the first successful work done with this virus in laboratory animals. Moreover, adaptation to mice eventually resulted in attenuation of the virus so that it could be used for immunization of human beings.

Dr. Sabin was the commanding officer of our unit and, unquestionably, the driving force and the experienced leader of our team. Yet we were both deeply involved in all the studies, those involving human volunteers as well as the extensive work on laboratory animals. Under the exigencies of war, not much thought was given to the question of

After the war Schlesinger became associate research professor of pathology at the School of Medicine at the University of Pittsburgh and continued his studies of viruses. He did not remain there very long, however, and when he told me in the spring of 1947 that he was looking for a new post I called George Hirst at the Public Health Research Institute and asked him if he wanted to take Schlesinger on as an associate. I didn't have to twist his arm; he knew Dr. Schlesinger and he just grabbed him.

That appointment was no mistake. From the very beginning of his tenure, Schlesinger proved to be an asset in developing virus research at the Public Health Research Institute. He not only took an active role in supervising the virus diagnostic service that Dr. Hirst had established, but he engaged in independent research as well. In one instance, his work in propagating a special strain of dengue virus in eggs led to the development of a vaccination program against dengue fever, in collaboration with the U.S. Public Health Service.

For me, however, the most interesting research that Dr. Schlesinger undertook involved his work on interference. As you know, interference is a procedure whereby an experimentalist can use one virus to so modify a host cell that a second virus cannot multiply normally in, or produce injury to, that cell. I am not going to claim that Schlesinger made world-shaking discoveries in this research. I still remember the frustrations he reported to Dr. Hirst and Dr. Palmer in those early years at the Institute. At that time he was using an encephalitic virus

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publication. Out of the two years of work on dengue and other viruses, one short preliminary paper resulted, of which Sabin and I were coauthors. When things quieted down after our separation from the Army in 1946, I hoped, of course that the vast volumes of work would be the subject of more detailed joint publication. This did not materialize, and many people, myself included, wondered whether publication in full would ever result. Much of the material finally appeared in print in the form of published talks and review articles under Dr. Sabin's sole authorship. The collaboration of various people, including mine, was acknowledged in the text or in footnotes. This is, of course, no substitute for the proper acknowledgment of the scientific contributions made by junior investigators in joint efforts. But the record should be corrected in the sense that no quarrel developed about this matter between Dr. Sabin and myself during our association. . . . I do not believe that a moment of disagreement between two mature people will affect their relationship adversely, and therefore such a relationship is really not furthered or truly represented by excessive emphasis on such an episode. In the long run, I am sure, Dr. Sabin and I are satisfied with the rewards which we have reaped from the work we did together and from the independent careers which we have followed since then (private communication).

as the infecting virus and influenza virus as the interfering virus, and I remember that he had put forth a theory on what the blocking mechanism might be. I don't now recall what that particular notion was—it's unimportant—what I do remember is his saying that the evidence was such that he would have to discard it. Every report he submitted in those early years seemed to produce a new theory, but one thing was constant and that was his concern with trying to elucidate the mechanism of interference. In effect, he came face to face with one of the key problems in virus research—the nature of viral reproduction—and what I like about the boy is that he wouldn't let go. A great deal of work has been done these past 15 years on the problem of interference and I am happy to say that Dr. Schlesinger has contributed his share to our understanding of this phenomenon. When I came to edit my volume on *Viral and Rickettsial Infections,* I chose Schlesinger to write the chapter on interference. It is true that I am fond of him, but if he wasn't the worker he is I wouldn't have touched him with a ten foot pole.

Q: Dr. Rivers, you have given an impressive account of the early accomplishments of the various divisions of the Public Health Research Institute. What were the reactions of city officials to this work? Did they, for example, appreciate the importance of the research programs of the Institute to the city?

Rivers: That is a difficult question to answer. As I mentioned earlier, we had little or no trouble with Mayor La Guardia, because he took us on faith. However, when Bill O'Dwyer became mayor, that faith was lacking, and our troubles began, although I might add for reasons that had nothing to do with the Institute. As far as O'Dwyer was concerned, everything that La Guardia had done before him was wrong, and he was determined to make it right. From 1945 to 1947 the Public Health Research Institute was harassed by City Hall and investigation followed investigation. First, they wanted to see if we had misappropriated any money; then they wanted to see if we had wasted our time. If it wasn't one thing, it was the other, but throughout one thing was clear: Mayor O'Dwyer was doing his damnedest to break the contract that the Institute had with the city. In the end, he was not successful, but it wasn't because he didn't try. Actually it was only because of an accident that Mayor O'Dwyer finally changed his mind about the Institute.

In 1947 New York City had a smallpox scare. A traveler from Mexico came down with smallpox while visiting the city, and in a very brief period twelve cases and two deaths were reported. Under ordinary circumstances, I think that the Department of Health could have coped with this problem, but at the time it had a problem of its own—speaking plainly—it had no leadership. Soon after O'Dwyer became mayor, one of the first people to leave office was Dr. Ernest Stebbins, the commissioner of health. Stebby was a first-rate public health officer, but one day O'Dwyer got him so sore—I don't know the details—that Stebby could hardly wait to get back to his office to resign. When he resigned, Dr. Israel Weinstein, who had a post in the Bureau of Health Education in the Department of Health, was appointed commissioner of health. To my mind Dr. Weinstein, no matter what his talents, was just not cut out to be commissioner of health, and in time a number of people including Mayor O'Dwyer came to the same conclusion. In 1947, a year after he took office, Dr. Weinstein was replaced as commissioner of health by Dr. Harry Mustard.

When the smallpox scare began, Mayor O'Dwyer bypassed Commissioner Weinstein and asked a number of health officials in the city, including Ralph Muckenfuss and myself, to meet with him at City Hall to discuss ways and means of coping with the epidemic. Approximately 150 people attended that meeting, including representatives of pharmaceutical houses. In the beginning, there was a rather long discussion by those present—both pro and con—of what twelve cases and two deaths of smallpox meant in a city of more than 8 million people. Following that, there was a consideration of what was likely to happen if nothing was done. That discussion went on for the better part of an afternoon, and finally the meeting agreed that the people of New York City should be vaccinated on a voluntary basis. That decision brought us face to face with our first real problem, which was to estimate the amount of vaccine we would need to carry out such a program.

After some debate, we finally decided that we would need enough vaccine to vaccinate at least six million people. That figure was dictated by two considerations. The first consideration was that, since the vaccination program was to be carried out on a voluntary basis, we could not realistically expect to vaccinate more than three-quarters of the population, or six million people. Secondly, the virologists and epidemiologists present at the meeting felt that vaccinating six million people would be enough to stop the epidemic, because it was highly unlikely that two million unvaccinated people could sustain an epidemic in the face of that many vaccinated people. If you say that was a rough rule of thumb, I will agree: it was a rough rule of thumb but, in the situation we were in, it was not wise to stop and calculate to the first or third decimal place. In such cases, the rule is to calculate to get the job done.

Knowing approximately how much vaccine we would need was only the beginning of our problems. For example, we soon discovered that, when pharmaceutical houses produced smallpox vaccine it was stored in 10-cc vials and dated, because it could not be kept beyond a certain period in this state. As a matter of fact, when smallpox vaccine was sold to the City of New York or private physicians, it was packaged in one-dose capillary tubes to insure immediate use upon being opened. Moreover, it was against federal law to dispense smallpox vaccine from anything but a capillary tube. It was at that point that we made the harsh discovery that less than five million capillary tubes were to be found among all the pharmaceutical houses combined. Since the lack of capillary tubes meant a delay in starting the vaccination program, it was decided that the city would ask the U.S. Public Health Service to allow pharmaceutical firms to package the vaccine in 10-cc vials and to waive federal regulation on the use of capillary tubes. At that time, Dr. Milton Veldee was chief of the Biologic Control Laboratories of the U.S. Public Health Service, and it didn't take long to convince him that, in the mass vaccination program that we contemplated, it was very likely that a 10-cc vial would be used up during the course of an afternoon in the various city clinics, and that there was just no chance that it would lie around unused for weeks, as might happen under normal conditions.

Throughout the discussion and debate, O'Dwyer said very little. However, when everything seemed to be settled, he turned to the representatives of the various pharmaceutical firms and asked the sixty-four dollar question, "How much is this vaccine going to cost the City of New York?" It didn't take them long to set a figure. I don't remember now exactly what it was, but I do remember that it was high. O'Dwyer looked over to Dr. Muckenfuss and me, and when he saw the expression on our faces he very quickly got the idea that it was too high. "Gentlemen," he said, "I am not going to pay that, it's too much, too much." They allowed that it wasn't and began to dicker. Finally, O'Dwyer got mad as a boil. "Doggone your buttons," he said, "you fellows think you have me over a barrel, well, you haven't. I'm telling you now, if you don't sell me that vaccine cheaper than the price you have quoted, you will never sell me or the City of New York another doggoned thing. What we buy from your companies every year is a hell of a lot more than what we are talking about now for this vaccine." He was madder than hell, but the boys from the pharmaceutical houses just sat quietly. Finally they said, "Mayor O'Dwyer we have to go back to our companies and discuss this thing further; we are in no position to make a decision." Then they got the surprise of their lives. O'Dwyer said, "You boys are not going anywhere. I have put a policeman on every door and not one of you is going to leave this room until you give me a contract at a price I think is fair." They said, "Mr. Mayor we can't give you a contract; everything has to be cleared through the home office." That didn't stop O'Dwyer. "Did you fellows ever hear of the telephone?" he asked. "Well you can go right into my office, one at a time with a policeman and telephone your bosses. You can tell them for me that, if they don't give me that vaccine at what I consider to be a fair price, you will not be allowed to leave City Hall. You can also tell them that, if they don't give me such a contract, I won't buy anything again from their companies. If they still say no, you can tell them to go to hell."

Well, they went in one at a time, and they came out one at a time, and in the end O'Dwyer got the contract he wanted. He paid enough and I assure you that the companies didn't lose anything. I told you this story in detail because this is the kind of man O'Dwyer could be. When he thought he was right, he could be very positive and pretty hard.

The vaccination program, with one exception, was a success and in a period of two weeks approximately 6 million people were vaccinated on a voluntary basis. The exception was tragic. General practitioners at that time knew that it was not good practice to vaccinate a person who had eczema. But what they didn't know and I am not so sure that they had been told properly was that a child with eczema could catch the vaccinia virus from those members of the family that had received a smallpox vaccination. What happened was that several people got vaccinated and later, when the pustule that had formed over the scarification broke, they paid it little mind and went on doing what they normally did without taking precautions. Some parents by merely holding their babies inadvertently vaccinated them when their arms touched some eczema or badly chaffed skin. Five babies died as a result of such inadvertent vaccination. The strange thing is that the people of the City of New York paid little attention to these deaths. They were certainly not kept a secret. As a matter of fact, Harry Mustard's son, who was then an interne at one of the city hospitals, published an account of these cases in one of the leading medical journals.[10] When that article appeared, I expected that there would be the devil to pay, but no one even batted an eye. It just seemed as though the people of New York accepted the fact that they had to be vaccinated against smallpox, no matter what the price. That attitude does not hold for vaccination against other diseases. I'm sure that if five babies ever died as a result of vaccination against polio, there would be one hell of a public outcry.

After the smallpox incident, Ralph Muckenfuss and I were both made welcome at City Hall by Mayor O'Dwyer, and for the first time since he took office we had an opportunity to talk to him about the work of the Public Health Research Institute. His attitude toward the Institute was certainly more friendly than it had been before, and it was clear that he was receptive to the idea of medical research. I don't think that his attitude changed because Dr. Muckenfuss and I were particularly persuasive, though we may have been. My feeling is rather that the incipient epidemic taught him a lesson that he would never forget, and from that time forward he understood the value of having good scientific advice and know-how available to the city in times of crisis.

[10] H. S. Mustard, Jr., and P. W. Hedrick, "Generalized vaccinia; study of 15 cases," *J. Pediat.*, vol. 33:629 (1948).

In 1948 when the contract between the city and the Institute had run seven years, the Research Council decided to approach the mayor for a renewal of contract. There were good reasons for this move. The original contract between the city and the Institute had but three more years to run, and many on the council felt that it would be wiser to try for a renewal with a friendly mayor instead of waiting for the contract to run out and to risk negotiations with a new and perhaps unfriendly mayor. A memorandum outlining the organization, accomplishments, and future program of the Institute was prepared, and I went to see Mayor O'Dwyer. He was cordial enough when I came into his office and that encouraged me. "Mr. Mayor," I said, "the Public Health Research Institute now receives a stipend of $200,000 a year from the city. We have three years to go under the present contract but we would now like to get a renewal for twenty years, at a rate of $400,000 a year for the first two years and $500,000 a year for the subsequent years. We need the increase because we are growing and will continue to grow." O'Dwyer looked at me. "Dr. Rivers," he said, "You haven't learned anything about politics. If you want a contract for twenty years, ask for one for thirty years and maybe you will get twenty. If you want $400,000, ask for $600,000 and you probably will get your $400,000. I'll do all I can, but I am sure you will not get a contract for twenty years. However, I'll do my best." Bless my soul, he was right. In the end, we got a contract for seventeen years. I guess I am just not a politician; my habit is to ask for what I want and I expect to get it, but that apparently is not the way in politics.

Q: Dr. Rivers, as a member of the Research Council of the Public Health Research Institute, and as a member of the Board of Health, you had occasion to work closely with various comissioners of health in New York City. Do you remember any of the early commissioners you had dealings with?

Rivers: The first commissioner of health that I had any dealings with in an official capacity was Dr. John L. Rice who served under Mayor La Guardia. Although he was not very forceful, he made a very good commissioner, because he knew preventive medicine and knew how to run a department of health. Unfortunately he was just not equal to running La Guardia, and La Guardia ran him ragged. Boy, that La Guardia was something to handle.

Q: Did you ever have any run-ins with him?

Rivers: Hell yes!

Q: Could you tell me about some of them?

Rivers: I'll tell you about a couple of them. La Guardia had some of the queerest notions. They were not very intelligent, and they certainly were queer. One day he called the Board of Health together at his office—I don't know whether somebody put him up to this, I think it was probably his own idea—and harangued us for the better part of an hour about how degrading it was to eat horsemeat. "Gentlemen," he said, "I was over in the Balkans during the World War and I saw people eating horsemeat and I want to tell you the eating of horsemeat is degrading. I don't want any of the people of the City of New York eating horsemeat. I want you to write into the sanitary code that it's unhealthy to eat horsemeat and I want you to close all the stores in New York that are selling horsemeat."

There were five of us on the board and we kept looking at one another while he harangued us. Finally they elected me to do what had to be done. Hell, I didn't know what in the name of God to do. Once La Guardia got to ranting, he was a hard man to stop, because it was hard to get him to listen. You had to do something startling to catch his attention. Finally I said, "Mr. Mayor, would you please quit being a God damned fool!" I want to tell you, that got him. He said, "What do you mean, Rivers?" I said, "I mean just what I said. I love New York City, and I don't want a God damned fool as mayor of the city, and that's what you're making out of yourself." He said, "What do you mean?" I said, "Everything you've said is nonsense. I am not going to argue with you, and I'm going to lay my cards on the table. You say that the eating of horsemeat is unhealthy—good—I am going to publicize that statement in all the newspapers, and then I am going to ask you publicly to explain to the people of New York City why you spent $165,000 last year to buy antipneumococcal serum made in horses. I'll also ask you how degrading it is to get all of that horse protein intravenously."

He looked at me and the board. "Gentlemen," he said, "the meeting is over." We got up and went out; not another word was said. The next time I met the mayor he looked at me with a grin all across his face. "Rivers," he said, "how is horsemeat?" That's the kind of a guy he was. He had a wonderful sense of humor, and it never took him long to admit a mistake.

Q: You must admit that you brought him up with a start?

Rivers: Oh, I would do this to any opponent whether he was a politician or scientist. The next time I had a run in with La Guardia, it involved the Milk Drivers' Union. La Guardia was an extremely strong believer in unions, and when he served in Congress he helped push through a bill outlawing "yellow dog contracts." One of the strongest unions, then and even now, in the City of New York was the Milk Drivers' Union, and at one point they persuaded La Guardia that he ought to eliminate Class C dealers in milk. Perhaps I ought to explain that there are three types of milk dealers in New York, A, B and C. I won't try to explain who A and B dealers are, but I will tell you that Class C dealers are small businessmen who perhaps own a truck and have a small milk delivery route. There were at one time about 900 to 1000 such dealers in the city. You see, there is always milk left over at pasteurizing plants and, after the large companies pick up their milk, these small dealers appear and buy pasteurized bottles of milk at a cheaper rate. They in turn sell it to their customers, who are, generally speaking, poor people, two or three cents cheaper a quart than the large companies might charge. Furthermore, they would deliver their milk directly to the door, whereas the regular milk drivers might leave it in a bunch on the first floor of a five-story apartment building. Door delivery griped the Milk Drivers' Union and somehow they persuaded Mayor La Guardia that the milk that Class C dealers delivered was unhealthy. Again La Guardia called the Board of Health together and said, "Gentlemen, I want you to write into the sanitary code that the milk that Class C dealers deliver is not up to the standards of the other dealers and probably dangerous to the health of the people drinking it. I want you to rule in such a manner that it will be impossible for these people to operate."

The mayor harangued us a long time about Class C dealers, and again the boys on the board didn't feel like tackling him. Finally I said, "Mr. La Guardia, what you are saying is not true. We all know how this milk is bought and delivered, and you might as well know that you are not going to get anybody on this board to say that the milk delivered by Class C dealers is any different from the milk delivered otherwise. If you make an issue of it, the board will go on record that you are in cahoots with the Milk Drivers' Union to do the poor people of this city a hard knock—that you are in favor of taking money out of their pockets. I can tell you now that in matters of health they would take our word against yours. So you might as well tell the Milk Drivers' Union to head in."

That was the end of it. To this day, we still have Class C dealers in New York. These are the things that one has to do when one is on a board of health. Politics. I don't blame La Guardia. How the hell was he supposed to know all these things? These fights and other fights like them never affected my personal relations with either Mayor La Guardia or Mayor O'Dwyer. Each, by the way, offered me the post of commissioner of health and commissioner of hospitals. Although I was interested in both these organizations and worked hard on their boards, I couldn't see giving up my job at the Rockefeller Institute and becoming a man that might not have a job tomorrow—as a commissioner. The commissioner is appointed by the mayor and can be discharged on a moment's notice. If a new mayor is elected he is as likely to go out as not. I didn't mind working on the Board of Health and doing all I could, but I couldn't see being a boy to be shot at every day, so I never did accept.

After I rejected Mayor O'Dwyer's offer, Harry Mustard, then director of the School of Public Health and Hygiene at Columbia University was offered the post and accepted. Of the health commissioners that I worked with up to Mustard's time, I must say that he was the best of the lot, because he had the unusual quality of being able to deal with people without irritating them. He was also a man who understood public health problems. Prior to coming to Columbia, he had served as a county public health officer in West Virginia, and at one time in the early thirties had even had a stint as state commissioner of health in Tennessee. He also had the understanding that comes from the study of public health problems as a professor of public health administration. As I say, he made an excellent commissioner and I'm sure he could have stayed on much longer if he wanted to, but after two years as commissioner he decided he had had enough and left to take a post as executive director of State Charities Aid Association of New York. You know, of all the commissioners of health that I have seen, as a member of the Board of Health, my great favorite is the lady who runs the show now—Leona Baumgartner.

I first ran into Leona Baumgartner when she was a Ph.D. at Yale and was working with John Fulton, the neurophysiologist and historian of medicine. At one time Dr. Fulton got her interested in the history of medicine to the extent that she later published with him a very interesting bibliography on the various editions of the poem *Syphilis* by Fracastorius. I don't remember the reason but on one occasion she came to visit me at the Rockefeller Hospital. She was a pretty little thing, and I took advantage of my august position as a member of the Rockefeller Institute to give her a long lecture. "Leona," I said, "a Ph.D. woman just never gets anywhere because everybody takes advantage of her. People know that she can't get a job easily, and so she never gets advanced and never gets a raise. Why in the name of God don't you take an M.D.? Then you can thumb your nose at anybody because, if they don't give you what you want or ask for, you can always go out and practice medicine and make a living."

I didn't realize that I had been convincing, but some time later Leona did go on to get her M.D. After she completed her interneship, she joined the New York City Department of Health, specializing in problems of child health and hygiene and was actually assistant commissioner in charge of maternal and child health service when Mayor Wagner appointed her commissioner in 1954.

Q: How did the Board of Health take to a woman commissioner?

Rivers: You never think of Leona as a woman although she's very good-looking. The point is that she can hold her place against a man.

Soon after she became commissioner of health, somebody gave a luncheon in her honor, and I found myself sitting next to her. "Leona," I said, "I have never asked you whether that sermon I gave you about becoming an M.D. helped." Of course, she could never have been made commissioner of health unless she had an M.D. because the charter of New York City specifies that the commissioner has to be an M.D. She looked at me and said, "Tom, you know, one other person gave me the same sermon, and that was old Dean Winternitz of the Yale Medical School. When two people like you two gave me the same sermon, and I knew damn well you hadn't been conferring, I began to think. The sermons that you two gave me made me take my M.D." I said, "Are you glad?" It was a hell of a question to ask, and it turned out she wasn't about to answer it. She just smiled. "Tom," she said, "those sermons had a great influence on the rest of my life."

Q: Dr. Rivers, would it be possible for you to distinguish the way Dr. Baumgartner runs the Department of Health from the way previous commissioners have administered it?

Rivers: I don't know that that is possible without making a minute examination of the decisions made by Dr. Baumgartner and the other commissioners. Speaking broadly, however, I don't know that Dr. Baumgartner runs the show any differently from the way it would be run by any good commissioner of health. In the first place, she is not a free agent and has to follow the rules and regulations that are set down in the sanitary code and in other city statutes. To be sure she can do a lot of things, but there are certain things that she just can't do. She has to stay within the framework. All commissioners do, or they are immediately called. I know, because Harry Mustard and I were once called down by the corporation counsel for the City of New York and told that one of the regulations that the Board of Health wanted to write into the sanitary code was in direct contravention of the criminal code of the City of New York and would have to be rescinded. Briefly, we wanted to have boys exposed to mumps before puberty and girls exposed to German measles before they were married. The regulation we wrote urged parents and doctors to see to it that boys and girls were exposed to these diseases. Well, when the corporation counsel for the city saw the draft of this regulation, he called us down and spanked hell out of us. "You can't do that," he said, "the penal code states very clearly that it is a criminal offense for anyone knowingly to expose another human being to an infectious disease."

I looked at him and said, "Well, what about vaccination against smallpox? We give people vaccinia virus in such cases." "Well," he said, "we don't consider that breaking the law. It's been going on for such a long time that it's accepted." I said, "What about BCG vaccine? Isn't that giving live tubercle bacilli to people?" "Oh no," he said, "we don't consider the administration of BCG breaking the law." I said, "How about yellow fever vaccine? Isn't that introducing people to a live virus?" "Yes," he said, "but we think that is acceptable." You know, we just couldn't budge that fellow, and he made us take the regulation out. Finally I looked at him and said, "Why in the name of God was that statute put in the criminal code in the first place?" He replied, "So we can catch prostitutes. We can't say prostitutes in the law, because that would be discriminatory, so we put in this statute and a policeman now can pick up a streetwalker on the grounds that she may be exposing a person to an infectious disease." It was something I had never heard of before.

In many ways my service on the Board of Health provided me with a liberal education. The function of the Board of Health is not only to advise the commissioner of health; when you come right down to it, its basic function is to write the sanitary code. That code, I might add, touches every aspect of life of every citizen and for that matter noncitizen in the City of New York. The Board of Health does not only deal with the control of disease, it concerns itself with child hygiene and welfare, the keeping of vital statistics, the distribution and handling of food and drugs, drinking water, the disposal of sewage, the heat in tenements, and a hundred things I haven't mentioned. When I was on the board we used to have a meeting once a month. Before that meeting we would receive a calendar with all the appropriate documents relating to items on the calendar. We were expected to do our homework, and to help us we would frequently have people in to hearings and take testimony. The one day that we spent at formal meetings is really no gauge of the amount of work we did. I can tell you one thing: the decisions that the Board of Health arrived at were not frivolous or haphazard.

I was a member of the Board of Health for 18 years. In 1955 I figured I had served long enough, and I suggested to Dr. Baumgartner that I get off and let some younger blood come on. When she asked who I thought would be capable of taking my place, I recommended Frank Horsfall and Lew Thomas. She tried Frank first but he was too busy. Later she tried Lew and he accepted and he's been on ever since. According to Leona, he's one of the best board members she has ever had. It's what I would have expected.

Q: Dr. Rivers, a little bird once whispered to me that you left the Board of Health because you didn't want to work on the revision of the sanitary code.

Rivers: Who ever said that don't know me very well. Hell, I just love to rewrite things.

CHAPTER 11

Some Aspects of Poliomyelitis Research—1946-1948

The solution of the major problems related to infantile paralysis will not materialize out of the cerebrations of one individual or of a small advisory group. On the contrary, the solutions to these problems will come forth only if properly qualified investigators are stimulated to approach these problems in a critical and thorough fashion.

Dr. Harry Weaver to Dr. Hart Van Riper, *Interoffice Communication, The National Foundation for Infantile Paralysis,* August 9, 1946

Q: Dr. Rivers, I would like to examine with you the developments in polio research in the immediate postwar period as you saw them from your vantage point as chairman of the Virus Research Committee of the National Foundation.

Rivers: The first order of business that I became involved in when I returned to the Foundation after the war had nothing to do with research per se or with polio. It involved editing a textbook called *Viral and Rickettsial Infections of Man.* Although I had previously edited a volume on *Filterable Viruses* in 1928, in no sense did the idea for this particular book originate with me. That honor belongs to Paul Clark of the University of Wisconsin. Dr. Clark was a grantee of the Foundation and in 1946, while making a request for some extra monkeys for experimental purposes, he urged the Foundation to undertake the publication of a new textbook on virus diseases. I have that letter in