CHAPTER 9

NAMRU 2—A Virologist at War

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

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

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

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

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

In 1917 the Rockefeller Institute was militarized and became U.S. Army Auxiliary *Laboratory No. 1, and the members of its staff secured commissions as officers of the U.S.* Army *Reserves under the Commanding Officer, Colonel Simon Flexner. There were two exceptions, Drs.* Avery *and Kligler (apart from German citizens for whom special rules were made by Washington to retain them). Dr.* Avery *was a Canadian and hence could only serve as private; he later was commissioned as officer. Dr. Kligler was also a private for reasons unknown to me; he later became a sergeant. An adjutant of the army was detailed to the Institute to take care of the Army paper work and commanded the technicians and helpers, maintained an Army discipline among them, and drilled them at parades, etc., on York Avenue, with our lay neighbors looking on, I hope, with pride. A mobile, complete hospital unit (on wheels) consisting of several small buildings, wards, laboratories, laundry, kitchen, etc., was rolled into the front yard* we would become involved in World War II. I didn't see how we could keep out of it, and early in 1940 I persuaded some of the doctors in the hospital and some of the boys in the laboratories to join the naval reserve. Soon afterward I helped organize the Rockefeller Hospital Naval Research Unit. It wasn't difficult to persuade the Navy's Bureau of Medicine and Surgery of the value of such a unit, but I did have the devil's own time in convincing them that they ought to sign a contract with the Rockefeller Hospital to take care of navy patients. It was difficult, because the Rockefeller Hospital could not receive payment for any services it performed, and the U.S. Navy had an inflexible rule that said it could not get something for nothing. We argued the question for the better part of a year, and if Pearl Harbor hadn't happened we would probably still be discussing it. However, once we were in war, Gardner Robertson, who was in command of the Brooklyn Naval Hospital, received instructions from Washington to reach an agreement with the Rockefeller Hospital. He proved to be a very understanding negotiator, and it didn't take long for us to agree that for a dollar a year the Rockefeller Hospital would take navy patients, feed them, furnish nursing care, and do research on particular diseases.

It turned out to be a very fortunate arrangement. The Brooklyn Naval Hospital, I am happy to say, followed a hands-off policy. Had they interfered with us, it might have been difficult for us to achieve what we finally accomplished; however, they had complete confidence in us and I practically never heard from Captain Robertson. Actually all of my business with the Naval Hospital was carried on through a very wonderful liaison officer named Captain Julius Neuberger. Dr. Neuberger was a real character. He was small and round and never

of the Institute (from 64th to 66th Street); here were treated chiefly soldiers returned from abroad, especially such as were afflicted with intractable wounds.

Apart from this service, and frequent visits for epidemiological observations to nearby cantonments struck down by epidemics, most of the activity consisted in training officers (and a few enlisted men) in military medicine and surgery, the Institute staff conducting the courses, which included laboratory procedures. New classes of a score or more appeared each month. Also, certain laboratories prepared various antisera for treatment and diagnosis. Horse antisera were produced at the Institute's laboratories at Princeton, N.J. The entire vast expense of all these activities, which were carried out for the duration of the war, were borne by the Institute. For the most part, research work was engaged in whenever time permitted—it was, however, at a greatly reduced level (private communication).

without a big fat cigar in his mouth. I always thought that, if he had walked out on a vaudeville stage and just acted naturally, he would have had great success as a vaudevillian. He could always make me laugh by just coming into my office.

Dr. Neuberger was an eye, ear, nose, and throat man and an excellent clinician. While he knew little about research—and had been trained to know little about it—he nevertheless had great understanding and appreciation of the difficulties involved in carrying on such activities, and on more than one occasion kept us from running into difficulties with naval medical administrative procedures. He did this without disrupting the folks at the Brooklyn Naval Hospital and without making us at the Rockefeller Institute feel inferior.

He was helpful in other ways. The Rockefeller Hospital was never listed as a naval hospital and was always classified as a civilian hospital, and when rationing was put into effect we like other civilians had to get in line to get food and supplies. I never said anything about this to Dr. Neuberger, but he seemed to have a sixth sense when we needed extra food and other material. If we ran short of meat, suddenly Neuberger would turn up with turkeys and roasts and what have you. I never asked him where he got the stuff, and I am still not asking. Unlike Aladdin, I didn't have to have any magic lamp because I had Dr. Neuberger and I want to say right here that he contributed a great deal to the success that the Rockefeller Hospital had in caring for naval personnel throughout the war.

Q: Dr. Rivers, was there much collaboration between the Rockefeller Hospital and the Brooklyn Naval Hospital?

Rivers: I would say that the relations between the Rockefeller Hospital and the Brooklyn Naval Hospital were minimal, and we had little impact on one another. The Brooklyn Naval Hospital was a unit unto itself and during the war had the services of many excellent clinicians and surgeons. Unlike the Rockefeller Hospital, they did not do research, nor were they set up to do research, although it is true that from time to time reserve officers who knew something about research were stationed there. For example, Dr. Alvin Coburn, who had done some very nice work on rheumatic fever before World War II, served at the Brooklyn Naval Hospital for a time, as did Dr. Marion B. Sulzberger, an extraordinarily gifted dermatologist who during the war worked on problems related to the effects of lewisite gas and later served with me on Guam.

Occasionally when the boys at Brooklyn got a tough case they would send it on to the Rockefeller Hospital. Once, for example, they sent us a case of tropical eosinophilia. It was a disease which we at the Rockefeller Hospital had at that time never seen before or for that matter heard of. I turned it over to Kendall Emerson, and within a very brief period he ran it down. Another time, a young marine officer was sent to us from Brooklyn with an unconfirmed diagnosis. When he arrived at the Rockefeller Hospital he was partially disoriented, and it was almost impossible to get a case history. Robert Watson, who was then a resident at the hospital, was put in charge of the case and to his credit came up with the correct diagnosis. The poor marine had a dermatomyositis. A day or two after the patient was admitted, his commanding officer came to visit him. It was an act of kindness, but he got a reception that I don't think he will ever forget. The young marine greeted him with language that would have made a dock walloper blush. I have heard some plain and fancy cursing in my time but none to compare with this. Luckily the boy's commanding officer understood that he was out of his head and not responsible for what he said, and he kept his temper. Nevertheless by the time he left the hospital he was fit to be tied. At the time we didn't think the boy would get better, because the prognosis then in cases of dermatomyositis was very poor. If I remember correctly, at least 50 per cent died. However, this young marine recovered and later took part in the invasion of Europe. Dr. Watson followed the case for a long time and some years after the war I learned from him that the patient was still alive and well.

I don't want to give you the impression that the Rockefeller Hospital only took cases that the boys at the Brooklyn Naval Hospital couldn't handle. Actually such cases were but a small fraction of the cases we admitted. The bulk of our naval patients fell into three categories: hepatitis, primary atypical pneumonia, and rheumatic fever, and in practice we accepted such cases routinely from the first, third, fourth, and fifth naval districts.

Q: Dr. Rivers, earlier you said you didn't want the war to disrupt the research in progress at the Rockefeller Hospital. Didn't the acceptance of cases of hepatitis and primary atypical pneumonia disrupt the research programs at the Rockefeller Hospital?

Rivers: Sure it did. But what difference did that make? It didn't ruin the hospital. It just meant that we had a different class of patients. To be sure, we hadn't worked very much on these diseases before the war, but I assure you that it did not alter the attitude of our physicians one bit, and they took the change in stride. For example, just prior to World War II, Dr. Charles Hoagland organized a laboratory for the study of muscular dystrophy; however, as soon as naval patients came into the hospital, he switched his laboratory to a study of diseases of the liver. It's not boastful to say that Hoagland's unit made outstanding contribution to our understanding of hepatitis during World War II. Looking back, I would say that it was a good thing that shift occurred, because it is highly doubtful that Hoagland and his group would have made as outstanding a contribution in muscular dystrophy as they did in hepatitis. It is, of course, speculation on my part, but you know nothing much has been discovered in the field of muscular dystrophy since 1940, and many good minds have been occupied with the subject since that time.

Q: Dr. Rivers, could you tell me the nature of the Rockefeller Hospital's contribution toward an understanding of primary atypical pneumonia?

Rivers: To do that properly I have to tell you about Frank Horsfall, and that ain't hard because he has long been one of my favorites, although he personally never worked in my lab, like Joe Smadel or Charley Hoagland or Lew Thomas. Horsfall was born in Seattle, Washington, about 55 years ago and received his education in that city. He is a big tall fellow, and when he attended the University of Washington he was a member of the rowing team. I understand that it was a good team. I don't know what made Frank decide to become a doctor, but after he took his B.A. at the University of Washington, he went east to Montreal and enrolled in medical school at McGill

University. McGill, then as now, had an excellent medical school and Frank received good training, but he did not stay there after he received his medical degree. While in medical school, Frank became interested in the bacteriological work that Hans Zinsser was doing in Boston, and instead of taking a regular interneship at the Royal Victoria Hospital he took a job as house officer in pathology at the Peter Bent Brigham Hospital in Boston.

It was during his tenure in Boston that he became interested in the phenomenon of hypersensitivity and began to study the condition experimentally in human beings. I would say that it was probably his first important work, and it basically had two results. First, his results indicated that in the case of formaldehyde hypersensitiveness, the chemical combines with a protein in the host, thereby creating a new antigenic and reactive substance. And, second, he developed one of the most exquisite sensitivities to formalin that you can imagine. To this day, Frank can walk into a building and if anyone at all has been working with formalin in that building—it doesn't matter when—he'll break out, although no one else can even smell the stuff.

By the time Dr. Horsfall finished his work in Boston, he was no slouch in the laboratory. However, after a year in Boston, he returned to the Royal Victoria Hospital in Montreal as a resident physician, and the following year came to the Rockefeller Hospital as an assistant resident and worked in Dr. Avery's department. As I mentioned earlier, Dr. Avery's department was interested in problems of pneumonia, and it should come as no surprise that in this atmosphere Dr. Horsfall turned his attention to pneumococcal infections and more directly to the nature and behavior of antibodies specifically directed against pneumococci. Together with Kenneth Goodner, who today is professor of bacteriology and immunology at the Jefferson Medical College in Philadelphia, Horsfall developed an antipneumococcal rabbit serum. Don't ask me why they became interested in the antipneumococcal effects of rabbit serum, because I don't know, but let me tell you that the serum that they developed was far superior to the horse serum which was then generally used for the treatment of pneumococcal pneumonia. It was pretty phenomenal. Both Ken and Frank were on the threshold of reaping a lot of renown for developing the best antiserum in the world against pneumococci. As a matter of fact, when the Lederle people heard about this development they began to build rabbit hutches in anticipation of producing the serum commercially. The future looked bright, and then along came sulfapyridine. Lord bless you, sulfapyridine was so far superior and cheaper than the antipneumococcal rabbit serum that it knocked everything into a cocked hat, and poor Ken and Frank were left high and dry. In 1937 Dr. Goodner left the Rockefeller Hospital and joined the International Health Board of the Rockefeller Foundation to help in the making of yellow fever vaccine. At the same time Dr. Horsfall also joined the International Health Board, but unlike Goodner he elected to continue his studies with Arne Tiselius at the Institute of Physical Chemistry in Uppsala in Sweden.

To tell the truth, I was sorry to see Dr. Horsfall leave the hospital, although I knew that he didn't care much for clinical work and would rather spend his time doing research. I guess he thought that the Rockefeller Foundation offered him a better chance for research than the hospital. I won't argue the point, but I will say that Horsfall's work with Dr. Tiselius proved important for the subsequent development of research at the Rockefeller Hospital. After World War II, for example, the Rockefeller Hospital sent Dr. Henry Kunkel to Tiselius's laboratory for advanced study and training. Henry is a bright young fellow and benefited a great deal from his stay in Uppsala. He and Tiselius worked well together. However, I have always felt that a good deal of Kunk's success with Tiselius was due to the fact that Frank Horsfall had preceded him at Uppsala. Frank set the tone, and ever since his initial trip to Sweden in 1938 there has been a continuous interchange of personnel between the Rockefeller Hospital and Tiselius's laboratory to the mutual benefit, I think, of both parties.

Dr. Horsfall spent four years with the International Health Board of the Rockefeller Foundation, working a large part of the time on problems relating to influenza, and it was during this period that he developed a complex vaccine against type A influenza. Unless my memory completely fails me, I believe that this vaccine was the first to be used on a large scale in the prevention of influenza in human beings. For me, however, the most important aspect of his studies during these years was his discovery of a new virus which is indigenous and usually latent in most stocks of ordinary albino mice used for experimental studies. I will speak more of the importance of this finding later; for now, let me say that I watched Dr. Horsfall's progress carefully, and, when I thought that he had had enough with the International Health Board, I made an all-out effort to get him back.

Q: What do you call an all-out effort?

Rivers: I offered him the world with a fence around it—in other words, a membership in the Rockefeller Institute and a post as physician to the Rockefeller Hospital. Let me tell you that, in 1941, there were not many people around who would turn down a membership in the Institute, and as I had hoped Frank accepted my offer. When naval patients started coming into the hospital, Frank took charge of them—hepatitis, rheumatic fevers, as well as the primary atypical pneumonias. In time, however, he began to concentrate his research efforts on primary atypical pneumonia and very quickly demonstrated that it was futile to try to make a diagnosis of such cases in the absence of abundant laboratory data. For instance, he showed that the presence or absence of pulmonary consolidation was not consistently discernible without roentgenological studies, and thus cast doubt that a trustworthy etiological diagnosis could be arrived at without proper serological tests with acute phase and convalescent sera. As a matter of fact, he demonstrated that some patients thought to have pneumococcal pneumonia actually had pneumonias induced by influenza B. I might add here that, at that time, few physicians recognized that influenza B might induce a pneumonia. His work with primary atypical pneumonias gradually led him to the conclusion that many respiratory infections were not simply due to an invasion by a bacterium or a virus, but were due to more complicated relationships, and he turned his attention once more to the role that latent viruses (particularly PVM) played an inducing respiratory infection.[2]

In 1944 pneumonia research everywhere got a boost when A. R. Dochez at the College of Physicians and Surgeons, with the aid of an assistant, Miss Katherine Mills, demonstrated that heated PVM agglutinated red blood cells, while unheated virus did not. In one stroke Dochez and his coworker made it possible to study PVM by

[2] For a review of the problems in diagnosing primary atypical pneumonia, see F. L. Horsfall, Jr., "The diagnosis of primary atypical pneumonia," in F. L. Horsfall, Jr. (ed.), *Diagnosis of Viral and Rickettsial Infections*. Columbia University Press, New York, 1949, pp. 42–56.

means of simple and rapid techniques in the test tube, and I am not exaggerating when I say that that discovery soon yielded results that were not possible by animal experimentation alone.[3] For example, within a year Dr. Horsfall extended Dr. Dochez's studies and was able to cast light on the mechanism governing the relation of PVM to both the host cell and red blood cells. He showed that, while PVM in mouse lung suspensions was infectious for mice, it was not necessarily infectious for chick embryos, nor would it agglutinate red blood cells. However, if the whole mouse lung infected with PVM was centrifuged and tissue fluids squeezed out without breaking up cells, these fluids would contain PVM that would be infectious for mice and chick embryos and would agglutinate red blood cells. The most extraordinary aspect of this finding was that, in its former state, the PVM was a particle with a diameter of approximately 100 millimicrons. However, after centrifugation, the PVM found in the tissue fluids became a particle with a diameter of about 30 millimicrons. In essence, Frank demonstrated that, when the pneumonia virus of mice was usually encountered, it represented a combination of virus with host protein, and that it was in fact possible to separate the virus from the host protein without destroying its activity.

There is one other contribution that Horsfall made to our understanding of primary atypical pneumonia during World War II that I would like to mention before you toss another question at me. Sometime in the nineteen forties—I don't remember exactly when—a patient by the name of McGuiness was admitted to the Rockefeller Hospital. The poor fellow had a primary atypical pneumonia, and in spite of our efforts soon died. On autopsy, Dr. Lew Thomas isolated a nonhemolytic streptococcus from the lung of this patient and in his honor named it McGuinococcus. It has since come down in the literature as streptococcus MG. I should add here that no evidence has ever been presented that would implicate this strep as the cause of primary atypical pneumonia cases, and many patients during their convalescence also develop agglutinins against this strep. Mind you, its existence isn't proof positive of primary atypical pneumonia, but it is helpful as a diagnostic aid. Since the MG strep turned up in about

[3] K. C. Mills and A. R. Dochez, "Specific agglutination of murine erythrocytes by a pneumonitis virus in mice," *Proc. Soc. Exptl. Biol. Med.*, vol. 57:140 (1944).

50 per cent of all cases, and since many workers also believed that a virus was associated with the disease, Dr. Horsfall and Dr. Thomas decided to study the effect on mice of a combined infection of MG strep and PVM. I don't think that I am wrong in suggesting that in designing such an experiment they were following a path developed earlier by Dick Shope.[4]

Some years before, Dick Shope had demonstrated that typical swine influenza did not result from the action of a single agent but was, in fact, due to the synergistic action of swine influenza virus and swine influenza bacillus.[5] Be that as it may, very soon after starting their experiments, Dr. Horsfall and Dr. Thomas discovered a most unexpected phenomenon. Instead of the MG strep enhancing the infection of the mouse pneumonia virus, it completely inhibited its action. The inhibition wasn't lasting and disappeared in a period of about two weeks, but there is no doubt in my mind that this experiment was the first time that the inhibition of virus action by means of a bacterium was clearly demonstrated.

What makes Frank's work during World War II even more remarkable was that he did it while carrying the administrative burden of running the hospital. I should have mentioned earlier that, late in 1943, I turned over the administration of the hospital to Frank when I undertook duties which subsequently led to the formation of Naval Medical Research Unit No. 2 in the Pacific.

Q: Dr. Rivers, I have heard a great deal of talk about NAMRU 2, and I wonder what were the circumstances that led to the establishment of this unit?

[4] For further details of the work of Thomas and Horsfall on streptococcus MG and primary atypical pneumonia during World War II, see F. L. Horsfall, Jr., et al., "A virus recovered from patients with primary atypical pneumonia," *Science*, vol. 97:289 (1943); L. Thomas, et al., "Serological reactions with an indifferent streptococcus in primary atypical pneumonia," *Science*, vol. 98:566 (1943); L. Thomas, et al., "Complement fixation with dissimilar antigens in primary atypical pneumonia," *Proc. Soc. Exptl. Biol. Med.*, vol. 52:121 (1943); E. C. Curnen, et al., "Studies in primary atypical pneumonia. 1. Clinical features and results of laboratory investigations," *J. Clin. Invest.*, vol. 24:209 (1945). These articles are by no means the totality of the work on primary atypical pneumonia that came from Dr. Horsfall's laboratory during the war and they are cited only as a representative selection of that work.

[5] Dr. Rivers has reference here to R. E. Shope, "Swine influenza. III. Filtration experiments and etiology," *J. Exptl. Med.*, vol. 54:373 (1931).

Rivers: In July of 1943 the Surgeon General of the Navy, Ross McIntire, invited me to Washington to join a committee of senior naval medical officers to discuss the problems of scrub typhus and infectious hepatitis, which were then plaguing our armed forces in the South Pacific. I don't remember how many cases there were, but it was enough for the committee to consider the advisability of sending a commission to that part of the world to study the question. During the discussion, a great deal of doubt was expressed as to the utility of work performed by temporary commissions, and some one suggested that it might be a better idea if a permanent research unit were established close to the fighting lines to investigate medical problems as they came up. While I don't remember who made the suggestion, there is no doubt in my mind that the idea was close to the heart of Admiral McIntire. The army at that time was getting a great deal of publicity for their research work, and Admiral McIntire was concerned that the navy had not done very much along these lines. Whether the idea for a research unit originally came from Ross McIntire, I don't know. It could just as easily have been planted in his mind by Rear Admiral H. W. Smith. Smith was quite a sharp fellow, and I had great admiration for him. I'll tell you this, if he did plant the idea in Ross McIntire's head he would never let on to such a thing, because he was a very discreet man. As a result of this meeting, I was asked to take a trip to the South Pacific to see whether it would be useful to organize a medical research unit at the fighting front.

Q: Was this the first time you had ever been in the South Pacific?

Rivers: Prior to this trip, I had never been farther west than San Francisco and now with one jump—I should say several jumps—I went to Noumea in New Caledonia, which then served as headquarters for Admiral Halsey. Actually my trip had a double purpose. I had to make a survey of the medical research needs in the South Pacific, and I had to convince the top naval commanders in the area that they needed a medical research unit. I should make clear that no unit of the kind that Ross McIntire had in mind could go to the South Pacific unless it was requested by the area command. In other words, Washington couldn't be sending out a bunch of truck that would be in the way.

For several days after I arrived in New Caledonia I visited army and navy hospitals which were located on the island to become familiar with the kinds of patients they cared for, and after this brief orientation I set out to make my survey. During the next three weeks, I visited medical units and hospitals in the New Hebrides, on up through the Solomons, as far as the Russell Islands. At the time I wanted to go up and see some of the fighting which was then going on in New Georgia, but I couldn't get any orders to do that approved, and so I called it a day and returned to Noumea. During my survey I was struck by the fact that medical officers near the fighting front were really and truly out of contact with what was going on in the United States in regard to medical research, and what information was available had filtered down very slowly.

Q: Dr. Rivers, during the Crimean War, the Russian surgeon, Nikolai Pirogoff, defined war as an epidemic of trauma. Were you concerned in your survey with medical problems caused by actual warfare, or with the diseases that were to be found in the South Pacific?

Rivers: Medically speaking, there were other things besides gunshot or shell wounds that bothered the boys in the South Pacific. For example, one of the biggest problems that we faced was malaria. While it is true that there was not too much at Noumea, there was plenty in the Solomon Islands. Heavens above, when I visited Guadalcanal, one whole army division was practically put out of commission by malaria and eventually had to be sent to New Zealand to recuperate. I have now forgotten which division it was, but when I saw them they were all yellow from taking atabrine and were in a helluva bad shape. In other places some of the boys picked up filaria, which caused elephantiasis.

I can't now begin to catalogue all the medical problems that we faced. For instance, some of the boys on Guadalcanal went swimming and came down with a schistosomiasis that hit their livers pretty hard. Now it is true that we knew about schistosomiasis, but the schistosomiasis we knew about was found in the Caribbean, and the snail that carried this type was quite different from the snail that carried the disease in the South Pacific. Then there were a lot of things that we just didn't know. For example, at that time we knew very little about scrub typhus, which hit a lot of our boys in the South Pacific with explosive force. Japanese scientists, on the other hand, knew a great deal about this disease, since it was also found in the Japanese Islands, and they had early contributed to discovering its vector, etiologic agent, and rodent reservoir. You must keep in mind that there were many places in the South Pacific where the white man hadn't been in years. One of the major reasons for creating a medical research unit was to get into such areas as soon as possible, so we could see what was what and protect our troops effectively and quickly.

Q: Dr. Rivers, during your survey of the South Pacific, you visited a number of naval hospitals. How would you say they compared with hospitals back home?

Rivers: They were damned good hospitals, as good, I would say, as any you could find in the States. They were staffed with excellent physicians and surgeons, had good technicians and nurses, and supplied high-grade medical care. I was surprised when I went into operating rooms and saw the techniques they were using. What surprised me most of all was the nursing. You see, nurses in the navy are officers and they don't nurse—hospital corpsmen nurse. A nurse may be in charge of an operating room but she never assists at an operation—hospital corpsmen do. I was just dumfounded. Of course, it turned out that the hospital corpsmen I saw assisting at operations were college graduates and, of course, they learned very rapidly. While I was at Fleet Hospital No. 5 at Noumea, I found two technicians who so impressed me that when I got back to the States I requested that they be allowed to join my research unit. By gosh, the navy sent those two fellows all the way back to New York—just in time for them to join my unit going to Guam. I was impressed by the hospitals that I saw, but none, so far as I remember, was set up to do research.

When I returned to Noumea, Captain A. H. Dearing, who was one of the senior medical officers in that part of the world, arranged for me to have a talk with Admiral Halsey. I spent a most delightful hour with him. Actually I was surprised that the old man gave me so much time. When I came in, I found him holding a book in his hand—it looked as if he had been reading, and we got down to business. I didn't say much. Halsey did all the talking. Apparently Captain Dearing had briefed him very well, and when I walked out it was pretty obvious that he was favorable to the idea of a medical research unit. Later I discovered that just before I went in to see Admiral Halsey he had been informed that his son had been downed in an airplane somewhere in the Solomons. Here he was, his son was missing, and I am sure he was suffering to beat hell, but he gave no outward evidence of it whatsoever. Thank God, the boy was later found alive and well. He had landed off a small island and was rescued by friendly natives. However, at the time Halsey was speaking to me, he didn't know that his boy was safe, and I didn't even know he was missing, because it was kept secret.

Q: Dr. Rivers, did everyone like the idea of a medical research unit?

Rivers: Heck, no. Although medical officers like Admiral Chambers and Captain Dearing were favorably disposed to the idea, there was one regular naval medical officer who didn't like it at all, and he didn't mind speaking his mind. His name was Commander James Sapero. Sapero was no ordinary medical officer. I should explain here that, before World War II, the navy trained its regular medical officers to take care of administrative problems and when war came always expected reservers to do the medicine and surgery. Except for a few bang-up surgeons who were trained at the Mayo Clinic and later placed on battleships where they needed good surgeons, most navy docs were administrators. Sapero, on the other hand, was one of these queer birds who was not only a good administrator—actually he wasn't a very good administrator, because he had a talent for making everybody mad—but a good doc as well. For instance, he knew a great deal about malaria and other tropical diseases. He had other talents. He was an excellent small boat sailor, and when we were on Guam he used to race sailboats with Admiral Nimitz. Sapero could even run a destroyer and on several occasions was allowed to dock one. I can tell you right now that medical men in the navy don't dock destroyers very often, if at all. These were some of the things that Sapero could do. Now let me tell you a story that gives the measure of the man.

Do you remember the naval battle which took place in the Solomons a short time after we invaded Guadalcanal when the Japanese bombed and sank a lot of our ships? Well, Sapero was on one of those vessels, and when the ship he was on was going down, a sailor came running up to him and said, "My buddy is wounded and I'm not going to get off this ship without taking him." Sapero went back with this sailor boy to where his buddy was, grabbed him, and jumped into the water. The wounded sailor had a hole in his chest, but that didn't faze Sapero; he stuck his thumb in the boy's chest and swam a mile to a raft. Eventually they were picked up. Sapero stayed with this boy until they got him to a hospital where I understand he made a good recovery.

This is the kind of fellow Sapero was. He was a rough and ready guy who could do a hell of a lot of things and who knew a great deal about what was going on in medicine. But he couldn't see why it was necessary to bring a research unit to the Pacific. In time, he became reconciled to the idea. As a matter of fact, we later became good friends, although it is true that we fought a great deal. Sapero, for example, always thought it was a waste of money for me to have the wonderful equipment I bought for my laboratories. Yet, after the war when he was appointed commander of NAMRU 3 at Cairo, Egypt, he didn't object very hard when some of that wonderful equipment was transferred to his command.

Q: Dr. Rivers, did you need the approval of Admiral Nimitz for the final sanctification of the research unit?

Rivers: Yes, and on my way back to the States I stopped at Pearl Harbor to speak with him. Here again the groundwork has been well laid by people like Captain Johnson and Commodore Anderson of the Medical Corps and I had no difficulty in persuading Admiral Nimitz of the advantages of establishing a medical research unit close to the fighting front. I remember that Captain Johnson personally took me to see Nimitz and Nimitz, like Halsey, gave me about an hour. He at that time had just returned from an inspection of Tarawa, where we had won a victory at the cost of a terrible beating. Admiral Nimitz was so shocked and saddened at the loss of life at Tarawa that he spent at least half an hour walking up and down in front of me and Captain Johnson getting this terrible sorrow off his chest. I think he was aware that medical people would take kindly to this kind of talk, whereas hardened combat people might not. He was an extremely kind-hearted person and was very reluctant to expose this side of his nature to anybody, except people he thought would understand.

Later when I went out to Guam, I saw a great deal of Admiral Nimitz, because his headquarters was on the island. The night before we invaded Iwo Jima I had dinner with him. There were I suppose about a dozen or fifteen people present that night, and during dinner Admiral Nimitz told all of us about the forthcoming invasion. It was obvious that he was worried about sending a certain number of boys to death and affliction the next morning. I know that he didn't get much sleep that night, because when we left to go home the old man went down to his headquarters where he stayed the rest of the night. Nimitz never said much. He was extremely shy and stayed away from newspaper people.

When I returned to the States in December 1943, I reported to Ross McIntire in Washington. I told him what I had seen and, after briefing him on my talks with Admiral Halsey and Admiral Nimitz, I urged the formation of a research unit. Subsequently Ross invited me down to Washington to talk with people in the Bureau of Medicine and Surgery, and early in January 1944 the Secretary of the Navy authorized the formation of Naval Medical Research Unit No. 2. I want to emphasize here that the navy took quite a gamble in organizing such a unit. First, no one in the navy had ever had any previous experience in organizing and running a medical research unit close to the fighting lines; second, no one had the slightest idea whether doctors and scientists could actually do scientific research under military conditions; and third, even if they could do such research, no one knew whether the results they would achieve warranted the existence of such a unit in a military force. About a month after the unit was authorized, I was put in command and told to get things rolling.

Q: Dr. Rivers, did you have any problems in organizing the unit?

Rivers: I must say that I had few problems, largely because the navy gave me a free hand in acquiring both equipment and personnel. As far as equipment went, I had a handwritten personal letter from Admiral McIntire to Admiral Mullholland, who was then in command of the Navy Supply Base in Brooklyn, that said, "Give Rivers whatever he wants." I want to tell you that I got what I wanted. I didn't have to wait for bids either. You know the navy had a rule which said that, if anyone ordered equipment above a certain sum, they had to wait until three bids were made and in the end had to take the lowest bid. I knew that certain apparatus I had previously used at the Rockefeller Institute was first rate, and I knew that it was exactly what I wanted for my unit. To be sure, there was a lot of stuff on the market that was similar to it, but I had no way of knowing whether it was exactly what I wanted or whether it would stand up. You don't have to be a wizard to guess that in the circumstances I bought what I knew to be tried and tested. No question was ever raised about what I bought or the amount of money I spent. I don't believe that anyone in the navy had ever had that privilege before, but as a result I wound up with some of the most beautiful laboratories you have ever seen. I can honestly say that I didn't throw the navy's money away, and my boys—not me because I personally never did any research on Guam—later did some first rate research in those laboratories. By the end of the war, NAMRU 2 published approximately 140 papers covering the fields of virology, bacteriology, entomology, and pathology.

I would like to say that the navy also gave me the same kind of freedom to choose my personnel—that is, with one exception. That exception rested in the fact that I was only allowed to choose but one half of my petty officers. Looking back, I would hardly call it an exception, because I was given permission to go through naval personnel files with a fine tooth comb and, with the assistance of Commander Sapero, I came up with about 125 of the best pharmacist mates, electricians, and carpenters that the navy had. I figured that, even if the navy assigned me a bunch of duds, that the boys I had chosen would more than offset them. It later turned out that I didn't even have to worry on this score, because the navy didn't shortchange me on the petty officers they finally assigned to my unit.

Choosing my officers was no problem at all. I had a completely free hand, and I chose investigators who had worked with me at the Rockefeller Hospital or whose work I knew about. Among them were such scientists and physicians as Dick Shope, Jerry Syverton, Lew Thomas, Francis Schwentker, Horace Hodes, George Mirick, Kendall Emerson, Marion Sulzberger and Harry Zimmerman. There were, of course, many more. If memory still serves, my complement of officers numbered forty-four men. There is one thing, however, that I would like to add here which has nothing to do with memory, and that is that this group was as topnotch a group of investigators and physicians as anyone could find.

Q: Dr. Rivers, was your second in command a regular naval medical officer?

Rivers: No. I chose one of my old boys, Francis Schwentker, to be my executive officer. I knew Dr. Schwentker intimately. During the early thirties he had worked with me at the Rockefeller Hospital on psittacosis, louping-ill and Rift Valley fever, and had demonstrated marked ability as an investigator. He moreover knew a great deal about infectious diseases clinically, and, even after he left the Rockefeller Hospital to go to the Harriet Lane Home in Baltimore, he continued to pursue these interests vigorously. In 1938 the International Health Board of the Rockefeller Foundation asked Schwentker to join John Janney in making an epidemiological study of scarlet fever in Rumania. I mention this because Schwentker was writing up a report of those investigations at the laboratories of the International Health Board at the Rockefeller Institute when war was declared in 1941, and soon afterward he joined the Rockefeller Hospital Naval Unit under my command.[6]

I must say that, as executive officer, Dr. Schwentker never let me down, and he played an important role in the administration of NAMRU 2 from the day it was activated until the end of the war. In the beginning he helped me order equipment and later joined me in planning the layout and interiors of all our buildings. At one point I even sent him and my construction officer, Ensign Edward Lee, out to Galesburg, Illinois, to alter the specifications in some buildings I had ordered. His work during this period contributed a great deal to

[6] F. F. Schwentker, J. H. Janney, and J. E. Gordon, "Epidemiology of scarlet fever," *Amer. J. Hyg.*, vol. 38:27 (1943).

the success we later had in setting up our laboratories on Guam.

It took us almost nine months to collect the material and personnel we needed for NAMRU 2. As a matter of fact, by the time we were ready to go into the field, the war had moved out of the South Pacific and since the purpose of the unit was to be as close as possible to the fighting lines, it was decided to locate NAMRU 2's headquarters on Guam instead of Guadalcanal as originally planned. The main body of NAMRU 2 didn't leave for the Pacific until late November 1944.

Q: Dr. Rivers, you say, "main body." Am I right in assuming that you sent advance parties into the field before November 1944?

Rivers: Yes. Soon after I took command, it became apparent to me that my unit would not be ready for duty much before the fall of 1944. I had to recruit personnel, I had to order equipment; heck, there were a thousand and one things that had to be done before we could do the job that we were set up to do. But in spite of that, I didn't think that it would be particularly wise to wait to do research until all my laboratories were neatly organized on Guam. There was, for example, an immediate and pressing need to deal with the problems of malaria and schistosomiasis, and I thought that the quicker we got down to work, the better off we would be.

Early in April 1944, approximately two months after NAMRU 2 was activated, I sent an advance group, headed by Lieut. Comdr. Herbert S. Hurlbut, Lieut. Bernard T. Travis, and Lieut. John D. Maple, to study methods of insect control in the Pacific. These boys got down to work immediately, and during the marine invasion of Peleliu in 1944 successfully introduced aerial spraying of DDT. It marked the first time that an airplane had been used for the dispersal of DDT under combat conditions. Several months later, they used the same techniques during the Okinawa campaign. The techniques were highly successful; only this time NAMRU 2 suffered its first and only fatal casualty when the plane Lieut. Maple was using crashed.

Q: Dr. Rivers, was this the only advance group you sent out?

Rivers: No, actually two other advance groups were sent into the field before NAMRU 2 reached the Pacific. One group headed by

Lieut. j.g. George Wharton, Jr., an acarologist, and Lieut. j.g. David H. Johnson, a mammalogist, were sent to study mite and mite bearing animals in relation to scrub typhus, which was then found in Bougainville, while another group directed by Lieut. Kenneth Knight and Lieut. Lloyd Rozeboom, both of whom were entomologists, were asked to study taxonomic problems relating to vectors of malaria in New Guinea and the Philippines. Again both these groups did excellent work and discovered new species of mites and mosquitoes that had an important bearing on the disease entities they were studying. I think it is fair to say that NAMRU 2 did a great deal of work in the Pacific long before its headquarters were set up on Guam. I would like to add here that my officers in the States were not idle during this period either. Some continued to work on problems of disease of interest to the navy at the Rockefeller Institute, others began to train technicians in specialized techniques that would be needed for the study of virus and rickettsial diseases, while still others began to collect information and to study specialized laboratory techniques that might be needed later. It was during this period, for example, that Lew Thomas and Jerry Syverton began working on a complement-fixation test for scrub typhus and developed a new method for staining rickettsia, all of which later proved helpful to us on Guam.

Q: Dr. Rivers, moving the unit to Guam must have been quite a headache.

Rivers: I don't know. In retrospect, I don't think that it was too bad. Our material and equipment were shipped on ahead to the West Coast long before we were scheduled to move out, and I had little or no problem about laboratory animals. I figured that if I got them in New York and shipped them across the country I would lose most of them. So instead, I called Karl Meyer at the Hooper Foundation in California and asked him to get me a stock of laboratory animals. You can depend on Karl, and when I reached the West Coast he presented me with a superb supply of different breeds of laboratory animals, which we later used as a nucleus for establishing breeding colonies on Guam.

My biggest worry was the men. We were stationed in San Francisco for almost three weeks before we set sail for Guam. San

Francisco is a wonderful city, but during the war it was not like it is today. The streets were jammed with soldiers and sailors and there seemed to be young women everywhere. These girls were not prostitutes. They were what we would ordinarily call good girls from good families. Hell, here were nice fellows going off to war, maybe to be killed, and these girls wanted to make their last hours in the States as happy as they could—and they did. There was a good deal of venereal disease in the San Francisco area, and I just didn't want my boys to come down, because it would complicate things. I was damn glad when we finally left San Francisco.

We reached Pearl Harbor without incident, and then we got stuck because the engines of the victory ship we were on gave out and had to be repaired. The delay put me in a dilemma because our supplies were on another ship, and that ship was proceeding without delay for Guam. I was afraid that if I didn't get some men out to Guam in a hurry I might not have any supplies by the time NAMRU 2 reached Guam. In wartime everything is fair game if you can get your hands on it. It is standard operating procedure. Well, I got hold of Dick Shope and Ensign Lee and sent them on ahead by plane to Guam to guard my supplies. Heck, I had refrigerators, alcohol, and a hundred other things that guys would have given their eyeteeth for, and I wasn't going to take any chances losing it. Dick was an excellent choice; he knows how to get along with people and how to get things done, and by the time I arrived in Guam, three weeks later, all my supplies had been unloaded, checked, and stored. Best of all, not a thing was missing.

Dick had made friends with practically all of the officers of the Third Marine Division and Captain José Perez and his staff of Fleet Hospital No. 111. He cemented relations with everybody before we even landed. A day or two after we reached Guam, we were given an uncleared piece of land between Fleet Hospital No. 111 and Fleet Hospital No. 103, and, in less than four months, with the aid of a naval construction battalion we cleared the land, set up 62 buildings, 12 complete laboratories, special wards, and provided facilities for water, sewage, electricity, and air conditioning. It was a hell of a job.

Q: Dr. Rivers, can you tell me about the early research that NAMRU 2 undertook?

Rivers: Well, of all the things in the world, the first and for that matter the best work that NAMRU 2 did while we were in the Pacific was on hookworm. As I mentioned earlier, one of the members of my unit was Norman Stoll. By profession Dr. Stoll is a parasitologist and a very distinguished one. Early in his career he had worked with Dr. Stiles in the southern United States and the Caribbean on problems of hookworm, and still later continued his hookworm studies in China. But I want to make it clear from the outset that I didn't take him with me because he knew about hookworm. I took him because he was an excellent general parasitologist. I thought that, since we were going to a part of the world that was heavily infested with parasites of various kinds, it would be a good idea to have a fellow like Dr. Stoll around. Well, the day we landed on Guam Dick Shope met us and told us that there were at least 75 to 100 Guamanian babies under a year old at the Agaña Hospital who were critically ill with hookworm. Some, he told us, had already died; others, he said, were on the point of death. I looked at Stoll and Stoll looked at me. "Dick, I said, "I just can't believe you; children that age just don't get hookworm." I as well as Stoll had been brought up with the idea that you got hookworm by walking around in polluted soil with your bare feet. Dick Shope is a stubborn guy and he is tough. "Look Tom," he said, "I know hookworm when I see it and I tell you these kids have got hookworm." I looked at Stoll and said, "Dr. Stoll break out a microscope from our supplies and go down to the Agaña Hospital and see if these kids have really got hookworm." Well he went, made his examinations and returned. "Dr. Rivers," he said, "It's a true bill of goods. I have never seen such infestations in my whole life. I have never seen anything like it before." Some of these kids were so infested that they had become anemic, while others had perforations of the intestines and died of peritonitis. Dr. Horace Hodes later developed a treatment which saved a good many of these kids.

The question of treatment was relatively simple; our big problem was to discover how those babies picked up their hookworm, and I want to tell you that it bothered us. One day some navy boys from one of the fleet hospitals turned up with stuff between their fingers that looked like ground itch. When an examination showed that they had hookworm eggs in their stools, we sat up. The interesting thing about these eggs was that they were of the oriental type rather than of the American type, and since these particular fellows had come directly from America to Guam it meant that they had picked up their hookworm on the island. When it turned out that they had been washing soiled blankets, underclothes, and shirts of patients at the Agaña Hospital, it gave us the idea that our hookworm might be fomites borne. Very shortly thereafter, Dr. Stoll demonstrated that hookworm eggs on soiled clothes would hatch, and that the larvae would develop to the infective stage in approximately five days if the clothes remained moist.

He then took a blanket that one of the infected babies in the Agaña Hospital had been sleeping on for only 24 hours and wet it, and after keeping it moist in the laboratory for five days was able to recover over 20,000 infective larvae, although the blanket gave no outward evidence of fecal contamination. The problem of transmission had been nailed down. Still later, we learned that before we liberated Guam, the Japanese had herded the native population out of the towns of Agaña and Agat into tent refugee camps. It was rainy, sanitary conditions were very primitive and poor, and when mothers had babies in these badly crowded and wet tents they kept them on planks held by two wooden horses. The quilts and other bedding used to cover these planks were always moist and in a very short time, without anyone being aware of it, became ideal hatching places for hookworm larvae. It seemed very clear after Stoll's work that this was the way the native babies on Guam got hookworm.

Prior to Dr. Stoll's research, fomites-borne infection in hookworm had never been observed, and I don't know that it has been observed since. It has always amused me that the first important contribution of NAMRU 2 should have been in such a prosaic and unexpected field. Before we leave the question of hookworm, I want to make one point. Although the Guamanians had hookworm, they were an intensely clean people. Please keep in mind that when we arrived on Guam, we found them living in unsanitary tent camps, with no water and no soap. Later, when they were left alone, you could always see them washing themselves and their clothes in the sea. Their virulent infestation of hookworm was a wartime phenomenon.

Q: Dr. Rivers, had there been any hookworm on Guam before the war?

Rivers: Oh, yes. Actually they had both American and oriental types of hookworm on the island. I have no doubt that the oriental type of hookworm was on the island when we acquired it after the Spanish American War. I suppose our sailor boys introduced the American type when we took over the island in 1898. However, prior to World War II, hookworm had been brought under control.

Q: Dr. Rivers, what was the relationship between NAMRU 2 and other hospitals on Guam? For example, did they ever ask you to do research on their clinical problems?

Rivers: Well, we had to pinpoint our activities. Remember, we weren't stationed on Guam to find out what was going on on Guam; our primary job was to discover what was going on just behind the battle lines. Of course, on many occasions we were tempted to wander and follow up things just under our noses, especially when it began to look as if we were going to win the war without too much delay. At such time, the temptation became very great indeed. There is, however, one thing that we did do on Guam. When we first came to Guam, I got hold of Harry Zimmerman and ordered him to do autopsies on everybody who died on the island. It made no difference who they were or where they died, whether in a hospital, hut, or tent—if they died on the island, they were autopsied. We found some very revealing things. For example, there was no evidence of arteriosclerosis on the island, though some of the people who had died were well over 80. I believe that in all the time we were on the island, only one case of hypertension was observed, and that in a person of mixed native and Spanish origin. There was no appendicitis on the island; as a matter of fact, only one person had an appendiceal scar, and that person had incurred appendicitis while she was in Honolulu.

Actually, there were only nine types of mosquitoes on Guam and no anopheles. I understand that later anopheles did get in, but that it was a poor transmitter of malaria. I don't know whether there are any anopheles there now or not, but when I was on Guam there was no malaria. Neither was there any rabies, although there were plenty of dogs on the island. Guam was a kind of little paradise. The navy, you know, didn't want to give it up, and I think that in some quarters there was a gnashing of teeth when the Department of Interior took it over. At the time I was there, the people of Guam were not citizens of the United States. Why the government didn't make it possible for them to become American citizens, I will never know, because in some respects they were a lot better than some of our American citizens living right here in New York.

Q: Dr. Rivers, what did people in this paradise die of?

Rivers: Tuberculosis. In peacetime tuberculosis was the major cause of death on Guam. However, when we arrived on the island in 1945, the major cause of death was hookworm. When we left Guam, tuberculosis again became the major cause of death. It was rampant throughout the island.

Q: Dr. Rivers, did NAMRU 2 later participate in any of the island campaigns?

Rivers: Yes, although in a way it was hard to get started. You see, when we arrived on Guam, it was too late for the navy to allow us to participate in the Iwo Jima campaign. However, long before our laboratories were established, I was informed by Commodore T. C. Anderson that I would be allowed to send a group of men to participate in the projected Okinawa campaign. There was nothing in writing; it was one of those things that are just understood. I made my preparations by putting everything into Dick Shope's hands. He was the logical man to put in charge. He was a ranking officer, he knew a great many officers of the Third Marine Division which was one of the units that had the responsibility of going into Okinawa, but, most important, Dick Shope is probably one of the finest investigators I have ever seen.

I have known Dick Shope for a very long time, and it has always seemed to me that Dick would no sooner start to work on a problem than he would make some fundamental discovery. It has never made one bit of difference to him where he was. He could make his discoveries anywhere. For instance, while we were on Guam, Dick isolated a substance produced by the fungus mold *Penicillium funicu-*

losum, which he called helenine after his wife Helen. The interesting thing about helenine is that it is one of the few agents isolated from a fungus mold that has some effect on viral infections particularly western and eastern equine encephalitis and Columbia SK virus. This was and is Dick Shope. Believe me when I say that he was right for this command.

To get back—in the end Dick organized a unit of about 10 officers and 20 men, outfitted it with mobile laboratories, jeeps, and supplies, and set sail with the invasion fleet for Okinawa. Although they arrived on D day, they were not allowed to debark until 12 days later. Once ashore, they set up their mobile laboratories just behind the fighting lines.

Q: Did you have any notion of what your men would find on Okinawa?

Rivers: No. Prior to the invasion, few white men had been on Okinawa for several years. We didn't know if they had schistosomiasis-bearing snails, or if there were any scrub typhus mites. We didn't even know if there were any snakes on the island. We knew absolutely nothing. One of the reasons Dr. Shope's unit was formed was to find out as quickly as possible what major medical problems we would have to face on the island. I must say that Shope's boys did their job efficiently and well, and they surprised us with their findings. Originally it was half expected that Okinawa would be a pesthole; instead it was found to be relatively healthy. For example, we discovered that there were few malarial infections in the native population. Did you ever see the picture I have of Dick Shope squatting beside a swamp on Okinawa? Well, that's testimony of the searching he did during the invasion for snails that were carriers of schistosomiasis. He never found any, nor were any mites capable of transmitting scrub typhus ever discovered. We had to look anyway, because we just didn't know. Although most of our findings in the end were essentially negative, they were none the less important, because they prevented the diversion of men and supplies for unnecessary disease-control programs. The research was not without excitement, because toward the end of the campaign, our mobile laboratories came under sniper fire. By early June, most of Shope's officers and men had returned to Guam.

During the campaign a number of army officers passing through Guam stopped off to see me to tell me about a new kind of scrub typhus they had found on Okinawa that brought men down with what they called Okinawa fever. Although at that time they hadn't yet found the mite, they were sure of the diagnosis, because they reported that the patients had the rash over the belly which was found in scrub typhus, and that some of their doctors had reported positive OXK agglutinations. When some of these "scrub typhus" patients returned to Guam, I called in Jerry Syverton of my rickettsial laboratories and asked him to do confirmatory OXK agglutination tests on the sera of these patients. Jerry had then been working on a complement-fixation-test antigen for the diagnosis of scrub typhus, and he set immediately to work. You never had to ask Jerry twice to do a thing, and a few days later he showed up and said, "Tom, all the OXK agglutination tests I have done are negative, but I have been having a look at those boys, and if you ask me I think they have typhoid fever." I asked Jerry to continue his investigations, and in a very brief period he established that 21 of the patients he examined had paratyphoid A fever, while three others had typhoid. I then had the pleasure of wiring my friend, Dr. Stanhope Bayne-Jones—he was then a general in the Army Medical Corps attached to the Surgeon General—"B-J., for Pete's sake don't send a scrub typhus commission to Okinawa, the boys have paratyphoid A and typhoid."

Q: Weren't the troops vaccinated against typhoid?

Rivers: Yes, they were, but they caught the disease in spite of it. They caught it because, they were overwhelmingly exposed to it. It just so happens that Okinawa is one of the filthiest places I have ever seen. There are few animals on the island, and farmers as farmers throughout the orient fertilized everything with night soil. Well our soldier boys would be crawling through patches and fields, get hungry or thirsty and pull out sweet potatoes and turnips, brush the dirt off, and eat them. At the same time, they would also be eating a lot of typhoid and paratyphoid bacilli that came from the night soil that fertilized the ground. Once we knew what the so-called fever was, it was easy enough to control.

Early in July of 1945, a number of cases of encephalitis were discovered among some of the natives on one of the small islands off Okinawa, and I immediately sent Lew Thomas to investigate the outbreak. In a little less than a week, Lew sent us paired sera from these patients, and two days after the sera arrived on Guam Horace Hodes established that the epidemic in progress was Japanese B encephalitis. My boys then really went to work; clinical and pathological studies were instituted, insects were collected in hopes of identifying the vector; host vectors were sought, and it was soon established that most of the horses on Okinawa possessed antibodies in their sera against Jap B encephalitis. Some weeks later, Dr. Hodes and Dr. Hurlbut were able to demonstrate the transmission of Jap B encephalitis to infant mice by means of bites of *Culex quinquefasciatus*, *Culex jepsoni*, and *Aedes vexans* mosquitoes.

It was during this incident that I had a hell of a row with Albert Sabin. At the time Sabin was in the army and doing a lot of work for the Army Neurotropic Virus Commission. In 1943 he and John Paul were sent to the Middle East to investigate polio, sandfly fever and dengue. I expect that it was during the course of this work that Sabin first learned about sandfly fever and dengue. As I have said before, he is a good scientist and it didn't take him long to make himself familiar with these diseases. As a matter of fact, when I later came to edit my volume on *Viral and Rickettsial Infections of Man*, I asked Sabin to take responsibility for the chapters on sandfly fever and dengue.

Well, one night after we had established that the outbreak on Okinawa was Jap B encephalitis, I don't remember the exact date, but it was toward the end of July 1945—I received a phone call from the Agaña airfield. It was Albert Sabin. "Dr. Rivers," he said, "Can you come down to the airfield to see me?" "What about?" I asked. He replied, "I am on way up to Okinawa to look into this outbreak of encephalitis and find out what's causing it. I'd like to talk with you about it." "Young man," I said, "it's two o'clock in the morning, and

I am not going down to the Agaña airfield to see you or anybody else. Besides, I already know what virus is causing the encephalitis." Do you think that stopped him? "Dr. Rivers," he said, "Can I come over to your place? I have to wait here for two or three hours." "Dr. Sabin," I said, "you can't come over to my place because I am going back to bed." That ended the conversation and Sabin went on to Okinawa.

Several days later I was sent to Okinawa and I did get to see him. As a matter of fact, he was responsible for my trip. It seems that the minute Sabin landed on Okinawa he tried to persuade everybody in sight[7] that they ought to be vaccinated against encephalitis with a formalin-killed vaccine that he had made in mice. When the navy heard about this, Commodore Anderson at Nimitz's headquarters called me and told me to "get the hell up to Okinawa and find out what's going on." When I asked him when I would receive my orders, he said, "Dr. Rivers, I want you to go immediately, write your own orders."

I'll never forget the look of the duty officer at the Agaña airfield when I handed him my orders. "Captain Rivers," he said, "I can't let you on the plane; these orders are no good. You can't write your own orders." "Well," I said, "if you don't think these orders are any good, why don't you call Admiral Nimitz's headquarters and find out?" He made the call and ten minutes later came back shaking his head. "I guess they are all right," he said.

When I got to Okinawa I got hold of Major Downs, of the Army Medical Corps, and Dr. Sabin, and I told Sabin. "Look, I can't tell you or the army what to do. As far as I am concerned, the army can do what it damn pleases. But I am going to tell the navy to let your vaccine alone. There is no proof that it is worth a damn, and I am not going to have boys go to the trouble of taking that damn stuff when it probably isn't worth anything." I later advised the navy to stay off the vaccine and they did. I don't mind admitting to you that I was kind of sore at Sabin. In the first place, no one in the army or navy had at that time come down with encephalitis, and there was

[7] Dr. Sabin notes here, "This was a decision taken earlier by the Preventive Medicine Division of the Surgeon General's Office of the Army. The vaccine had been prepared for just such an emergency" (private communication).

ample evidence that they wouldn't.[8] It was apparent that the outbreak was localized among the natives, and I thought that we ought to leave well enough alone. Sabin did give his vaccine to the army boys, but no one has ever proved one way or another that it was useful in preventing an encephalitis outbreak among those troops.

Not all of the research that NAMRU 2 did was as useful as the work done on encephalitis and hookworm. Some of it was routine and a damned pain in the neck. Once I received a request from headquarters to determine why the beer on Ulithi tasted so bad that the men would not drink it. I put Kendall Emerson, who was in charge of the chemical laboratories, on that problem, and he found that the bad taste was due to imperfections in the lining of the cans caused by exposure to heat. At the time, this kind of work seemed very important to some naval officers, and when I got several other similar requests I called headquarters and made it clear that, except under the most unusual circumstances, I would not allow my officers to do such routine jobs. I can tell you that from that time forward we weren't bothered very often with that kind of request.

The last important investigation that NAMRU 2 engaged in under my command occurred several months after the war ended when I received a request from the command in Tokyo to investigate an outbreak of hematuria among sailors stationed on ships in the Wayakama anchorage in Japan. I detailed Dr. Beach M. Chenoweth, Jr., and Dr. Albert Harris to Japan to determine the cause if possible. It didn't take them long to demonstrate that the cause of this trouble was due to picric acid in the drinking water. In searching for the source of the acid, they discovered that approximately 100 tons of the acid had been dumped into the ocean about fifteen miles from the Wayakama anchorage. Drinking water on ships is made from sea water by means of evaporators. It was clear from the investigation

[8] Dr. Sabin comments here, "There were already some cases among army personnel, and, because of the complete lack of immunity among Americans, there was reason to expect more" (private communication).

Ed. note: Later events did not confirm Dr. Rivers' statement that in 1945 it was not necessary to experiment further with vaccination because no one in the army or navy had at that time come down with encephalitis and that there was ample evidence that they wouldn't. Cf. W. D. Tigertt and W. M. Hammon, "Japanese B encephalitis: A complete review of experience on Okinawa 1945–1949," *Amer. J. Trop. Med.*, vol. 30:689 (1950).

that in this case the tides had brought in some of the acid, and being a volatile substance some of it was carried over when the sea water was being distilled.

Q: Dr. Rivers, did you ever get to Japan?

Rivers: No. But just before the surrender in Tokyo Bay, I was asked by Commodore Anderson—he was a medical officer—if I wanted to be present at the surrender. I was told that I would be on the flagship, that I would have a stateroom to myself, and that in general I would be a bigwig. It was a tremendous temptation, but I turned it down. I had at that time a young entomologist in my unit named Linsely R. Gressitt. Gressitt's father was a missionary and a prisoner of the Japanese. He himself had spent the first twelve years of his life in Japan, knew the islands exceedingly well, and could speak and read Japanese. As a matter of fact he could also read Chinese and spoke several Chinese dialects. I told Commodore Anderson, "If you really need someone who can be of help to you, why don't you take Lieut. Gressitt? I can tell you now that, although he is only a j.g., he will be a lot more service to you than I can, with all my rank." I didn't have to argue very hard to persuade Commodore Anderson. He took Gressitt, and, as I thought, the boy was a tremendous help. He rounded up American prisoners, made contact for the navy with important Japanese officials, and interpreted. Best of all, he found his father in a prison camp and had the privilege of being with him before he died. I never regretted making that recommendation.

Q: Dr. Rivers, how long did NAMRU 2 remain on Guam?

Rivers: I got to Guam the 12th of January 1945 and left the following year on January 6, 1946. I and most of my unit were on Guam for at least a year. When I left, Commander Harry Zimmerman, who was the ranking officer, took over. I can't tell you now just how long NAMRU 2 remained active. After I left there was some talk that the navy would dredge the harbor at Guam and establish a large medical center on the island and that NAMRU 2 would be the core of this new medical center. It was just talk though, because when the boys in

Washington got down to estimating the actual cost of dredging the harbor and putting up this new medical center the matter was dropped. I believe that NAMRU 2 was decommissioned sometime in 1946; however, it still exists today. Several years ago it was recommissioned, and when I last heard it was stationed at Taipeh on Taiwan and was commanded by Captain Robert Phillips. Strangely enough, Phillips used to work at the Rockefeller Institute under Dr. Van Slyke and was a member of my original Rockefeller naval unit. He went into the navy as a reserve officer, but after the war he decided that he wanted to remain in the navy and become a regular naval medical officer. I correspond some with Phillips and I understand that NAMRU 2 today carries on pretty much in the tradition we established on Guam. While they do not have so large a unit as I had on Guam, they have been most successful in combating disease in southeast Asia.

Q: Dr. Rivers, what impact would you say the war had on you and the Rockefeller Hospital?

Rivers: That is a difficult question to answer, because I have never really thought about it. Let me say that, although the war ended in August 1945, my activities and the activities of the Rockefeller Hospital with regard to the war did not come to an end before July 1, 1946. I cite this date because it was then that we stopped admitting naval patients to the hospital. Actually most of the naval patients were out of the hospital before that date, though we did keep one or two cases who had bad hearts as a result of rheumatic fever several months longer.

I believe that the war did have some influence on the postwar activities of the Rockefeller Hospital. During the war Dr. Hoagland's laboratory, which was originally set up to study muscular dystrophy, shifted its research to infectious hepatitis. Unfortunately, Hoagland died in August 1946. Now, when the head of a laboratory at the Rockefeller Institute dies, there is no rule that says you have to carry on his laboratory and work. However, I thought Dr. Hoagland's work important, and I just didn't want to close it out. I looked around and finally asked Dr. Daniel Labby, who was then working at the hospital, if he would take over the lab. I think that he would have, but his wife who came from the West Coast was anxious to return home, and he turned me down. I then offered it to Henry Kunkel, who was then a member of the Rockefeller Hospital naval unit. I was a little surprised to find him hesitant. "Dr. Rivers," he said, "I'd like to, but I have just accepted a job with McGehee Harvey at Johns Hopkins." "Look Kunk," I said, "You are only an assistant now, but you will be head of a laboratory, and I promise you that you will be given every opportunity to become a member of the Institute. I want you to come, but I won't take you unless Dr. Harvey will release you. Why don't you go down to Baltimore and tell McGehee Harvey what I have offered you. If he will release you, come back; if he won't I am afraid you will have to stay in Baltimore."

Kunk went to Baltimore, saw McGehee Harvey, and when Harvey released him I took him. Dr. Kunkel was an exceedingly young man for this job, but from the beginning he functioned well. In 1951 he went to work with Arne Tiselius in Uppsala, and when he returned to the hospital in 1952 he quickly demonstrated that he had mastered electrophoretic techniques. His biophysical approach to disease has, I think, in part determined his research interests. Although he began his work on infectious hepatitis, in time he drifted to a study of cirrhosis of the liver, first to the type that is usually seen as a result of poor nutrition following an excessive use of alcohol, later to a type that he discovered and described as occurring for the most part in young women without any obvious reason. Still later, he began to study abnormal proteins in the blood, and today is absorbed in studying this phenomenon in relation to rheumatoid arthritis. Kunk, of course, has always been allowed to follow his nose, and he has. And I must say that the route he has taken has turned out to be a very profitable one indeed. He has never disappointed me in any way, and in 1952, some years before I retired, I saw to it that he was made a member of the Institute.

Q: Dr. Rivers, after the war did you feel that you wanted to get back to your laboratory?

Rivers: Yep, I did. But how I felt made no difference. During the war my laboratory gradually fell into Frank Horsfall's hands, and when I came back I didn't think that it was right or fair to kick Dr. Horsfall out. He was obviously very much in love with studying viruses, and so I let him be. Who am I kidding? By 1946 my days as an investigator were over, and, besides, after the war I had to undertake the responsibility of overhauling and rebuilding the Rockefeller Hospital. In 1946 medicine had outgrown the hospital structure that was put up in 1910, and I had to spend a great deal of time planning how I would renovate it. One of my biggest jobs was adding a new wing.

Q: Dr. Rivers, I would like to concentrate on the problems you faced in planning for this new wing.

Rivers: When you build a hospital, you must take into consideration not only what is going on now in medicine but what will be going on 15 years from now. You cannot only consider your own interests; you also have to take account of the interests of some future director whom you don't even know. In other words, you have got to plan a building that can function under many conditions. You cannot build too specifically or your building will be obsolete before too many years. Right across the street from the Rockefeller Hospital, there is an object lesson for every hospital planner in the country. I am talking about the New York Hospital. When it was put up in 1932, it won an architectural prize because it is beautiful in form, but some of the docs were ready to pull it down from the first day, because it really wasn't fit for certain kinds of medical procedures.

Q: Dr. Rivers, did you call in anybody to help you with the planning?

Rivers: I called nobody, because the more people you call in the bigger mess you get in. No two people agree, and one man has to take the responsibility. Well, I say I called in nobody; what I mean to say is that I called in no doc. I had an architect, a good one, and I had a good business man, E. B. Smith, who was the business manager of the Institute. Mr. Smith knew his way around money, and the architect knew his way around structure, and these two, bless their souls never let me get out on a limb architecturally or financially. Outside of that, I took the entire responsibility for overhauling the hospital.

Q: Dr. Rivers, how did the new wing differ from what you had before?

Rivers: Previously the isolation ward was separate from the rest of the hospital. It was actually in a different building. I had long been interested in infectious and contagious diseases, and I was firmly convinced that such diseases could be handled right in the middle of an ordinary hospital, provided the proper facilities for isolation were built into the hospital. I wasn't the only person who believed that; others had expressed such beliefs long before I renovated the Rockefeller Hospital. The great difference is that I was one of the first to have the opportunity to put my ideas into practice.

When the new wing was put up, each room was individually ventilated and each room had a negative pressure relative to the hall. In other words, if you opened the door to a room, instead of the air from the room going out into the hall, the air from the hall would be sucked into the room. Under these circumstances, it would be difficult for contagious material like measles and chickenpox—which can be easily spread by air—to get into the hall. There aren't many diseases that easily spread by air. Of course, as far as streptococci go, the important thing is to be sure that nothing is carried on your hands, or on objects, from one room to another. In addition, the air in each room came in under a special opening which was built under the window. This air was then drawn out the other side of the room by a special vent, which did not mix with other vents until it got to the roof of the building. In the circumstances, there was no chance for the air from one room to go back in, down the vent, to any of the other rooms. This special ventilation was the most expensive feature of the building. I should add here that my new wing cost approximately three million dollars.

Q: Dr. Rivers, how about lab space?

Rivers: I was bitterly opposed to having laboratories and animals in a hospital building. In the first place, laboratories are dangerous. Explosions can take place, fires can take place, and they do, more frequently I think, in laboratories than in bedrooms or living quarters or hospital rooms. I saw no reason why the Rockefeller Hospital should have this extra fire hazard, and I didn't make any arrangements for any laboratories in the hospital.

Q: How about the fellows on your hospital staff who were interested in research. How did they feel about that?

Rivers: I didn't ask them. I didn't see any use in stirring up a lot of conversation when I knew damn well what I was going to do. Hell, I was the boss. I don't know how they felt, because they never expressed themselves to me. I have always suspected, however, that they expressed themselves pretty strongly behind my back. But they were not too obstreperous. Remember the hospital had plenty of laboratory space in Founders Hall. There were connections on the third floor between the hospital and Founders Hall. I fully expected my boys who had patients to do laboratory work, but I expected them to do it in Founders Hall and not in the hospital building. Actually they had more laboratory space in Founders Hall than I could have given them in the hospital.

This concept has been altered since I left the Institute. Today the whole fifth floor, which once served as the residence for hospital physicians has been transformed into a floor of magnificent laboratories for Dr. Edward Ahrens and Dr. Jules Hirsch and their associates. On the seventh floor another suite of laboratories has since been built for Dr. Alexander Bearn and his coworkers. So my idea of having a hospital building without any laboratories in it does not exist. I think it's wrong, because I still think that the hospital is not as safe (so far as fire and explosion are concerned) as it would be without the laboratories in it.

Q: Dr. Rivers, when you were renovating the Rockefeller Hospital, did you have any plans to establish any new department?

Rivers: You know, in the beginning I left the seventh and eighth floors of the hospital unfinished, because I hoped eventually to establish a department of psychiatry. It was not a postwar idea with me.

When I took over the hospital in 1937, I had several long talks with Dr. Gasser about such a department, and he agreed with me that it should be done. My big problem was that I could never find the man I wanted to run it.

Now I am going to tell you something that on its face may sound a little odd. I don't like psychiatrists. It is not a personal matter, because personally I like some of the psychiatrists that I know very much. I just don't think much of them as docs. That sounds like a contradiction, wanting to establish a department of psychiatry and not liking psychiatrists; well, it isn't. My plan for psychiatry was to establish a department where my doctors would be willing to look upon mental disorders the way they looked at diseases of the liver, lungs, and kidneys: that is, in physiological, biochemical, or biophysical terms. It should be clear from what I have said that I don't think much of psychoanalysis, and I regard Freudian talk about egos and ids as just so much mysticism.

You know, a lot of psychoanalysts bristle when I say you ought to look upon mental disorder as you do diseases of the liver. They think I am sacrilegious. They talk endlessly about personality as if it were something sacred. Well I don't understand words like personality. Your personality is made up of your gonads, your brain, your internal secretions, and whether you were drunk last night—you know your surroundings. Heck, if your liver doesn't put out bile right, you get sour; if your kidneys don't function you get uremic and, believe me, in such a state you can become psychotic as hell. The brain is a wonderful organ, and everything it does is physiological and biochemical. When we learn to explain what the brain does in these terms, I think we will have taken a long step in treating mental diseases. To be sure, we are not yet able to explain all of the brain's activities this way, but as sure as I am sitting here someday we will.

Starting a department of psychiatry on the principles I have sketched is one of the things that I wanted to do, and when Lew Thomas came to work at the Rockefeller Hospital I thought I had found my man. Lew had ability in many fields: he was a first-rate pathologist, did excellent work in virology, had interest in neurology —in a word he was an all-round investigator. When the war began he joined the Rockefeller Hospital Naval Unit and eventually went out to Guam with me. I gave him plenty of work and kept a close watch on him. I don't have to tell you that I liked what I saw, but, damn it, so did Schwentker. Unfortunately, Lew left Guam before I did, and when I got home I found that he had taken a job with Schwentker in the Department of Pediatrics at Johns Hopkins. I always regretted that, because, if Lew Thomas had been available when I came home, I would have established a department of psychiatry at the hospital. He was the one guy I thought could run such a department the way I wanted to. I very early saw in him future Rockefeller Institute membership material. I wasn't wrong either. From the Hopkins he went to an associate professorship of medicine with a specialty in infectious diseases at Tulane. Later he was given a special professorship at the University of Minnesota Medical School, and still later came to New York University Medical School, first as professor of pathology and then as professor of medicine, the position he holds today.

Q: Dr. Rivers, telling me your dream about a department of psychiatry reminded me that, beginning in the mid thirties, Dr. Alan Gregg of the Rockefeller Foundation did a good deal of proselytizing for developing psychiatric studies and I wonder if you ever had occasion to talk with him about your plans.

Rivers: Well, I had plenty of occasions, but I didn't talk to him, because Alan was pretty well sold on psychiatry as it was. I never was one to try to influence people too much. He had a job with the Rockefeller Foundation, and I had a job with the Rockefeller Institute. I was pretty sure that, had I discussed this with Alan, Alan would have disagreed with me, and I am pretty sure that I would have disagreed with him, and, being certain of this I figured, why waste time? I respected Alan's opinions, but I didn't always have to agree with them, and I know he felt the same way about me. No matter, I never did establish my department of psychiatry, and today the seventh floor of the Rockefeller Hospital is given over to Dr. Bearn's laboratories, while the eighth floor contains offices, a suite of rooms for the physician on duty, and a very nice setup in occupational therapy.

Q: Dr. Rivers, in 1946–1947 did you expect a diminution in clinical work at the hospital?

Rivers: No. We had a lot of clinical work at the hospital during the war, and I expected it to continue after the war. It has. However, the hospital has not always been as crowded as it was during the war. Crowding comes by fits and spurts. A doctor may become interested in a certain phase of a disease and suddenly he will want a lot of patients in to prove a point. When he has proved or disproved the point, his interest in patients may lag for a time until he gets another brain wave. This is what you expect in a research hospital. The Rockefeller Hospital is not like the New York Hospital. The New York Hospital is always full and it never has any vacant beds. If you are a patient there, you usually have to wait until somebody gets out, unless of course you are an emergency. At the Rockefeller Hospital, the pattern is quite different. Here it is the problem that the doctor is working on that determines which patient comes in, how long he stays, and when he leaves. It doesn't matter whether we are full or not, because patients at the Rockefeller Hospital do not pay fees. Research determines the patient census, and that is something you can never anticipate.

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,