CHAPTER 1

Beginnings

CARTER: *Be both your legs bedfellows every night together?*
FAUSTUS: *Would's't thou make a colossus of me that thou asketh me such questions?*
CARTER: *No, truly sir, I would make nothing of you, but I would fain know that.*

Christopher Marlowe, *Dr. Faustus*

Q: Dr. Rivers, I wonder if you would tell me something about your parents.

Rivers: My father was named Alonzo Burrill Rivers and was the son of Burrill Greene Rivers, who lived in Milner, Georgia. My father was born either just before or during the Civil War. My Grandfather Rivers was in the Southern Army and received a wound which did not incapacitate him.

My mother was named Mary Martha Coleman and was born in Henry County, near McDonough, Georgia. Later my mother's family moved to Jonesboro and bought a farm and a home there. In addition to running a farm, they also ran a hotel in Jonesboro, and it was very close to the railroad station where my father-to-be worked. This is the way my father met my mother.

Q: I take it the railroad was the Georgia Central.

Rivers: My father worked for the Central of Georgia Railroad as a railroad telegrapher. However, he got out of the railroad business when the Central of Georgia people insisted that he leave Jonesboro

and go to Jessup, Georgia. My mother, on the other hand, said that she would not marry him if he went to Jessup, since she didn't have any idea or inclination to live there. In the end my father gave up his job with the railroad and got a job with a Mr. Hines who had a big warehouse and cotton business in Jonesboro. Then my father and mother got married.

There was an older brother named Coleman Rivers who died when he was twelve years old.[1] I was born on the 3rd of September 1888 in a nice comfortable farmer's home. The house was actually within the town limits of Jonesboro, and some of the farm was within the town limits. Jonesboro is a very old town and is actually much older than Atlanta, which is close by. Atlanta is a kind of an upstart. Originally I think it was called Marthasville or Marysville, that is, before the Georgia Railroad and the Central of Georgia joined or met there. At that time, the town was renamed and was called Atlanta. It has since

[1] Just before Rivers entered the Navy during World War II, he discovered that he did not have a birth certificate, and he wrote to his mother for a notarized statement about the date of his birth, so that he could complete his application form. His mother sent the date with the following autobiographical letter:

February 24th, 1942

Dear Tom,

Dear Son,

I am sending you a few dates. You might like to put them down. Alonzo Burrell Rivers was born in Pike County, Ga., June 26, 1859, and died Sunday morning Jan. 18, 1918, in Jonesboro Ga. Mary Martha Coleman was born in Henry County, Ga., Sept. 17th, 1863. We were married Nov. 28, 1882, in the Methodist Church in Jonesboro, Ga. in the afternoon. Rev. T. S. L. Harwell was the preacher, I was nearly 15 when I joined the Church in Jonesboro. Rev. John Bowden the preacher. Your brother, Alonzo, joined the Methodist Church in Jonesboro May 8, 1899. Rev. J. W. Stipe, the preacher. He was 6½ years old.

You were nearly 5 years old when you joined with Coleman. Dr. Blosser, the preacher. As you grow older you can't remember all these things unless you have it written down. You remember you and Alonzo stood at your father's grave in your uniforms of the first world war. You were already in service and left the next day for Texas. Now you are in again. God bless you! I know you will give the best and all you are in the service as you did before. I am proud of you and always have been and will be. We and all we are and ever will be is God's anyway. So let Him use us. You are a great son and have no regrets as to me or any of us. You are dear to me and I love you. I give you willingly to your country. Bye bye,

MOTHER

Although Dr. Rivers would not speak to me of his family, all his associates at the Rockefeller Hospital speak of his close attachment and devotion to his brother, Alonzo, a businessman who died sometime in the 1950's, and his sister, Mrs. Sarah Turner, who still lives in Jonesboro, Georgia.

grown, of course, much more rapidly than Jonesboro. Jonesboro now is a suburb of Atlanta, and even though it's 20 miles from Atlanta it is spoken of as being a part of the metropolitan area around Atlanta.

Father worked at the warehouse, and eventually he bought out the business. Later he bought the farm from my mother's father and became a farmer as well. His interest in the cotton business led him to build a large gin house that could gin 100 bales of cotton a day. Still later he became a representative for a cottonseed oil business and bought cottonseed for half of the state of Georgia. He also became a salesman for one of the big fertilizer companies in Georgia. So my father was a fairly busy man, running a farm, a warehouse, and a gin, and traveling for a cottonseed oil company and a fertilizer company.

I had a very happy time as a child, because the farm where I lived was nice and had all the nice things that go with farms, and still I was within walking distance of the schoolhouse and walking distance of the town.

Q: Was it a one-room schoolhouse?

Rivers: No. The schoolhouse was quite a large one. It was two stories tall. I don't quite remember how many rooms it had, but I believe it had at least eight. Actually, I think the schoolhouse may have been built before the Civil War. It was certainly built before my time. At one time I know they spoke of it as the Middle Georgia College. It might have been an academy, but for the most part, when I went to school there, it was just an ordinary school, with a principal and two or three teachers, all paid for by private tuition, no public money. During my time they did take a try at having the Middle Georgia Military Academy there. I didn't participate in the activities of the military part of the academy, although a lot of the boys bought uniforms and drilled and had this, that, and the other. I never could see much in that. That lasted for a couple of years, and then the Middle Georgia Military Academy disappeared.

The schooling we received wasn't too good, and at times it was so bad that Father got a tutor for the children. Mrs. Land, who was a well-educated lady who lived in Jonesboro, tutored me for about two years before I went to college.

When I got to be sixteen, my father thought that it was about time I went to college, because it didn't seem that there was much more education to be had in Jonesboro. It was decided that I would go to Emory College. The college had this to recommend it: it was run pretty much by the Southern Methodist Church, and my family was Methodist, and it was not too far from home. However my schooling had been so bad that I couldn't enter any higher than subfreshman, and naturally it took me five years to graduate.

I missed four days from school in my subfreshman year, but in the next four years I never missed a class, nor was I late for any class, the whole time.

I took the A.B. course at Emory and received a general education. In order to take an A.B. at that time, you had to take an awful lot of Latin, an awful lot of Greek, and an awful lot of mathematics. In all frankness, I didn't particularly like any of it. I could put up with the mathematics, but I didn't do well in it. I didn't really get much fun out of college until I became a junior. I should explain that, in the A.B. course, you were pretty well told what subjects you could take until the junior year. But in your junior and senior years, you could select subjects of your own choice. I chose as much biology and chemistry as I could.

Q: What made you choose those subjects?

Rivers: Dr. Duncan who had charge of the courses in biology was an old bachelor who lived at the same boarding house that I did, and we ate at the same table. He had a great deal of influence on me and helped persuade me to choose electives in biology and chemistry. Anyway, I did. I liked the work and became extremely interested in that area, and soon after I decided that I wanted to be a physician. I think that if I hadn't taken these courses in biology and chemistry that I might have gone back to Jonesboro and helped my father. My father was not very well at the time, and he was very anxious that I go into business with him. I didn't, and he proved willing for me to go to medical school. I stood first in my class at Emory. The time that I was there, my average for the five years was higher than any average that had ever been made at the college, and I graduated summa cum laude.

I had no difficulty in getting into Johns Hopkins, even though I came from a small southern college.

Q: What made you choose the Hopkins?

Rivers: It was about the only one I knew. You know, I lived in the South, and I didn't hear much about Harvard and Yale and such places. But I had heard of the Hopkins from Dr. Duncan, and he seemed to think it was just about the best school there ever was, and so, naturally, not knowing very much about the others and having heard a lot about this one, the choice was very easy.

Q: Dr. Rivers, did you see much disease in Georgia as a young man? I know that in South Carolina Dr. James W. Babcock was doing some very interesting work in pellagra, and I daresay you must have seen some pellagra in Georgia.

Rivers: We had pellagra in Georgia. Many of the southern states had it. Pellagra, as you know, is a food-deficiency disease caused by a lack of nicotinic acid. However, some of the ideas about the cause of pellagra held by southern physicians in my youth were rather curious and unique. Dr. Stuart Roberts, one of the physicians I knew in Atlanta, I believe at one time wrote a book showing that the eating of cornbread was the cause of pellagra. And he wasn't the only one who believed that. Others believed it too. I'm afraid Dr. Roberts—I knew him very well—didn't know how to go about a scientific investigation, and I'm not surprised he arrived at the wrong conclusions. Dr. Joseph Goldberger of the U.S. Public Health Service did know how to experiment, and he and Dr. Claude Lavinder in a beautiful series of experiments demonstrated the true nature of the disease. Actually, I shouldn't run Stuart Roberts down too much, because he was pretty close by marriage to our family.[2]

[2] Rivers' memory does not serve him well in this instance. Although Claude Lavinder of the United States Public Health Service played a key role in arousing medical interest in pellagra in the United States, he did not work with Joseph Goldberger when the latter engaged in his pellagra investigations. Dr. Goldberger's associates, among others, were David G. Willetts, C. H. Waring, G. A. Wheeler, and Edgar Sydenstricker. For further information see C. H. Lavinder, "Pellagra, brief comments on our present knowledge of the disease," *Public Health Repts.* (*U.S.*), vol. 28:2461 (1913); J. Goldberger,

Another disease we had plenty of when I was a boy was typhoid fever. In 1900, when I was twelve years old, I had typhoid and stayed in bed from the 26th of June until the 15th of October. The treatment was heroic: it was starvation and freezing. From time to time they also gave me something called peptinoids. I don't know what it was, but it sure was awful tasting stuff; it was supposed to be nourishing. Actually, I had very little food, and I used to dream about food. Ever so often, they would wrap me in a sheet with ice around it to bring the temperature down. Of course, by the time I got to Hopkins and was a student there and later an interne, they were not starving typhoid patients; they were actually feeding them. Some of the patients I had as an interne gained weight while they were having the disease. Instead of getting ice cold baths, they were giving tepid baths. That kind of treatment was much more sensible and much more gentle than the kind that I got.

Q: Did you ever have hookworm?

Rivers: The Rockefeller Sanitary Commission[3] had not yet made the famous hookworm survey of the southern states when I was a boy so I really don't know whether I ever had hookworm or not. But as a boy I went barefooted every summer up until the time I went to college, and I had ground itch. I have been told that ground itch is caused by the larvae of the worm getting beneath the skin. Well, I had ground itch in between my toes, and so I suppose that I had

C. H. Waring, and D. G. Willetts, "The prevention of pellagra. A test of diet among institutional inmates," *Public Health Repts. (U.S.)*, vol. 30:3117 (1915); J. Goldberger and G. A. Wheeler, "The experimental production of pellagra in human subjects by means of diet," *Hyg. Lab. Bull., No. 120*, 7–116 (1920); E. Sydenstricker, "The prevalence of pellagra. Its possible relation to the rise in cost of food," *Public Health Repts. (U.S.)*, vol. 30:3132 (1915). These are by no means all of the papers produced by Dr. Goldberger and his associates on the problem of pellagra and are only representative of two facets of his early work.

Rivers' strictures on Stuart Roberts are not completely fair. Dr. Roberts' later observations that pellagra sometimes occurred among well-to-do people because of eccentricities in diet were not unimportant. See S. R. Roberts, "Types and treatment of pellagra," *J. Amer. Med. Assoc.*, vol. 75:21 (1920).

[3] The Rockefeller Sanitary Commission (1909–1914) was established for the purpose of eliminating hookworm from the southern United States. An account of its work is furnished by R. B. Fosdick, *The Story of the Rockefeller Foundation*. Harper & Brothers, New York, 1952, pp. 30–43.

hookworm. However, by the time I got old enough and went places where somebody would know what hookworm eggs looked like, I didn't have hookworm. I think I've been fairly active, enough so that no one would accuse me of being lazy because of hookworm.

Q: Was the transition from Jonesboro and Emory College to Baltimore and the Johns Hopkins difficult?

Rivers: It was not too easy. The boys from Harvard, the boys from Yale, and the boys from other old and well-established institutions, such as Princeton, didn't think much of us boys that came from the South, and more particularly from a small college that had only 350 students. They didn't think much of us, and they used to make an awful lot of fun of us. Well, it didn't bother me very much. But it did worry some of the boys. Some of the northern boys were rather rough on the poor southern boys. I didn't mind that very much, because I was fairly strong physically and could take care of myself. However, it was hard to get into certain of the medical fraternities unless you came from the right family or knew someone. And you must remember that fraternities at least had, as one of their functions, providing an eating place. Well nonfraternity boys had to eat too. Luckily there were some nice women along North Broadway that were perfectly willing to provide meals for nonfraternity boys—an Aunt Dora Gordon was one of them. Aunt Dora set a table with the most delicious food, and it cost us $1 a day for three meals. It makes my mouth water yet to think about getting all that delicious food for $1 a day.

Later, after I graduated and got out of the army after the First World War, I had a room and facilities for bathing (there was not a private bath) at Aunt Dora's for $8 a month.

Q: Where did you room as a student?

Rivers: Part of the time I roomed at 424 North Broadway, with people named Harrison. They were an old couple, and they let me have a front room on the second floor which I shared with Bill Hayden, a classmate of mine at Emory. Bill and I roomed together the first two years, until I dropped out of school. Later, I roomed at a fra-

ternity house. Since none of the older Hopkins fraternities would take us southern boys, Pi Mu, an old fraternity—Dr. Hugh Young was a member of Pi Mu—came in and set up a new fraternity chapter.

Q: What was the nature of your preclinical training?

Rivers: The Hopkins had a very strong preclinical group when I was a student. This was true for anatomy, pathology, physiology, pharmacology, and chemistry. For example, I had John Abel for pharmacology. John Abel was a most distinguished pharmacologist and had, at the turn of the century, discovered epinephrine. A short time later with the aid of Takamine he purified and crystallized epinephrine, which was no mean feat. Walter Jones in physiological chemistry was of like caliber as was William H. Howell who taught physiology. Howell was a wonderful teacher and man, and I don't know how many generations of medical students were brought up on Howell's *Textbook of Physiology*. In my day it was used throughout the United States. Later, when Howell retired from his professorship of physiology, he succeeded Popsy Welch as dean of the School of Public Health at Johns Hopkins. I, of course, had Popsy Welch as my professor of pathology, and he is so well known that I need not make further comment.

The fellow I want to talk about is Franklin P. Mall, who was my professor of anatomy. At the time I took anatomy at the Hopkins, Dr. Mall had two wonderful assistants. One was Eliot R. Clark who later became professor of anatomy at the University of Pennsylvania, and the other was Florence Sabin, who was in charge of the course in histology. Later Dr. Sabin went to the Rockefeller Institute, where she worked on tuberculosis and various diseases of the blood. When she retired, she went back to Denver to live with her sister, and there in the twilight of her life began a new career in public health. I don't think that it is too much to say that she was the driving force behind the public health renaissance that Colorado had in the late thirties and early forties. It always amused me to hear the mayor of Denver and the governor of Colorado refer to Dr. Sabin as "a little old lady." She was little and she did live to the age of 84 or thereabouts, but she had more iron in her than the whole state of Colorado.

Now all of this is prologue to saying that Franklin P. Mall paid lit-

tle attention to his students. He didn't think that we were worth paying any attention to. Mall was a brilliant scientist but did not believe in lectures and other orthodox techniques of teaching. From time to time he would speak to us; however, most of the teaching that we got came from people like Clark, Sabin, and others who would come around and instruct us while we were working in the laboratory. Mall was not a good teacher,[4] but he was a brilliant man, probably greater than any one of the four doctors—Osler, Welch, Kelly and Halsted

[4] Alan Gregg of the Rockefeller Foundation presents a different view of Dr. Mall's teaching in his unpublished autobiographical reminiscences.

In the spring vacation of my senior year I left the tutoring job that I had in New York and went down to Baltimore. On arrival at the Hopkins I wandered into the anatomy building. I saw a very sharp looking person with very intelligent eyes there in the hall, and he came over to me and said, "Is there anything you are looking for?" I said, "No." "Well," he said, "You are looking around." I said, "I don't mean to bother you with my own affairs but I am trying to decide what medical school to go to."

This individual was Franklin P. Mall, whom I didn't know from a hole in the wall. He gave me about fifteen minutes of talk about what Johns Hopkins was. I left in a perfect storm. I wanted to sign on so much. I went out into the street and I saw . . . a fellow I had been a counselor with up in a YMCA camp in my freshman year. He looked at me with astonishment and said, "Why Alan Gregg, what the hell are you doing here?"

I said, "Why, I am trying to make up my mind what medical school to go to. I am going to go to medical school next autumn."

He said, "You come here. You won't make any mistake. We have wonderful professors. In physiology there is Howell. Popsy Welch is in pathology. Then there is Thayer in medicine." He was all enthusiastic, and then he added, "All except one son of a bitch."

I said, rather guardedly, thinking that it would be a good thing to know early on, "And who is that?"

"He thinks he's our professor of anatomy. His name is Franklin P. Mall. I'll just tell you what he did to us. The first day he met us in anatomy he said, 'Gentlemen, the dissection room will be open from nine o'clock in the morning until ten at night except on Saturday afternoon when you ought to go out and get some exercise anyhow. Up until six o'clock there'll always be somebody to get you out of any tangles you get into. I can recommend the three following textbooks, and when you are ready to take the examination let me know.' Now," said my young friend, "if you can beat that as a son of a bitch of laziness in a professor you're going some."

Mall has stayed in my mind as a good example of excellent teaching completely wasted on a badly prepared student mind. This boy I spoke of just missed Franklin Mall by yards and yards and yards. I privately think that Mall's teaching was designed for and appreciated by A No. 1 students, but that Mall had an appalling mortality because so many of his students thought, "Well, he must be interested in golf or something. He isn't teaching." This boy, incidentally, was 1908 at Yale, and the level of Yale instruction then completely failed to fit him for Johns Hopkins and Mall's instruction. Mall was remarkable, and he's far nearer to being the core of Hopkins than many people realize.—S. Benison, *Alan Gregg, An Oral History Memoir.* Columbia University Oral History Research Office, 1956, pp. 33–35.

For another positive view of Dr. Mall as a teacher, see F. R. Sabin, *Franklin P. Mall: The Story of a Mind.* The Johns Hopkins Press, Baltimore, 1934, pp. 220–222.

—that Sargent painted. In spite of his poor teaching I would say that he had a great influence on the development of the Johns Hopkins Medical School. In a word he provided standards of excellence.

Q: Did any of your preclinical teachers attract you to do research work?

Rivers: They wanted me to, and they tried to, but I didn't do a bit of research. When I went to Hopkins, I was determined to be a practicing physician, and I was going to be as good a one as I could. It never occurred to me that doing research would make me a top notch practicing physician, and I went through school, my interneships, and residencies without doing any real research. However, I did learn as much physiology, anatomy, and pathology as I could, and by the end of my second year I was quite adept in bacteriological and pathological laboratory work.

Q: Could you then tell me why you dropped out of medical school at the end of your second year?

Rivers: Well, I don't know how many people know the reason, because I kept it quiet for a long time. When I was in college, I was considered the strongest man in the entire student body. I took a great deal of interest in athletics, particularly in gymnasium, and during the last two years in college, after our regular gym professor left, I conducted two classes a day in gymnasium, in addition to my regular classwork. I was a pretty good-looking physical specimen when I got into the Hopkins. However, during my second year—I don't remember just when—I began to notice that my left hand was getting weaker and smaller and losing a lot of muscle.

Finally, I went to see Dr. Henry Thomas, who was a professor of neurology, and had him examine me. Then Dr. Lewellys F. Barker, the professor of medicine, who also knew a great deal about neurology, also went over me. They told me that I had progressive central muscular atrophy of the Aran-Duchenne type, and that the disease usually ran a fairly rapid and fatal course, with an exception here and

there. After they told me, they advised me to drop out of medical school and go back home.

Well, I returned to Jonesboro and after mooning around for a while I kind of got fed up waiting to die. At that time I had a friend named Claude Winn, who was a doctor in charge of the laboratory at the San Tomas Hospital in Panama. I wrote Claude asking if he had any jobs open for a laboratory assistant, and he replied, "If you can get Popsy Welch to write you a recommendation, you won't have any difficulty getting a job down here."

I thereupon took a train to Baltimore and went around to call on Dr. Welch. I told Dr. Welch that I wanted this job in Panama, and that Dr. Winn had told me that if I could get a recommendation from him I'd have no difficulty in getting the job.

I said, "Dr. Welch, would you mind giving me a recommendation?"

He looked at me, and said, "Yes. What's your name?"

I don't believe Popsy Welch knew me from Adam, but he sat down and wrote me a glowing recommendation in longhand. If I had that recommendation today, I think I could be elected President of the United States!

At this point, I might tell a story that went the rounds at the Hopkins while I was there; even after I left Baltimore, I heard it on several occasions in different parts of the country. The story went something like this:

Dr. Mall was pretty indifferent toward his students. He never lectured very much, and he saw very little of his students. Apparently some members of the faculty thought Mall should give more attention to the students, and so they appointed Welch to go over and talk to Mall. Welch was true to his charge and chided Mall about not lecturing and not knowing more about his students, to all of which Mall replied, "Yes, all of that's true. It's true. I believe everything you say." And that ended that visit.

Several months later, Mall went over to see Welch, and said, "Welch, I've been thinking about that talk that you had with me several months ago about the way I neglected my students. You were quite right. I have been neglecting them: I understand that you know students and don't neglect them, and I just thought I'd come over

and admit my shortcomings and ask you about Sam Jones who is in your class."

Popsy said, "Yes, I know Sam Jones," and they talked for a while about Sam. Then Dr. Mall said, "I'm sure you must also know Eddie Blythe." Dr. Welch said, "Yes, I know Eddie," and he talked about him for a while.

Dr. Mall subsequently mentioned several other names, and Dr. Welch talked about them. After this had gone on for half an hour or more, Dr. Mall said to Welch, "Now, look, Welch, you're just a damned liar. None of those boys are in your class. They're just names I made up!"

Now I don't know if this story is true but I think that it reflected the truth of what went on at the Hopkins. Mind you, not the truth as Welch or Mall understood or looked at it, but the truth of what other folks thought they saw.

Q: Was your trip to Panama the first time you were out of the country?

Rivers: Oh, I'd been to Baltimore. I wasn't like some of the folks in Jonesboro. Some of my townmates—I won't mention any names—heard one day that so-and-so had been over to a place in Alabama, and this townmate of mine said, "Well, that must have been the first time he ever went out of the country." I wasn't quite that bad. I did read the newspapers. I did know the boundaries of the United States. And I knew what was going on down on the Canal, and I wanted to see what was going on there for myself. However, I didn't realize just what I'd have to go through to reach Panama.

I borrowed my railroad and steamship fare from my father before I left Jonesboro, and I boarded a train to New Orleans. About 50 miles out of New Orleans, the firebox of the engine fell out, and we were left stranded in the middle of a bayou with no way to get into New Orleans except to walk the crossties. Originally we were scheduled to reach New Orleans the early part of the morning, around 7 to 8, and my boat was to sail somewhere around midday. As a consequence of this mishap, I missed my boat to Panama. I had figured pretty closely

on the amount of money I'd need to get to Panama, but I hadn't figured on a layover for four days in New Orleans.

While I was waiting for the spare engine to come out and pick us up and drag us in, I noticed that there was a good-looking girl back in the observation car, and I got to talking to her. I soon discovered that she was a school teacher and was on her way to New Orleans to visit her brother. When I told her that I didn't know the name of a single hotel in New Orleans, she told me that her brother was the desk clerk at the Monteleone Hotel, that she was stopping there, and that it would be nice if I stopped there.

Well, I stopped at the Monteleone Hotel, and for the next four days I saw the sights and went to shows with this girl and her brother. When the time came for me to catch my boat to Panama, I discovered that I didn't have enough money to pay my hotel bill. Well this girl's brother proved to be very understanding, and for some reason or other said, "I'll hang your bill on the hook, and you can pay it back when you get your paycheck in Panama."

The trip to Panama took several days and again I spent more money than I had. When I arrived in Panama, I found that I didn't have enough money to buy a ticket across the Canal to Panama City. This time I was rescued by a friendly Panamanian shipboard acquaintance who loaned me $10 to cross the Canal. I was honest enough. I paid my bill at the Monteleone Hotel, and I repaid the $10 I borrowed from my Panamanian friend.

Q: What was Panama City like when you got there?

Rivers: Panama City was under the jurisdiction of the Panamanian government and was just a kind of an island at the western end of the Canal. Actually it ran right into Ancon, the American city that grew up adjacent to it. The Ancon Hospital, which was the American Hospital, was in Ancon. I lived in Panama City, in the San Tomas Hospital, which was quite an old hospital. Like most European hospitals it was originally built as kind of an almshouse where the needy and the sick would come. The hospital was built around an entire square and was surrounded by a brick wall. To enter it you had to pass through

an iron gate that led into a patio surrounded by several buildings. One of the buildings housed a priest. The building I lived in housed the operating rooms and was quite old. The hospital at that time had approximately 350 beds and was run by a Junta or Board composed of Americans and Panamanians. The only difficulty in that situation was that the Americans had one more vote than the Panamanians had, and so controlled the Board. I thought that it was handled diplomatically. The Panamanians didn't. The whole time I was at the San Tomas Hospital, Dr. Bill Caldwell was superintendent. Actually Panama City was not too bad a place to live. But the Panamanians didn't like us, and they still don't like us. The main reason they didn't like us, at that time, was that we promised to pay them $250,000 a year rent for the Canal Zone, but we never gave them the $250,000. What we did was to spend it on putting in proper sewage, putting in proper waterworks, paving the streets, and doing the things that we thought the politicians down there would never do. The Panamanians just didn't like the way we spent their money. And I must admit that we were a bit highhanded.

Q: What were your duties in the hospital?

Rivers: In the beginning I was laboratory assistant, and it was my job to count bloods, do urine analysis, stain pathological tissues, and do the dirty work on autopsies. As I learned, I was given more interesting things to do. When I arrived in Panama I started to take lessons in Spanish and after about three months I began to speak it—it's not too difficult a language. One day Dr. Caldwell called me to his office and asked me if I could speak Spanish. When I said that I could, he told me that the Salana brothers, two doctors who were running the dispensary, had left, and that I was to go over and take charge of the dispensary. I said, "Dr. Caldwell, I can speak Spanish, but I don't know how to take care of patients." "Oh," he said, "that's easy. I am sure you can take care of patients and better than the Salana brothers, and I paid them $200 a month. You go over and take care of the patients in the dispensary."

Following Dr. Caldwell's instructions, I went to the dispensary, and immediately after I got in there, since they were short of hands,

they worked me into assisting at operations. Initially I gave anesthesias. Then one of the internes decided he'd go back to the States, and so one fine day Dr. Caldwell called me in and said, "Rivers, go over and take care of the prison ward and the police ward."

I said, "Dr. Caldwell, I can't do that. I'm only a second-year medical student. There are a lot of major operations on those two wards. I just ought not to do that."

He said, "Don't worry about that. I'm in charge of those two wards. I'll take all the responsibility. You go on and take over."

But he didn't say anything about giving me any more money. So I said, "Look, Dr. Caldwell, I came down here for $75 a month and my keep. I took over the dispensary and saved you $200 a month, and you didn't give me any extra money. Now I'm taking over two more wards, and I'm saving you another $150 to $200 a month. Don't I get any raise?"

"Look, Rivers," he said, "You're not a doc, and I am giving you a chance to do things that a man with your training would never get to do." Well I argued with him a while, and he finally agreed to give me an increase of $25 a month; in the end I got $100 a month and my keep. But he was right, I learned a tremendous amount.

Q: When you say you learned a tremendous amount, was this in surgery or clinical medicine?

Rivers: In terms of everything. In terms of what to do. But more important, in terms of what not to do. I think a lot of doctors have to learn what not to do, and I sure learned what not to do. I did 85 major operations. The first operation I ever did was a double herniotomy on a policeman. A little Negro boy name of Joseph—he was about 20 years old—gave the anesthesia, while the head nurse of the operating room, Schwester Schuman (Sister Schuman), a German, assisted me. I saw quite a bit of this policeman afterward, and the repair was still holding up to the time I left Panama. So it wasn't too bad. Actually the boys in the operating room were pretty nice, and when I assisted they instructed me in what they were doing. Most of the boys believed in keeping the blood tied up, and I became adept at not losing too much blood during the course of an operation.

I must admit that the medical student or interne in this country never was exposed to the crudities of medicine that I saw in Panama. For instance, the first ambulance case that came in, when I was officer of the day, after I had taken over the police ward and the prison ward, was a dislocation of the shoulder. It was a Negro patient. A big ambulance man and other attendants dragged this man in and put him on an examining table. I didn't know what was the matter with him, but the ambulance man said, "He's got a dislocated shoulder."

I still kept looking at him, and he said, "You don't know how to set it, do you?"

I said, "No, I don't know how to set that dislocated shoulder, and you know I don't."

"Well, I'll show you," he said, and with that he took his big foot and put it up on the poor patient, took hold of his arm, and pulled the damned shoulder back in place. I didn't know whether it was set right or not, but he got the shoulder back in place, and the man went out of there with a perfectly good shoulder.

Q: Did the San Tomas Hospital have x-ray at the time?

Rivers: Yes. Soon after I came to Panama an x-ray machine arrived at the hospital, and the man who installed it showed me how to run it. It was a huge machine with two large plates, and every time a picture was taken a huge spark would cross between the two plates. The reason I never burned myself was that the machine came equipped with a small room for the operator, so I was safe enough. I still don't understand how I missed electrocuting the poor patients. I should say here that, since I was the young guy, everything dirty, everything new was pushed off on me. I even had to pull teeth. But there was nothing I could do about it, since most of the doctors in the hospital were teaching me, and even though they were not much older than I was, they were teaching me and I didn't want to risk making them mad by complaining, so I kept my mouth shut.

I think the most famous x-ray picture I ever took was a picture of the hand of President Poris of the Republic of Panama. The San Blas Indians of Panama were not a very friendly group of Indians, and for years they would not swear allegiance to the Republic of Panama.

Panama was never able to get tax money out of them or, for that matter, anything else. While I was in Panama, they finally sent one of their chiefs in to Panama City to reach some agreements with the Republic of Panama. While this chief was in Panama City, President Poris brought him over to see San Tomas Hospital. Poris had never seen an x-ray, and the San Blas chief had never seen an x-ray, and I'm sure that the interpreter who came with them hadn't seen one. I was amused that the San Blas who acted as interpreter could speak perfectly good English. "Where did you learn to speak English?" I asked him. "Oh," he said, "I lived in the United States a long time."

"Where?"

"I was a streetcar conductor in Philadelphia for many years," he replied.

Their interest in the machine was so great that I took President Poris over and took a picture of his hand. I must say that the President and the Indians were all greatly impressed. But I should stress that I only took x-ray pictures, I wasn't responsible for interpreting them—that was the duty of the regular doctors.

Q: What was the nature of the diseases that you saw at the San Tomas Hospital?

Rivers: One of the diseases I saw a great deal of was lobar pneumonia. When we think of Panama we usually think of tropical diseases, but it should be remembered that one of Dr. Gorgas's great achievements in Panama was his campaign against pneumonia. Many of the laborers in Panama lived in congested conditions in poor huts that were lined with tiers of beds. There was never any real honest-to-God dry season in Panama; there was only a wet season and a wetter season. In such an environment, laborers in Panama came down with lobar pneumonia. There was little to be done for lobar pneumonia in those days. There was, for example, no useful drug therapy, and each doctor went his own way. Some gave a little digitalis for the heart, which didn't do any good. Some gave alcohol, but most treatment was symptomatic. I don't think that our death rate was higher than it should have been. I think that letting patients alone was about as good a treatment as one could think of in those days. Gorgas made a

dent in the rate of lobar pneumonia by campaigning against congested living conditions in these huts. Later that technique was adopted by Sir Almroth Wright in his campaign against lobar pneumonia among mine laborers in Johannesburg, South Africa.[5]

Another disease I saw a great deal of in Panama was tuberculosis. The San Tomas Hospital had a very large TB ward, but there was little that we could do at that time for the patients. Nothing was known about how to handle such patients, and even if it had been known you still could do nothing for them. For one thing, there was not much money available for the feeding of patients in the hospital. If I remember correctly, it cost the Panamanian government 11 cents a day to feed a patient, and even in 1912 and 1913 that was a small sum of money to pay for three daily meals.

Q: Did you see much venereal disease?

Rivers: Yes I did. It was while I was in Panama that I learned how to do the so-called Wassermann test. In 1905 Schaudinn discovered the *Treponema pallidum*, and a year or two later August Wassermann helped devise a complement-fixation test for the diagnosis of syphilis. Wassermann's name is usually affixed to this test, but to my mind Jules Bordet deserves the credit because he developed the first complement-fixation test. Well, when I came to Panama, Wassermanns were being done at the American hospital in Ancon. When I went to work in the dispensary, Dr. Caldwell gave me instructions to learn how to do the Wassermann test, and I learned how to do it from one of the books we had in the library. I am not so sure about whether I was exactly as I should have been on the first Wassermanns that I did, but before I left Panama I had learned quite a bit about how to do a Wassermann and my results were more accurate. You might say that the worst that I did for a patient on that deal was to have some poor guy get some Salvarsan that didn't need it, or maybe miss giving a person Salvarsan who should have had it.

Fortunately, nobody died from the Salvarsan that I administered. I

[5] Almroth Wright's biographer, Dr. Leonard Colebrook, states that Dr. Wright attempted to control the pneumonia outbreak in Johannesburg with a pneumonia vaccine. L. Colebrook, *Almroth Wright*. Heineman, London, 1954, pp. 65–69.

mention this because the Salvarsan that I gave was the old "606." It was not the neo-Salvarsan that is sometimes given today. The Salvarsan that I gave had to be mixed with distilled water and an alkali, since it was very acid and had to be made properly alkaline before being given. So there was some danger. If you missed the vein in administering the Salvarsan, you caused the patient a great deal of pain, because it was terrifically irritating to the tissues. Of course, we at that time thought that one dose of Salvarsan was sufficient. There was reason for that. In Panama there was a good bit of tertiary syphilis, which often manifested itself by big gummas on the body, and frequently they would disappear like magic after a single dose of Salvarsan. We were ready to kiss our patients goodbye, but they were not well. It took us some time to realize that we had to follow the patients for quite a period.

Q: Was the Wassermann sufficient for diagnosis?

Rivers: There were many problems in diagnosis, and one of the problems we had to face in Panama was that there were diseases in that area that would give you a positive Wassermann. More specifically, there is a disease called yaws that is usually present in tropical regions, and it too is caused by a spirochete. Yaws can give you a positive Wassermann, and I can tell you that it tested a man's clinical ability when it came to telling the difference between yaws and syphilis in making a diagnosis. I suppose we at the San Tomas Hospital made the usual number of mistakes.

Since I did the Wassermanns and administered my fair share of Salvarsan, at one point I thought I would try to see what Salvarsan would do for leprosy. We saw a certain amount of leprosy in Panama. Many of the lepers were not Panamanians but came from Colombia in South America. In Panama, when a diagnosis of leprosy was made, the patient was picked up and immediately sent to a leper colony. In Colombia, they had no leper colony, and they allowed the lepers to go about their business. When a suspected case of leprosy was picked up, what we would do was to make an incision of the extant nodules either on the hand or ear lobe to see if we could get the acid-fast organism that was necessary for diagnosis. If it didn't turn up from this

procedure we usually took a nasal smear. It was my experience that you would always get an acid-fast organism from the nasal smear of a leprous patient. If the patient was a Colombian, we'd tell him what the diagnosis was and try to persuade him to go home, because the leper colony was not a nice place for anybody to go to, and we felt that as a Colombian he should have a choice. If the patient was a Panamanian, we had no choice in the matter.

Well, I tried to treat a number of my leprous patients with Salvarsan. I need not say that nothing happened, because that's what did happen—nothing—except that, during the course of such treatment I saw sensitization to a chemical for the first time. I gave one lady patient 12 injections of Salvarsan. Along about the 10th injection she had a mild reaction; at the 11th she had a more severe reaction; at the 12th injection she had a severe enough reaction so I was sure I was never going to give her another one.

It was hard for me to understand why a person would become sensitive to a chemical agent, because at that time most people believed that a person could only become sensitive to a protein. Some time later, Homer Swift, at the Rockefeller Institute, and Hans Zinsser noted sensitivity of patients to certain drugs and postulated that chemicals united with some proteins of the body, altering them sufficiently so that they would become antigenic for the human body. Landsteiner later proved this supposition experimentally, but, back in 1912 and 1913, it was all very nebulous why a patient would become sensitive to a chemical agent.

Q: Dr. William C. Gorgas is known for his achievement of ridding Havana and Panama of yellow fever. Were there any cases of yellow fever in Panama City during your stay in Panama?

Rivers: During the whole time I was in Panama, I only saw one case of yellow fever in the San Tomas Hospital. But that case came from Guayaquil. Actually, when I was in Panama, they had a fairly severe outbreak of yellow fever in Guayaquil and a special commission was sent from the United States to investigate that outbreak. The reason I remember that is because one of the members of that commission

was George Whipple, who later was to win the Nobel prize for his work on anemia—a prize he shared with George Minot of Boston. I might have said hello to Dr. Whipple at that time; if I did, it was all I said, because after all I was only a laboratory assistant, and what was he going to waste his time for talking to the likes of me? The thing that most impressed me about George Whipple at that time was that such a good-looking hombre—and he was more good looking than he had a right to be—was also such a serious-minded fellow.

If I saw only one case of yellow fever, I should add that I did see plenty of malaria on the wards of the San Tomas Hospital. Now, few of these cases originated from the Canal Zone; in the main they came from other parts of Panama. By this time old Dr. Henry Carter had just about cleared up malaria in the Canal Zone. I not only saw the ordinary type of malaria, I also saw the cerebral type which is pretty rare now but was not so rare at that time. A man might be walking down the street and suddenly drop. When the doctor came, one of the first things he did was to smell the guy's breath to see if he was drunk. The second thing he would do was to test his urine to see if he had diabetic coma. The third thing was to take a smear of the blood to see if he had malaria.

I never failed to find malaria on the first go round of cerebral cases, because cerebral malaria was caused by the plugging of the small blood vessels in the brain by malarial parasites. We usually had autopsies of such cases, and I want to tell you that these pathological sections were the most fantastic I ever saw, because these small blood vessels were just crammed with malarial parasites until you wondered how in hell the patient walked around as long as he did.

We had a pretty drastic method for treatment, because it usually killed one out of every three patients. When you knew that you had a case of cerebral malaria on your hands you usually gave him an intra venous injection of hydrochloride of quinine, and about one of every three would fall over dead before you could get the syringe out of the vein—the other two would live. It gave you pause when you realized that when you put the needle in the guy's arm he might not make it, but there was no option, because it was a question of saving two out of three patients.

Q: Dr. Rivers, your comments on malaria raise the question in my mind whether you received any formal instruction in pathology during your stay in Panama?

Rivers: You can't be exposed to as much pathology as I was exposed to in Panama without learning something. Actually a number of people had a hand in instructing me in pathology while I was in Panama. One, of course, was my friend Claude Winn who arranged the job for me as a lab assistant; another was William Deeks who at that time was in charge of the American hospital in Ancon. Dr. Deeks later became the chief medical officer of the United Fruit Company. He was an enormously fat man; as a matter of fact, he was just too fat to walk, and, since the Ancon Hospital was composed of many buildings, Deeks, in order to keep up with his medical duties, rode from building to building in a step-in carriage called a phaeton. In spite of his enormous bulk, he was really an excellent doctor.

The fellow I learned the most from was a doctor named Sam Darling. Sam was an extraordinary malariologist and very early went to work in Panama; as a matter of fact, he lived for and through the development of the Panama Canal and its medical activities. His initial pay, I believe was $50 a month. Later he became a field officer for the International Health Board of the Rockefeller Foundation and had a most distinguished career until he was killed in an auto accident in the Balkans somewhere around 1925. Sam loved to swim, and we frequently went on swimming parties to a beach about six miles out of Panama City. They had wonderful bath houses, and the beach was a delightful one, except that it was pretty rough. They had 12-foot tides on the Pacific side and only about a 3-foot tide on the Atlantic side. This difference in tides was one reason among many why it was difficult to build a sea-level canal.

Well, one night we had a swimming party at this beach, and the wife of one of Sam's assistants got too close to some rocks as the tide was coming in, and she was in trouble. I'm not a very good swimmer, but I was the man closest to this lady, and I started to swim toward her. Well, I didn't know whether she'd drown, or whether both of us would drown, but somehow I got over to her, got hold of her, and I must say that I yelled as loud as she did. In the end, three or four

good swimmers made their way to where we were and took this lady in to the beach. They were sure taking care of this good-looking lady, but no one, not even Sam, paid any attention to me. The truth is, they didn't give a damn whether I drowned or not.

Q: What decided you to return to medical school?

Rivers: After eighteen months in Panama, I got the impression that I liked the practice of medicine. Now, medicine had appealed to me before I went to Panama, but being directly in contact with it, even in a crude and rough way, I made the discovery that it was interesting. I began to appreciate that there was a great deal that we didn't know that we thought we knew about disease. I saw things as a laboratory assistant in Panama that a student doesn't ordinarily see until he has had more training. When you are at medical school, everyone is making damned sure that you don't get too close to anything. In Panama I was just mixed up in it, I was right in the middle of it, I was on top of it, and by God it was impressive.

When I decided to return to the Hopkins, I just wrote to the dean and told him of my wish to return. He told me to come on ahead, and I went

Q: What role did history-taking play in the clinical instruction of medical students at the Hopkins?

Rivers: The teaching of history-taking was deemed most important and regarded as a prime tool in making diagnosis. We were taught that doctors might get important leads in diagnosis from a good history, leads that might otherwise be overlooked. The teaching of history-taking began when the boys went over to the hospital for their third and fourth years' training. When we started in the dispensary, for example, we had such excellent clinicians as Louis Hamman and Thomas Futcher teaching us, and they were very specific in their instructions about how to take a history. When we went on the wards in the fourth year, we were also made responsible for taking histories. These histories were checked by the interne, and if we took an inadequate one or a bad one, the interne would make it pretty sorry for us,

and that would only be the half of it, because the professor who held rounds the next morning would harass us as well.

An important adjunct to taking history was physical diagnosis, and in our clinical years we were drilled in relying on touch, hearing, sight, and smell, and we became adept in it. Taking a good history and doing a thorough physical were two of the most important things you could do to arrive at a diagnosis, because at that time, outside of doing bloods and urines, there were few diagnostic aids, save x-ray.

When I was a student, one of the great pioneers in x-ray, Dr. Frederick Baetjer, taught at the Hopkins. You might say that Baetjer was one of the first clinical roentgenologists in the United States, and like Walter B. Cannon at Harvard was also a victim of his work. Because of exposure to x-ray, Baetjer developed a malignancy of the skin and lost several fingers from his hands, and in the end lost an eye from a malignancy of the retina caused by x-ray. Baetjer taught us how to interpret x-rays. In class he would use x-rays that had been taken of patients several days before. He never asked the names, age, or sex of the patients before presenting them to the class, but invariably he would tell us how old the patient was, whether they were male or female, and finally whether the picture showed a normal or pathological condition. He rarely missed. Of course, I suppose a man having seen as many x-rays as Baetjer had would sooner or later be able to recognize the difference between the bones of a male and female and distinguish between the bones of the young and old. But I must say that it seemed miraculous to a student to see Baetjer go down the line.

Q: Was the clinical pathological conference then in use for teaching purposes?

Rivers: I don't know when the Hopkins began to use the CPC, but it was certainly in use when I was a student. The CPC was the best show of the week and was always a tug of war between the pathologist, the clinician, and the surgeon, and the students dearly loved it. Popsy Welch, and Dr. Milton Winternitz who later became dean of the Yale School of Medicine, were the two pathologists who usually presided at the CPC in my day. The cases chosen for the CPC were chosen by the pathology department, and I don't think that I am far

from the mark when I say that, for the most part, cases were chosen with the idea that they would show up the clinicians and surgeons.

Q: Were excellent clinicians like Dr. Lewellys Barker and Dr. William Thayer, or surgeons like Dr. Thomas Cullen and Dr. John M. T. Finney, shown up very often by the CPC? The reason I ask the question is to get some notion of the validity of the statistical medicine which was then used.

Rivers: Lewellys Barker was an extremely intelligent man, but I don't think that he was a great clinician. Prior to coming to the Hopkins to replace Osler, who had resigned to become the Regius Professor at Oxford, Barker had been a professor of anatomy at the University of Chicago. He was appointed professor of medicine but that didn't make him a clinician. I watched Barker when I was an interne, and when he walked on the ward to make rounds he would always choose a patient who had been previously well worked up. If the patient were well worked up and Barker had the histories or notes from excellent diagnosticians like Thayer, Futcher, or Hamman, he would take the case. To listen to him discuss the history and the nature of a given disease was beautiful. Barker was a master at that. And his rounds and his clinics were not a complete bust, because he did add something. But he never liked to show a patient at a clinic unless he'd been previously well worked up. The reason I know this is because, every now and then when a patient was going to be shown in the clinic the next day, Barker would have me look up the literature and fetch the necessary books to his office. So I know that on certain occasions Barker knew what the diagnosis was on a patient for the next day, and he had a chance to cram up on the literature.

Theodore Janeway, on the other hand, was just the opposite. He did not want to know what the patient had and he did not want to have the patient worked up before he saw him on the ward or even in the clinic. He wanted to take a crack at it cold. Now, that was the difference between Barker and Janeway. Barker I don't think would have gotten to first base if he'd taken a crack at a case cold. Janeway knocked a double, frequently a triple, and occasionally a home run, on the patient that he saw.

I would like to illustrate how Janeway worked. On one occasion when I was an interne, I had just taken over Ward M, which was the men's colored ward. A very interesting case had come in the night before, and I took the history and worked up the case. The next morning when Dr. Janeway came in to make rounds, I tried to get him to go over and see this patient who I thought was so very interesting. But Janeway would have nothing to do with it. He saw an elderly Negro man sitting in a bed over on the other side of the ward and said, "I want to see that man."

I said, "Dr. Janeway, that man's been here a long time. Everybody has seen him and examined him, and no one has ever quite figured out the diagnosis."

Janeway said, "Well, I know what's the matter with him. I want to see him."

I said, "All right, we'll go over and see him." Well I went over the history and gave Janeway the findings of the other doctors who had previously examined him and finally came to the stone wall in diagnosis, namely, that nobody, but nobody, knew what was wrong with this poor fellow.

Janeway said, "I know what's wrong with that man. I don't have to examine him. He's got a carcinoma of the prostate, with metastases to his spine and his pelvic bones."

I said, "Dr. Janeway, the GU people have been over here, and they examined him and said his prostate was all right. There's nothing in the world wrong with his prostate."

Janeway replied, "I don't care who's examined him, I know what he's got."

I said, "Dr. Janeway, this is a little bit disturbing."

He said, "There's nothing disturbing about it."

"Well," I said, "won't you tell the students why you know what's the matter with him?"

"I don't mind," he said. "It's the way he's sitting in bed." "My father—and this was a joke, because he was always talking about his father—I saw several patients with my father in New York, and they were sitting in bed like this fellow is, and they all had carcinoma of the prostate with metastases."

I said, "Well, I'll take your word for it."

He said, "You don't have to take my word. We'll take the man over to x-ray." So Janeway went along with us to x-ray and had the pelvic bones and spine of this old colored man x-rayed, and, lo and behold, he had metastases in his pelvic bones and the lower part of the spine. Janeway had never laid his hand on the man. He made a diagnosis from the way the man was sitting in bed.

Q: Did Hugh Young miss this carcinoma?

Rivers: Hugh Young didn't miss it. Hugh Young did not examine this patient. He was examined by other people in the Department of Urology, but I'm not so sure that, in this instance, it wouldn't have been missed by any GU man, because you just don't always feel everything that's in a prostate. Janeway was full of surprises. Barker was never a surprise. Most of us young fellows who had been around the hospital any length of time knew his limitations. Later Barker was offered the full-time professorship in medicine and turned it down to go into private practice. Just let me say that Barker was one of the few practitioners of his day that actually made a million dollars from the practice of medicine and the investment of what he made at the practice of medicine. But even in private practice, his success depended on two people, Dr. Thomas Sprunt and young Dr. Walter Baetjer, who were two of the best diagnosticians in Baltimore.

Getting back to your original question, I would guess that no matter how good diagnosticians were at the Hopkins, and some of them were very good, they were never more than 65 to 70 per cent right in their diagnoses—and I believe I am being rather generous in my estimates. When you look back, even if they were right, there was precious little that they could do. At the time I was a student, there were not many drugs that could be used for the treatment of disease. When you got through with digitalis for the heart, quinine for malaria, mercury and Salvarsan for syphilis, and one or two drugs for intestinal worms, you had almost run out of therapeutic agents. Actually, the Rockefeller Institute in New York had its origin in the fact that one of John D. Rockefeller's advisors, Frederick T. Gates, who was a Baptist minister, early recognized that while doctors were fairly

adept in making diagnosis they had no way of treating diseases after they diagnosed them. It was Gates who urged Mr. Rockefeller to start an institute devoted not only to research on the nature of diseases but on how to treat them as well. Abraham Flexner in his autobiography gives most of the credit of the founding of the Institute to Dr. William H. Welch, and I must agree that Dr. Welch played an early and important role in the development of the Institute. He was a key figure on the Board of Scientific Directors of the Institute and was largely responsible for getting Dr. Simon Flexner appointed the first director of the Institute, but, no matter what Dr. Welch's role was, the idea for the Rockefeller Institute was indisputably Gates's. Recently a copy of a memorandum [6] that Mr. Gates prepared for Starr J. Murphy in 1915 on the origins of the Rockefeller Institute came to my attention, and I would like to submit it for the record, since it is a unique document in the history of American medicine and casts much light on the beginning of one of the great medical research institutes in the United States and, for that matter the world.

December 31st, 1915

Dear Mr. Murphy:

You asked me the other day to write you my recollections of the origin of the Rockefeller Institute for Mr. Rockefeller's private files. You wished me to trace the very idea back to its sources, and accordingly, so far as my recollections will serve, I will give you what may be termed the pre-natal history of the Institute. I have no doubt that the history of the Institute will some time be written, and I am aware that these obscure questions of origin however unimportant have a greater interest often than the later and more obvious facts. Moreover, if we who are acquainted with the earlier facts leave no record of them, it will be impossible for our successors to recover them when we have passed away. I therefore leave with you, for the files of Mr Rockefeller and the Institute, my recollections of the early history of the Institute in this my compliance with your request.

[6] This memorandum was originally quoted in part by Simon Flexner and James T. Flexner in their biography of William H. Welch. George Corner has since reproduced it as an appendix in his recent history of the Rockefeller Institute. When Dr. Rivers inserted it during his oral history interviews in the late spring of 1961, it had not yet been printed in its entirety. In order not to disturb the continuity of Dr. Rivers' remarks it is reprinted here again. S. Flexner and J. T. Flexner, *William H. Welch and the Heroic Age of American Medicine*. Viking Press, New York, 1941, pp. 269–272. G. W. Corner, *A History of the Rockefeller Institute*. Rockefeller Institute Press, New York, 1964, pp. 575–584.

But first of all, the historian of the Institute should be made acquainted with what I may call the atmosphere or spirit of Mr Rockefeller's private office. During all the twenty-five years in which I have been intimately associated with Mr Rockefeller, he has been sincerely desirous of employing his great fortune in the service of mankind, at home and abroad, whether that employment be in investments or in bestowal on private or public charity. It has been during all these years my chief end in life, as a member of Mr. Rockefeller's staff, to assist him in this aim, both in the sphere of investment and in the sphere of bestowal. It is not too much to say that every day and hour of my life during these years has had for its underlying motive, watchfulness for such opportunities of public service. With this introductory explanation, to you personally quite unnecessary, but perhaps useful to other eyes, I pass to the narrative. This I will make as full and comprehensive as possible.

Origins are often very humble. Christianity itself was born in a manger. It is so with the Institute. During the years 1880 to 1888 I was the pastor of the Central Baptist Church of Minneapolis. In my congregation was a young boy named Elon W. [sic] Huntington, a member of one of the most useful families in the church, a family which for many reasons was particularly dear to me. Elon himself I baptized as a boy in his teens. In due time he was graduated from the University of Minnesota. His father, long deceased, had been a physician and Elon chose his father's profession. Not finding in the West the best facilities for the study of medicine, Elon came to New York and became a student in the College of Physicians and Surgeons. This was in the early 90's; probably 1894. I had left my pastorate in Minneapolis in 1888 and at this time was living in Montclair as a member of Mr. Rockefeller's staff, assisting him in his private business investments and in his benefactions in the spirit of my introductory words above. Elon Huntington was a lonely student without a friend and almost without an acquaintance in New York, and he used to come out to visit us in Montclair, often spending the night or the Sunday with us to relieve his loneliness. In this way he spent many hours with me. We used to take long walks together and the subject of our conversation was quite naturally medicine, the subject in which he was most interested and in which he was then most intelligent. Thus, in simply entertaining Elon I found myself intensely interested in medicine. My interest reached a point in which I determined to know something more definite about medicine, and in the spring of 1897, when Elon if I recall it, was about to graduate, I told him that I would like to read medicine and I asked him if he could suggest to me a book which a layman like me might be able to understand and to read with profit. I remember telling him that I did not want any of the ordinary medical books for the family. I wanted to know what the best doctors are reading; I wanted the literature that was being taught currently in the best

schools to medical students. Was there any such book pre-eminently good? He replied that there was one such book; it was Osler's *Principles and Practice of Medicine* and said that this book was being taught to students in the College of Physicians and Surgeons and that it was written in a style so clear that with very slight knowledge of medicine I could read it with understanding and interest. He suggested further that I get a little pocket dictionary of medicine entitled "Twenty Thousand Medical Terms" or something to that effect, and he named the place in New York, a medical book store on 17th Street, if I remember, where I could get Osler's book and the little dictionary that ought to accompany it.

I took my first opportunity to hunt up this book store and there I bought my precious volume in June 1897. At this time my sole purpose was to become reasonably intelligent, as a layman, on the subject of medicine. Perhaps I ought to delay here long enough to say that this question of medicine had for many years been a subject of more interest to me than it is to laymen in general. My father, before he became a minister, had studied medicine. Then as a pastor from 1880 to 1888, I had been brought of course into direct and sympathetic relation with hundreds of sickrooms and with both schools of medicine. I had come into confidential relations also with several physicians, and I shall confess that I had come to acquire a profound scepticism about medicine of both schools as it was currently practised. I had read from beginning to end Dr. Hanneman's [*sic*] book known as the New Testament of Homeopathic Medicine. He was the originator of that school and I had come to believe from the reading of that book that Dr. Hanneman [*sic*], founder of the Homeopathic School, must have been to speak charitably little less than a lunatic, a belief cordially shared by a homeopathic physician friend. I would not be true to my feeling at the time, whether right or wrong, if I used any milder terms. Many years afterwards I wrote a review of Homeopathy in five chapters, which sought to exhibit its errors and impossibilities. But I had hardly more confidence in the allopathic school. My intimate conversations with allopathic physicians, who practised in my church had led me to see clearly that at least nine-tenths of their practice without guilt on their part was based substantially on the ignorance and credulity of their patients. I remember very distinctly the impression made on my mind, an impression entirely confirmatory of my previous observation by the remark to me of one of the most prominent allopathic practitioners in Minneapolis, that at least nine out of every ten calls made by physicians, might, for any possible good they did their patients, precisely as well not be made. I introduce the facts to fully explain my interest in medicine and why and in what spirit I began to read in June 1897, Osler's *Principles and Practice of Medicine.*

The book came into my hands at a time of abundant leisure. I spent a

considerable part of the months of July and August following with my family in the Catskill Highlands, at Lake Liberty, in Sullivan County, New York, and I had opportunity to give my undivided attention to Osler's book for considerable part of every day. My wife's diary of those days shows how my time was divided between kites, water-wheels and frog hunting with my children and reading Osler's *Principles and Practice of Medicine*. I read the whole book without skipping any of it. I speak of this not to commemorate my industry but to celebrate Osler's charm. Osler's *Principle and Practice of Medicine* is one of the very few scientific books that I have ever read possessed of literary charm. There was a fascination about the style itself that led me on and having once started I found a hook in my nose that pulled me from page to page, and chapter to chapter, until the whole of about a thousand closely written pages brought me to the end. But there were other things besides its style that attracted and constantly, in fact, intensified my interest. I had been a sceptic before, not only as to homeopathic medicine but as to allopathic medicine as currently practised. This book not only confirmed my scepticism but its revelation absolutely astounded and appalled me, sceptic as I was. Let me name some of the things, which, commonplace as they are to intelligent physicians, were absolutely appalling to me, a layman, although supposing himself to be a sceptic. I found, for illustration, that the best medical practice did not, and did not pretend to cure more than four or five diseases. That is, medicine had, at that time, specifics for about as many diseases as there are fingers on one hand. It was nature, and not the doctor, and in most instances nature practically unassisted, that performed the cures. I learned that with the exception of two or three, the physician had nothing whatever to prescribe for the infectious diseases, which could effect a cure. Osler's own attitude toward drugs was interesting and I came at length to approach his curative suggestion with a smile. His chapter on any particular disease would begin with a profound and learned discussion of the definition of the disease, of its extension throughout the world, of the history of discovery about it, of the revelations of innumerable postmortems, of the symptoms, cause and probable results of the disease, and the permanent complications and consequences likely to follow, but when he came to the vital point, namely, the *treatment* of the aforesaid disease, our author, who had up to this time been treading on solid grounds with the confidence and delight of sure knowledge, would almost invariably disclose a mental attitude of doubt and scepticism. He would suggest that such and such had found that this or that treatment was efficacious but such had not been his own experience; perhaps this or that might be found to be useful in some cases. To the layman student, like me, demanding cures, and specifics, he had no word of comfort whatever. In fact, I saw clearly from

the work of this able and honest man, that medicine had, with the few exceptions above mentioned, no cures and that about all that medicine up to 1897 could do was to nurse the patients and alleviate in some degree the suffering. Beyond this, medicine as a science, had not progressed. I found further that a large number of the most common diseases, especially of the young and middle aged, were simply infectious or contagious, were caused by infinitesimal germs that are breathed in with the atmosphere, or are imparted by contact or are taken in with the food or communicated by the incision of insects in the skin, which serves as a protective covering. I learned that of these germs, only a very few had been identified and isolated. I made a list, and it was a very long one at that time, much longer than it is now, of the germs which we might reasonably hope to discover but which as yet had never been, with certainty, identified, and I made a very much longer list of the infectious or contagious diseases for which there had been as yet no specific found.

When I laid down this book, I had begun to realize how woefully neglected in all civilized countries and perhaps most of all in this country, had been the scientific study of medicine. I saw very clearly also why this was true. In the first place, the instruments for investigation, the microscope, the science of chemistry, had not until recently been developed. Pasteur's germ theory of disease was very recent. Moreover, while other departments of science, astronomy, chemistry, physics, etc. had been endowed very generously in colleges and universities throughout the whole civilized world, medicine, owing to the peculiar commercial organization of medical colleges, had rarely, if ever, been anywhere endowed, and research and instruction alike had been left to shift for itself dependent altogether on such chance as the active practitioner might steal from his practice. It became clear to me that medicine could hardly hope to become a science until medicine should be endowed and qualified men could give themselves to uninterrupted study and investigation, on ample salary, entirely independent of practice. To this end, it seemed to me an Institute of medical research ought to be established in the United States. Here was an opportunity, to me the greatest, which the world could afford, for Mr. Rockefeller to become a pioneer. This idea took possession of me. The more I thought of it the more enthusiastic I became. I knew nothing of the cost of research; I did not realize its enormous difficulty; the only thing I saw was the overwhelming need and the infinite promise, worldwide, universal, eternal. Filled with these thoughts and enthusiasms, I returned from my vacation on July 24th; the year was, as I have stated, 1897. I brought my Osler into the office at #26 Broadway, and there I dictated to Mr. Jones, my secretary, for Mr. Rockefeller's eye, a memorandum in which I aimed to show to him, the to me amazing discoveries that I had made of the actual condition of medicine in the United States and the world as dis-

closed by Osler's book. I enumerated the infectious diseases and pointed out how few of the germs had yet been discovered and how great the field of discovery, how few specifics had yet been found and how appalling was the unremedied suffering. I pointed to the Koch Institute in Berlin and at greater length to the Pasteur Institute in Paris. It was either in this connection or a little later, for I kept up my inquiries on the subject, that I pointed out, as I remember the fact, that the results in dollars or francs of Pasteur's discoveries about anthrax and on the diseases of fermentation had saved for the French nation a sum far in excess of the entire cost of the Franco-German War. I remember insisting in this or some subsequent memorandum, that even if the proposed institute should fail to discover anything, the mere fact that he, Mr. Rockefeller, had established such an institute of research, if he were to consent to do so, would result in other institutes of a similar kind, or at least other funds for research being established, until research in this country would be conducted on a great scale and that out of the multitudes of workers, we might be sure in the end of abundant rewards even though those rewards did not come directly from the institute which he might found.

Mr. Rockefeller was at this time in Cleveland. I myself soon took a very long trip of a month or so to the Pacific Coast on business. I never saw my memorandum again. But that Mr. Rockefeller was impressed by the force of these considerations I have documentary evidence. These studies and memoranda, as I have said, took place in the summer and fall of 1897. In January 1898 an event took place which discloses to us the effect of these considerations on Mr. Rockefeller's mind.

Mr. Rockefeller had begun the work of founding the University of Chicago with a gift in 1889. The University opened its doors in 1892 and for five years had been rapidly expanding. The outlines of the proposed Institute of Research were, of course, only vaguely drawn in our minds at the time of its inception in 1897. We did not know whether the Institute would be practicable or possible. I had indeed received encouragement from such friends as I had addressed on the subject, notably from my physician in Montclair, Dr. J. S. Brown. What counsel, if any, Mr. Rockefeller had taken, I do not know but it is quite certain that our earliest conceptions associated the proposed medical institution with some great institution of learning and with some great medical school. Mr. Rockefeller was interested in the University of Chicago, as founder, and it was in his mind, as it was in my mind, that the institution of research would be associated, if the idea were ever realized, with that young and flourishing institution.

In 1894 an attempt had been made to associate the Rush Medical College in Chicago with the new university. The matter was discouraged by Mr. Rockefeller in 1894, and the interviews and correspondence on that subject culminating in 1895 led Mr. Rockefeller to suppose that no further

attempt would ever be made to associate Rush Medical College with the University of Chicago. To Mr. Rockefeller's very great surprise, and to mine, for I had been privy to all the previous negotiation, we were informed in January 1898 that official action had just been taken affiliating Rush Medical College with the University of Chicago. The practical effect of this association would be, as we foresaw it, to make Rush Medical College the medical college of the University of Chicago. The history and the ideals of Rush College at that time rendered it an unsuitable basis on which to rear an institute of research. Accordingly, under Mr. Rockefeller's direction, I immediately addressed a letter of earnest dissent to the authorities at Chicago. This letter was dated January 12th, 1898. As the letter was important and as our relations with the University of Chicago were extremely intimate, the letter, before being sent, was very carefully considered and was scrutinized, in advance of its being sent, by Mr. Rockefeller personally. Among other things, the letter contained the following pregnant and significant passage:

"I have no doubt that Mr. Rockefeller would favor an institution that was neither allopath nor homeopath, but simply scientific in its investigations into medical science. That is the ideal. For that the University should wait and reserve the great weight of its influence, authority and prestige, instead of bestowing the same gratuitously on Rush Medical College. Such an institution would have to be endowed and would run on a far higher principle than the principle of Rush College or any other of the ordinary institutions."

In the original letter, and in the letterbook copy from which I take this quotation, these words were made the central point of the entire letter by running along the margin of each side a heavy line. My purpose was to intimate to Dr. Goodspeed, official secretary to the University, to whom the letter was addressed, that this passage contained for him a pregnant meaning. It intended to intimate to him that if he would quietly wait, the founder would probably endow an institute of research in connection with the University of Chicago. Mr. Rockefeller understood the implication of these words of course as well as I did and he not only permitted, but ordered the letter to be sent with all the implications that it contained. This discloses clearly the fact that at that time the idea of an institution for investigation had already taken root and was germinating in Mr. Rockefeller's mind. This was January 12th, 1898, within six months after my studies in Osler's *Principles and Practice of Medicine.*

Dr. Goodspeed promptly replied, undertaking to justify and excuse, and perhaps to minimize the action of the University. This led to a second letter still more insistent than the first one. This letter was dated January 19th, 1898, just one week later than the first letter. In that letter was a

passage still more significant than the one from the first letter which I have quoted above. The passage is as follows:

"The whole effect and tendency of this movement will be to make Rush ultimately the medical department of the University of Chicago, as against that far higher and better conception, which has been one of the dreams of my own mind at least of a medical college in this country, conducted by the University of Chicago, magnificently endowed, devoted PRIMARILY TO INVESTIGATION, MAKING PRACTICE ITSELF AN INCIDENT OF INVESTIGATION and taking as its students only the choicest spirits quite irrespective of the question of funds. Against that ideal and possibility, a tremendous if not fatal current has been turned; I believed the ideal to be practicable and I hoped to live to see it realized."

You will observe here that the central thought is an institution for investigation. An institution in which whatever practice of medicine there is shall be in itself an incident of investigation, that while I said this was a dream of my own I qualified by saying "at least of my own mind", implying that it might also be a dream in another mind, and I not only stated that I believed it to be practical, but I added that I thought it was possible. This letter, with all these implications, passed under the very critical eye of Mr. Rockefeller. He understood the implications perfectly, he knew perfectly well that those who read that letter, although I signed it, would understand and give just the same significance to it as if he signed it himself. I was acting as his secretary, if not his amanuensis in sending it. This statement, therefore, of a week later, reveals still more clearly that the idea of an institution of research had taken such possession of Mr. Rockefeller's mind and that he was prepared to endorse the quasi public committal to it which is made in this letter.

But from this time forward Mr. Rockefeller never associated the proposed institute with the University of Chicago. I, for my part, while I still continued to cherish the idea, of an institute of research, found it impossible to pursue it in an effective way owing to the multiplicity of other absorbing duties. The matter, however, continued to be referred to and conferred upon for a year or two, particularly with Mr. John D. Rockefeller, Jr., who shared all my interest in it. Any active steps toward founding the institution would involve extensive conference with the leading men of research in this country, a study of the history of similar institutions in Europe and an amount of thought, correspondence and travel, that might well engage a large part of the time of a competent man. I therefore suggested one day to Mr. John D. Rockefeller, Jr., that we employ a man for this exclusive service and suggested a man entirely qualified whom I thought we could command for such a service, my friend and neighbor in Montclair, Mr. Starr J. Murphy.

After my enumeration of Mr. Murphy's rare qualifications, Mr. Rockefeller was so far impressed that he met Mr. Murphy and arrangements were made for his undertaking the work. For several years there was no other thought than that the proposed Institute of Research should be associated in the traditional way with some powerful University. Indeed, negotiations were for a time actively conducted with Harvard, but in the end it was thought best that the Institute be wholly independent, and time has justified the wisdom of this decision.

The story of your conferences and correspondence with the leaders in medical research in the United States, the first cautious tentative experimental gift made under their expert counsel, the gradual confidence of your advisers, and the final splendid scheme, awaits your own authoritative and graceful pen.

Very truly,
F. T. GATES

Mr. Starr J. Murphy,
61 Broadway, New York

CHAPTER 2

The Young Physician

Think for a moment, sir, of the situation in which we physicians are placed. We have no legislative chambers to enact laws for us. We are our own lawgivers, or rather we must discover the laws, on which our profession rests. We must discover them and not invent them; for the laws of nature are not to be invented.

Dr. Paul C. A. Louis to Dr. James Jackson, March 22, 1833

Rivers: When I went to the Hopkins I swore to myself that I would never stand first in any class again. I was first in my class at Emory, and I want to say that not missing a class and getting a summa cum laude put a pretty severe strain on me. I was determined when I reached the Hopkins not to get mixed up in such a thing again. Well, I made lots of efforts during my first two years to avoid such a goal, and I must say that I raised bloody hell. I was certainly no model of good deportment and behavior, but I did pass my examinations, and I did try to increase my knowledge about the practice of medicine. I didn't make any effort to do research, and in the end I stood first in my class. Now that didn't mean that I was the smartest in my class—I was far from that—it only meant that the fellows who had plenty on the ball were devoting their time to research instead of preparing for ordinary everyday quizzes. If they hadn't taken on the extra burdens that research entailed, I would have easily been beaten out.

One of the advantages of standing first in your class at that time was that your interneship was assured and there was no problem where you would go. Top man had first choice of the kind of interneship that he wanted at the Johns Hopkins Hospital, second man had next choice, and so on down the line for the first thirteen men in the