

**National Institute of Allergy and Infectious Diseases
Oral History Interview Project
Interview #1 with Dr. Robert Chanock
Conducted on January 11, 2001, by Peggy Dillon
at the National Institutes of Health, Bethesda, Maryland**

PD: Good morning, Dr. Chanock.

RC: Good morning.

PD: I'm meeting with you today to have a series of roughly chronological interviews that will chronicle your career and contributions to NIAID. I also hope that these interviews will serve as the starting point for other oral histories and to use them to help the Institute preserve its more than half century of history. So I'd like to start by going back a bit to your background prior to coming to the National Institutes of Health. I'm particularly interested in the events and circumstances that led you to pursue medicine and research. Let's start by having you tell me about your background, your family, and your education leading up to medical school.

RC: My father was a very successful businessman in Chicago who did not go past the sixth grade. When it was time for me to consider college, he wasn't very interested. In fact, he wanted me to go into business with him immediately. But I knew that that was not what I wanted. Interestingly, we lived one mile from the University of Chicago. In those days, there were no college counselors in the high schools and there was nobody to advise me. My father didn't want me to go to college and my mother was neutral. So, I got on my

bicycle and I rode to the University of Chicago because it was our local university. I didn't know that it was one of the greatest universities in the world. I was very lucky. I knocked on the door and I said, "What, how does one enter the university?" And they said, "Well, you'll have to come back in a few weeks and take a test and if you do well on it, we will accept you." So I took the test. I must have passed it because I was allowed to enter the University of Chicago after I graduated from high school.

I was most interested in biologic sciences, particularly genetics, but I had no intention of being a physician. That decision was made for me by the United States Army when I was drafted in 1943. After I had taken a number of tests I was told that I was going to be a doctor. First, I had to matriculate in a medical school that would accept me. I was told that I would be in the Army during my medical studies. The war was raging with full intensity at that time and the Army was concerned about not having enough doctors.

PD: Then that decision to become a doctor was made for you?

RC: It was made by the United States Army. And I chose the Medical School of the University of Chicago. It was an accelerated course lasting three years; there were no vacations. We were under tremendous pressure, obviously, to do things faster and faster and faster, so that we could be certified physicians who would serve the army. This was before the Normandy invasion and all of the events that led to the end of the war.

In medical school, I chose well. I was most impressed by the quality and humanity of the pediatricians who were on the faculty and hence I chose to go into pediatrics. The Chairman of Pediatrics was Francis Howell Wright, who had been appointed to his position very early in his career. He was a wonderful man, a perfect role model. He was one of the greatest persons I have ever known. He had spent several years at the Rockefeller Institute prior to coming to the University of Chicago. He knew Albert Sabin quite well because Albert had a laboratory on the same floor and they were good friends who talked and socialized often.

When the war ended I was in my junior year. The army said, "We have no further use for you," and overnight I became a civilian. I finished my last year and a half of medical school as a civilian. After graduation I did my internship in the Bay Area of California at the Oakland County Hospital. The reason I went there was because the hospital director was a physician, G. Otis Whitecotton, who had just retired from the University of Chicago where he had been director of its hospitals and clinics. Actually three members of our medical school class went with him. This was one of the most fortunate things that ever happened to me. A class friend of mine who I had known for many, many years—we went to camp together every summer and so forth—said, "You must look up so and so"—I forget the name now. She was a student at Mills College in Oakland. Mills College was famous for its strength in the arts, dance and music and other humanities. I don't know if you know about it?

PD: No. I just think I know where you're going with this. Go ahead.

RC: When I had a free night I drove to the campus and asked various students if they knew where this woman lived but nobody knew her. After I had visited each of the dormitories, I was preparing to leave the campus when I saw this beautiful apparition, of a woman of extraordinary beauty and poise, who had been dancing vigorously for many hours, dripping with sweat with pig tails ajar. She was absolutely incredible. I asked her but she didn't know the person I sought either. I said to myself, "Well, I don't want to continue the search for the other student, I'm going to call this young woman to see if she would go out with me." Unbeknowst to me I had a real problem. My father had purchased one of the first cars that General Motors produced after the war for my mother; it was an Olds '88 creme-colored convertible. It was the longest car I had ever seen; it had the same chassis as the largest Cadillac. My mother did not like the car and gave it to me. When I called the beautiful dancer she didn't want to go out with me, period; but I persisted. Later I learned that she did not want to have anything to do with me because I had playboy written all across my forehead. Who else but a playboy would drive such a car? [Laughter] Nonetheless, we met, dated, and were married during my internship. This is a picture of us a year after our marriage. You can see why I said beautiful apparition.

PD: So when did you meet her and when did you marry?

RC: We met in September and were married in February.

PD: Really? And what is your wife's name?

RC: Beth. Beth Osgood Chanock.

PD: And you were married in February of . . .

RC: '48. I graduated in '47. So, that was the most wonderful thing that's ever happened to me.

PD: That's great.

RC: So here's the picture.

PD: Thank you. This is pretty good resolution. So, after you were married, where were you in your career and your housing?

RC: Well, we couldn't find housing in those days because it was very tight in the Bay area. G. Otis Whitecotton liked me because I came from the University of Chicago and he allowed us to live in the basement of the nurse's home. We lived there for the remainder of the year. And then she went to New York and Connecticut to take classes with Martha Graham and other dance luminaries; she was a dance major. She had actually been

accepted to the University of Chicago after graduation from high school, but her parents wouldn't let her go there because it was too liberal. She wanted to go to the University of Chicago because you could be on a fast track and she could get her education over with in two years and then start dancing full-time. There were only two schools that offered a major in dance. One was Bennington and the other was Mills College. She chose Mills College.

After finishing my internship, I came back to Chicago as a resident in pediatrics with Howell Wright and she went off to spend the summer with Martha Graham to learn more about modern dance . . . she was in Martha Graham's class and then she went to Bennington where they held the Modern Dance Festival. So we didn't see each other for almost three months. And then we had a real, official honeymoon that lasted about four days. [Laughter]

PD: Where did you go?

RC: Well, we went to Taos, New Mexico, because in the three months between the time that I graduated from medical school and the time I started my internship, I worked in Taos in the Indian pueblo and in an Hispanic clinic in Taos itself. I loved New Mexico. I loved Taos. This was before it had become highly commercialized. And so we went there on our honeymoon and it was incredible. We had a wonderful time.

PD: Very remote at the time.

RC: One of the really great stories about our honeymoon had its origin before I met my wife.

The arrangements for my employment at the Taos pueblo and the Hispanic clinic were made by Ted Puck. He is a world-famous geneticist. He established the fact that there are 46 chromosomes in the human genome. Initially, he had been a very famous biophysicist at the University of Chicago. We became good friends when he came to the medical school to aid the war effort by developing procedures for inactivation of bacteria in air. The army was very interested in this during the war. Ted knew the people in New Mexico who had lived with D. H. Lawrence, after he left England.

His wife, Freda, had been the wife of a prominent banker in Germany. She left her husband to marry D.H. Lawrence. They moved from England to New Mexico because they were not accepted in his native land. He set up a huge home there because he was a very successful author. But there were two other women who absolutely adored him, so it was a menage a quatre. And one of Ted's friends was named Spud Johnson. He was D. H. Lawrence's confidant. Ted Puck knew Spud Johnson very well. Spud Johnson took me under his wing. Through him I met Lady Brett, a well known British painter, who was one of the women in the menage who pursued Lawrence the whole time. The third woman was Mabel Dodge Luhan, heir to the Dodge fortune. She was now on husband number three, a full-blooded Taos Indian named Tony Luhan. Nonetheless, she was an integral part of this group. Lawrence's wife, Freda, had died before I came to Taos. Incidentally, she was a cousin of Baron von Richtoven, the Red Baron.

PD: Yes.

RC: When we were in Taos on our honeymoon, I took my wife, Beth, to see Brett. Beth thought she was a lovely woman. We went to her ranch that was about half a mile from the road. While we were having tea, there was a large cloud of dust on the road, created by a car coming toward us. We could see this cloud from half a mile away. As the car came very close to the house, Brett said, "Oh, Stephen, I want you to meet some friends of mine." Stephen was Stephen Spender, the poet laureate of England. [Laughter]

PD: They were just coming in . . .

RC: Oh, no, he was coming to see her. So we spent the afternoon there.

PD: Wow! That's a great honeymoon story.

RC: Yes, it really was.

PD: Let's go back to the trajectory of your early career. At this point, where were you?

RC: I was a resident in pediatrics and the defining moment that set the direction of my career occurred during my residency. One of the most recent faculty members in the

Department of Pediatrics was Albert Dorfman. He had been three years ahead of me in medical school, but already he had a Ph.D. in biochemistry and he was a world-famous biochemist. But he wanted to study biochemistry as it related to pediatrics. He was the attending physician, i.e., the person I reported to. One night during the first winter of my residency, his daughter was admitted to the hospital and she almost died. She had croup, obstructive croup, and they had to do a tracheotomy and put a tube in her trachea so she could breathe. Later it was necessary to place her in a ventilator. It was touch and go. She could have died at any moment. And fortunately she recovered.

And at that time, I thought that the medical faculty at the University of Chicago knew the answer to everything. However, nobody knew what caused croup. It wasn't caused by a bacterium that could be cultivated on an agar plate. And nobody knew what this was all about or how it had happened. So, I went to the library and found many scientific papers that described acute laryngotracheitis as "viral" croup but no one had ever identified a virus that could produce this disease condition. This was true also for "viral" pneumonia and bronchiolitis. These conditions constituted the triad of serious lower respiratory tract disease in infants and children. Well, that really started me thinking, particularly during winter when pediatric wards fill up with pediatric patients with either severe diarrheal disease or severe lower respiratory tract disease.

At that time severe diarrheal disease was mostly non-bacterial. It wasn't the result of infection with cholera or other enteric bacterial pathogens that are important in the

developing countries. Nobody had any idea of what caused serious lower respiratory tract disease. In the context of the whole world situation, respiratory diseases and diarrheal diseases are major causes of mortality. So there was a void, a total void in our understanding of the etiology of pediatric enteric and respiratory infectious diseases. All a physician could do was to support the patient. With diarrheal disease, you could infuse fluids and salts to replace that which had been lost. In the early part of the century, diarrheal disease killed an awful lot of infants and young children in the US. But the biochemists who studied diarrheal disease were able to develop strategies for replacing fluid that had been lost and replacing the salts that were lost, the body because can't tolerate a salt imbalance very long. You can't have too much potassium or too little potassium or that sort of thing. So fluid replacement allowed infants and children who were brought to the hospital to survive. If they stayed home, they died.

PD: Okay.

RC: Also, I became very interested in respiratory diseases.

PD: That was the defining moment that made you want to study it further.

RC: That's right.

PD: And that was in 1940?

RC: That would be in 1948 or '49.

PD: Okay, tell me about this picture.

RC: Well, I'll lead up to the picture. After I finished my residency, Howell Wright said to me, "You need more discipline. I'm going to send you to somebody who will teach you how to do research and make sure that you do it in a disciplined way."

PD: He thought you were promising, but not disciplined?

RC: Correct. So he sent me to Albert Sabin, who, at that time—this would be 1950—was not working on polio. He was working on insect-borne encephalitis viruses. And he was working on toxoplasmosis. He had discovered the role of toxoplasmosis in congenital disease. Specifically he described the severe damage it does to the fetus in utero. This infection is one of the major causes of congenital anomalies. He had isolated dengue virus and sandfly fever virus during World War II. He was an incredible man. So in 1950, I went to his lab. The next year, he was elected to the National Academy of Sciences before he had started working on polio. All of the things that he had done leading up to polio were sufficient for his election to the Academy. This was at a time when there was no official pathway for biomedical scientists to be elected to the Academy.

PD: How was he elected then?

RC: Well, they called him something else. They sort of shoe-horned him into another category. And that was true for Bob Huebner and Charles Armstrong and Karl Habel, former chiefs of the LID.

PD: And yourself?

RC: No, the Academy had expanded by the time I was elected. By then there was a quota for biomedical scientists. I worked for Albert for two years and it was amazing, we hit it off and I was able to survive. He was very severe and very demanding and the many, many people who came to his laboratory for training left after a few weeks or a few months. They just couldn't take it.

PD: Could you tell me a little bit about what his discipline was like in the lab?

RC: Well, he monitored everything. The laboratory was on a single floor that had a long hall with laboratories on either side. And as he was going home—his office was at the very end of this long corridor—he would go into each lab and review what each person had done that day. Their notebooks had to be open and he would work his way down the long hall before he'd leave the building. He knew everything that had happened during the day and you would hear about it the next morning if things weren't just right. He was

just terribly rigorous. This extended to analysis of data; he wrote papers that were magnificent in terms of clarity and systematic organization. And if you didn't meet his standards, you were in for it. It was very difficult, but I survived.

PD: And you, in fact, caught his attention, according to this interview.

RC: That's right, yes.

PD: Well, what was it about your work that caught his attention?

RC: Well, I didn't screw up too much. [Laughter]

PD: You did what he asked you to do in your work?

RC: No, I did what I should have done. One day, I really screwed up terribly and his response will give you a small insight into Albert Sabin and the great confidence he had in himself. I was very depressed and he came in my laboratory and put his arm around my shoulder and he said, "Bob, don't be depressed, you know, I made a mistake once."
[Laughter] Fortunately, I didn't say, "only once," I just kept my mouth shut. On the whole, we got on well together or about as well as anybody had ever gotten on with him. At the end of my first two years with Albert, I had to pay my dues for the army support

of my medical education during World War II. I was drafted into the army as a medical officer.

PD: Right.

RC: The first people to be drafted were those who had been supported by the Army, but hadn't served in the army. I was assigned to the Far East, to Korea, to a M.A.S.H. hospital.

PD: Could I just stop you there for a second and could you just tell me a little bit more on tape about how Dr. Sabin suggested that you pursue a certain line of research?

RC: That comes later.

PD: Oh, okay, go ahead.

RC: That's in this picture.

PD: Okay, you're going to the Far East.

RC: Yes so I spent two years in the Far East at . . . actually, I was assigned to a M.A.S.H., of the type you have seen on television.

PD: Yes.

RC: You know what a M.A.S.H. is? The army established a special M.A.S.H. [unit] for treatment of a disease called Korean hemorrhagic fever. It was frightening because it had a high mortality. All of the significant biomedical scientists in microbiology had come over to Korea and attempted to isolate an etiologic agent but none was successful. I was assigned to this M.A.S.H. and I kissed my wife good-bye. We had a three-month-old son at the time. We were here in Washington because I was sent here to Walter Reed Medical Research Institute to prepare for my research in Korea.

When I got on the plane, I didn't expect to see them for at least two years. The Walter Reed laboratories actually staffed a huge laboratory—406 Medical general laboratory—in Tokyo that functioned as the base laboratory for the United Nations in the Far East. It was a seven-story building, Mitsubishi #7. It was three blocks from the Imperial Palace in the center of Tokyo, and that's where I worked during my tour of duty in the Far East.

The reason I worked there and not in Korea was a fortuitous appendectomy that was performed the night before our group was to leave for Korea. During dinner I developed an acute abdominal condition. It was clear that I had to do something but I was a jerk because I was having dinner at the Sergeant Major's—the top Sergeant of the Laboratory—home. And I felt this sudden severe pain but I didn't want to say anything

because I thought it might reflect adversely on the Sergeant Major's wife's cooking. So I just sat there and attempted to be as comfortable as possible.

And, finally, I couldn't stand it and a friend of mine who sat next to me transported me back to the BOQ, Bachelor Officers' Quarters, and then called for an ambulance from the Tokyo General Hospital, then an army hospital. Surgery was performed shortly thereafter. My appendix was about twelve inches long and it was ready to burst. Recovery was rapid and the next day I was walking. And then I found out that the army regulations stipulated that after abdominal surgery, you had to remain in the hospital and receive convalescent care for 21 days. But I'm due to go to Korea. Well, I had to stay. The group went over. They sent somebody else from Walter Reed and I was assigned to the Tokyo laboratory for the rest of my tour. I did research at the 406 Lab, and I worked on topics that I had studied when I was with Albert.

PD: What were some of them?

RC: The major focus was insect-borne encephalitis viruses. We did studies in Japan, not possible previously or since.

PD: The Tropical Virus Laboratory, was it called?

RC: No, it was called the 406 Medical General Laboratory.

PD: Okay.

RC: The virus we studied was Japanese B encephalitis virus (JE). This virus is present all through the Far East, Japan, Formosa, The Philippines, Korea, and India. And the army was very concerned about JE as a cause of disease in the military. Fortunately, I had studied JE during my two years with Albert through an arrangement with Imperial Palace and we were allowed to study JE in the Emperor's private duck preserve. This ecosystem represented a complete microcosm of JE pathogenesis and natural history of transmission of virus in its natural avian reservoir by its natural insect host, a culicine mosquito. It was incredible.

The preserve was outside of Tokyo and most Japanese did not even know it existed. The emperor's facility was called the Shinhama Imperial Duck Refuge. It was a small lake with several inlets where water flow ended at a little house that had a slide protruding from it. And on either side of the inlet was a large mound of earth. When the ducks heard a bell, they would swim up the inlet to the little house at the end of the inlet where the food came down the slide. The emperor entertained his friends by bringing them to this place. They hid behind the dirt mound and when the ducks came swimming by, they would stand up and net them. This was considered great sport.

The refuge was surrounded by a bamboo thicket where the two types of birds that are the most important as the natural hosts of Japanese-B encephalitis, the Snowy Egret and the Blue Heron, nested undisturbed. Nobody bothered because they belonged to the Emperor. Nobody was allowed within miles of this place. Also, it was absolutely idyllic. No one else was ever allowed to visit; we dropped down by helicopter. Our access to the site was exciting because we traveled by helicopter. Because the research program involved prospective surveillance of mosquitos and birds we sampled these components of the JE ecosystem on a regular schedule. As a bonus, we scheduled a picnic for many of the visits. We were privileged individuals. Our time together in Japan was one of our most enjoyable experiences. Wives were allowed allowed to participate. Beth enjoyed it immensely.

PD: She came over?

RC: After three months, she came over and we rented a small Japanese house that had to be approved by the Army.

So, I came back to Cincinnati. Now, I had the distinction of being the only person to ever return to Albert Sabin. Two years and I went back for another two years.

PD: At his invitation?

RC: Yes.

PD: Okay.

RC: So we come back on a troop ship.

PD: In 1962 now?

RC: This is '54. We come back on a troop ship that required thirteen days to cross the Pacific. Then across the United States, back to Cincinnati. On arrival, Albert informs me that I was to accompany him to the International Conference on Poliomyelitis in Rome in five days. My wife, child, and mother-in-law had settled in a seedy hotel. We hadn't been able to find an apartment, but he wouldn't take no for an answer, so I went on the trip with him.

PD: Five days after your return.

RC: That's right. My wife was seething. After several weeks she found a wonderful apartment and was well settled in when I returned after my three week trip. Almost all the scientists going to the Poliomyelitis Congress were on the USS Independence, the pride of the US Maritime Service. For example, John Enders, who received the Nobel Prize for discovering how to grow viruses in tissue culture, as well as his two associates

who shared the prize with him, Fred Robbins and Tom Weller, were on the boat. Jonas Salk was on the ship as well as Basil O'Connor, the man who founded the National Foundation for Infantile Paralysis, so-called Polio Foundation. He was FDR's law partner. Also many of the therapists from Warm Springs where FDR went. He actually died there.

PD: In Georgia.

RC: Yes, in Georgia. We were on this ship for thirteen days because we stopped at all of the various ports in the Mediterranean, such as Marseilles, and all of the large Northern Italian cities and finally reached Rome. A very interesting thing happened on the boat. I shared a table with Albert. This was the U.S.S. Independence, the pride of the American fleet, at the time.

It had the record for transatlantic travel and it was able to go across the Atlantic faster than any other boat and it was the biggest boat built for the U.S. Maritime fleet. So I couldn't afford to go on this boat but everybody was going first class. Fortunately, I was awarded a traveling fellowship from my father. I shared a table with Albert and Morris Shafer, a friend of Albert's for many years who later was Commissioner of Laboratories for New York City. He was a wonderful biologist and storyteller. In his retirement, he worked as a consultant to the FDA. The three of us had a table and we could count on Morris to provide an unending parade of very funny stories.

Early in the trip, something very interesting happened in the personal dynamics on board. The dining room on this boat was large and luxurious, but it was very tightly controlled by the Maitre d'. He assigned each passenger to a table and there were special times when you could eat. So we were sitting at our table the first night out and we saw the very dashing man sitting by himself. He could have been played by Cary Grant if they made a movie of his life. Well, it turns out that this was Alexis Lichine, the guru of French wines at that time, I mean, absolutely, the guru. He had escaped from Russia with his family, came to the United States, learned about wines as an employee of Frank Schoonmaker, who was the American guru of all imported wines of that time. This was the late forties, early fifties. So Alexis Lichine then decided that he would have his base in Europe and he convinced David Rockefeller and the man who owned the "21 Club" in New York to sponsor him by providing him with funds for the purchase of two second growth chateaus in Bordeaux, which is the wine capital of the world. Alexis Lichine was the most influential figure in the exportation of French wines to the US. And he wrote the *Encyclopedia of French Wines* and he was the guru of French wines at that time. At that time he was unchallenged. We noticed that an extraordinarily beautiful woman came into the dining room, went straight to the Maitre d' and pointed to Lichine. What she said to him, obviously, was "I want to sit with that man."

PD: Oh, they didn't know each other.

RC: But she wanted to know him and she ended up at his table but he didn't like it at all. She had been a dancer on Broadway, had married a very rich man who died a few years after they were married. Subsequently, she met and married a cotton millionaire who was in the inner circle of King Farouk of Egypt. She had just divorced her cotton millionaire and was looking for her next husband. Wherever he went, she followed. She selected Lichine and tracked him relentlessly.

And one night Albert and I were sitting in the bar and Lichine came running by. He turned to us and said, "If that woman comes after me, tell her I went that way," and he went in the opposite direction. In this way he was free for the night. The next day, he came over to our table and said, "You saved my life last night. Tell me what can I do for you?" We didn't know who he was. He said, well, "Let me invite you to my chateau and teach you about French wines," and we said, "Oh, that's wonderful," not knowing who he was.

Ultimately, we reached Rome in time to attend the opening ceremonies of the Congress. Scientifically, Albert's presentations were well received. He described his derivation of the first candidate live attenuated poliovirus vaccine strains. He had accomplished this extraordinary feat during the two years I was in the army. It was clear that Albert was well on his way to the development of a successful live oral poliovirus vaccine. As I will describe subsequently, this rapid success in vaccine development provided the setting for my emergence as an independent scientist. After the Congress I spent two days in Florence because I had never been to Italy before. Then I flew to Madrid and after a few

days we traveled to Bordeaux. Our host had two chateaus, Chateau Pierre Lichine and Chateau Lascombe, both were a "second-growth chateau." There are only five "first-growth chateaus" in Bordeaux, a designation made more than 150 years earlier. Lichine drove down from Paris with his current girlfriend. And there she is [points to picture].

PD: Oh, my.

RC: And this is Albert and this is me.

PD: Wow! What a great picture.

RC: Yes. During our wine post-graduate course, we went with Lichine from chateau to chateau. When we entered a chateau with him, everybody stopped what they were doing and escorted us through the wine cellar. We tapped various kegs and they opened bottles of wine I could never aspire to purchase. For four days, we did nothing but taste wines and talk about wines. Lichine's current girlfriend was very impressive. In other words, she had a very good outlook on life. The woman on the ship was very light-headed. It turned out that the best friend of Lichine's current amour was the woman on the ship. We discovered this during a dinner table conversation. It was incredible and I have no idea where this led. [Laughter]

More important, on the European trip, Albert and I were together a long time under very relaxed conditions, a state that never existed in Cincinnati. He kept saying to me, “What do you want to do when you grow up?” He advised me, “You’ve got to do something different than what I’m doing because I think I’ve got the poliovirus vaccine under control. I want you to decide what you want to do.” After a number of such discussions, I responded that I wanted to search for viruses that caused serious lower respiratory tract disease, based on my experience with Al Dorfman’s young daughter.

PD: Okay, thank you. Now, who are the people in the picture?

RC: Well, this is Albert Sabin and I don’t know her name. I don’t remember it now.

Subsequently, Lichine married Rhoda Fleming, a flaming red-headed movie star beauty of that time.

PD: And this is 19 . . . ?

RC: 1954, yes, that would be like August.

PD: And this is in Bordeaux?

RC: That was in Bordeaux at one of the chateaus of Lichine.

PD: Okay. And just before we go ahead, what era is this picture of you and your wife?

RC: Oh, that would be 1950.

PD: So you go to this conference with Dr. Sabin and all these other scientists.

RC: Oh, yes. We talked. We traveled extensively together and he kept probing to determine what I wanted to do. When I described my plan, he said, “That’s great. That’s wonderful. I will provide you with all the resources.”

PD: Back in Cincinnati.

RC: Back in Cincinnati. My wife established us in a wonderful apartment near the Children's Hospital Research Foundation.

PD: You were all settled by the time you returned.

RC: Yes. Amazingly, she didn’t divorce me. He provided me with all the laboratory support—funds, and actually established a clinical liaison with pediatricians in the hospital. The Cincinnati Children’s Hospital was one of the most highly regarded academic centers for pediatric research. The Children’s Hospital Research Foundation was the laboratory arm of the Hospital. So I was able to have access to clinical material.

PD: You're fine. You can drop it actually. Although, actually, why don't you hold that thought because we're almost at the end of the tape.

[End Tape 1, Side A]

[Begin Tape 1, Side B]

PD: Okay, so you were telling me about being set up in your laboratory.

RC: So I decided to study croup because that was really the centerpiece of my interest when I was a resident because of Al Dorfman's daughter. We collected specimens from a number of patients with croup and in short order, we isolated the first human parainfluenza virus I designated the virus as croup-associated (CA) virus. Later it was classified as Type 2 parainfluenza virus. It was the first human parainfluenza virus isolated. So now we, I had my foot in the door. I had a virus that was associated with croup. I spent the next two years with Albert searching for other viral respiratory tract pathogens. At the end of the second year, I decided that I had to establish myself as a totally independent scientist. At that point, I received a very attractive offer from Johns Hopkins School of Hygiene.

PD: After you spent two years with Dr. Sabin.

RC: Two years with Albert, yes.

PD: And he tried to get you to stay, didn't he?

RC: Absolutely.

PD: What did he, what was his rationale?

RC: Well, there was plenty of space and support. He actually received so much money from the National Foundation [on] Infantile Paralysis, that he couldn't spend it all. He turned much of it back to the Foundation. Also, the Endowment of the Children's Hospital Research Foundation was very large. Funds were provided primarily by Proctor and Gamble. Cincinnati is a Proctor and Gamble town. This corporation is the centerpiece of the whole city. Every year they increase their donation. So, he said, "You'll never have to worry about money." And I said, "I really want to try to do this myself." And so I accepted the offer and I moved to Johns Hopkins. Scientifically, it was a home run with the bases loaded that year because that's when we isolated the first human strain of respiratory syncytial virus (CRSV), which proved to be the most important single cause of serious viral lower tract respiratory disease in infants and children worldwide.

PD: How did that come about?

RC: Well, I set up a study at the Baltimore City Hospital that was staffed by the faculty of the Johns Hopkins Medical School. I set up a clinical liaison with the Pediatric Department at the Baltimore City Hospital, a city hospital. A good friend of mine from medical school, who was one year ahead of me, Lawrence Finberg, was the Deputy Chief of Pediatrics.

PD: Small world?

RC: Yes it was. He was actually the pediatrician for our children during our time in Baltimore. Anyway, I was there for about nine months, and it was very successful. But the person who had recruited me was a little more problematic.

PD: Shall remain nameless or can you say?

RC: Needless to say, I was so concerned about his conduct that I went to the Dean and I said, "This man is engaged in fraudulent research. Many of the things he's doing are just not true, not correct. What he has written has come more from his imagination than from the laboratory," and I was absolutely convinced of it. And I was told by the Dean that I had to submit my resignation.

PD: Really?

RC: That was before scientific fraud was up on most radar screens.

PD: So that's how they dealt with the problem, by asking you to leave.

RC: Yes, so I left. And this man actually was caught with his hand in the cookie jar about fifteen years later and was summarily dismissed. Fortunately, I met Bob Huebner at a national meeting shortly after my dismissal. I presented the croup parainfluenza virus story at the meeting. He came up behind me afterward and tapped me on the shoulder and said, "Would you like to come to NIH?" In a millisecond I said, "Yes." That was it.

PD: Where and when was this meeting?

RC: It was a meeting of the New York Academy of Science in March or April '56.

PD: And you had met him once before very quickly, right?

RC: Yes, and this is an interesting story. Albert Sabin did not have good things to say about very many scientists, but one of the scientists he thought was really outstanding was Bob Huebner. When I was in Washington in 1953 preparing to study Korean Hemorrhagic Fever, Albert came to town several times. Each time he came to see us. Each time he said, "Is there anything I can do for you?" And I said, "Yes, I would like to meet Dr. Huebner." And the reason I said that was, just before I had left Cincinnati, I saw a

television documentary drama based on an article that appeared in *The New Yorker*, by Berton Roueche. He wrote the best series of stories about medical detectives and how they solved medical mysteries. The story of Bob Huebner and the discovery of rickettsial pox appeared in *The New Yorker*. I had not read the *New Yorker* story but I did see its dramatization on the Philco Playhouse, one of the first national television presentations. This program was broadcast across the whole United States. It wasn't filmed, it was live. And later, I'll give you a copy of the *New Yorker* article.

PD: Wasn't it "The Alerting of Mr. Pomeranz" that was dramatized?

RC: Yes. That's it, exactly.

PD: Okay.

RC: You have that.

PD: I have it . . . I only have mention of it. I don't actually have what the document was.

RC: I'll give it to you. It's an incredible story.

PD: Okay.

RC: Bob Huebner had spent the war as a Public Health Service (PHS) Medical Officer on a Coast Guard cutter in the Alaskan area. He had never done any research at all. When he returned to the United States, Public Health Service assigned him to PHS Headquarters. I think he had a PHS scholarship, and he interned in the Public Health Service Hospital at Seattle. He was assigned to Department headquarters where the Surgeon General had his office. The story he told of this experience is just incredible. The Surgeon General was a very famous man, I think it was Thomas Perrin, who made his mark as an expert in sexually transmitted diseases. He was a very famous doctor. He developed a carbuncle on his posterior. Bob Huebner was working in the clinic.

PD: Oh, my.

RC: And this was very soon after penicillin had been in general use. Initially, many injections were required each day to maintain a therapeutic level of the drug. Later a depot formulation was developed. Penicillin was mixed with beeswax. This mixture had to be heated to liquify the formulation and it had to be injected immediately before it solidified again. Otherwise it froze in the syringe.

The Surgeon General called for some help from the clinic downstairs and they sent Bob up. Bob looked at the carbuncle and said, "Yes, we should give penicillin." "Fine, bring it up." Bob said, "It's going to be very difficult because by the time I go up on the elevator or run the stairs, it's going to solidify." He says, "No, I will not go down to the

clinic.” So, Bob heated up the syringe that contained this beeswax penicillin. After it had been liquified, ran up the stairs and injected the antibiotic as rapidly as possible. The formulation solidified after three to four attempts. Finally, Bob was successful because he had established an olympic record for stair climbing. The Surgeon General’s response was, “I think I’d better send you to a post where you can do research rather than deliver clinical care.” So that’s how he ended up at NIH.

PD: It is?

RC: Yes.

PD: And that was his inauspicious start here or?

RC: What happened was, very shortly after he arrived here, the senior members of the staff took leave. They deserved it because most of the senior staff members of our Institute served as preventive medicine officers in the military overseas for four to five years. They hadn’t had a vacation until the end of the war so this place was just bare. A call for help was received from the New York City Health Department. The only person around was Bob Huebner.

The call came from New York because a newly recognized infectious disease was of some concern because it was spreading through the Bronx and nobody could figure out

what it was. The NIAID was asked to send us your infectious disease authority. Well, he was the only one available. So he flies up to New York and he is filmed at the airport:

“A Public Health Service Expert Arrives to Aid in the solution of this mysterious disease condition.” What is it? What’s causing it? Bob visits a huge housing development that appeared to be a central focus for this disease. It was called Q Gardens. Patients with the disease had a high fever and they developed many skin vesicles. The vesicles, or blisters, were unlike those of smallpox or chicken pox.

During his visit to Q Gardens, Bob saw a small man moving from person to person in the halls and in the meeting rooms where experts from all over the country had congregated.

Microbiologists and public health experts came because they wanted to contribute to the solution of this medical mystery. This little man would go up to one of these famous microbiologists, talk to that person for maybe 30 seconds, and then suddenly, the person he was talking to would turn around and walk away. Bob Huebner was fascinated, so he went to this man and said, “Who are you?” He replied, “I’m Mr. Pomeranz.” And Bob said, “Well, that’s very nice. What do you do, Mr. Pomeranz?” He said, “I’m an exterminator.” That’s why nobody would listen to him. Bob said, “Well, that’s interesting. What do you think is going on here?” Pomeranz replied, “I think it’s a disease that’s transmitted by mites that are so small you can barely see them. Let me show you why I think so.” He took Bob into an empty apartment and peeled back some wallpaper and there were millions of mites in the wall. The walls were infested with mites.

PD: And they were moving.

RC: Yes that is why they could be seen. Sunlight was reflected by the slowly moving wallpaper. Bob scooped up some of the mites into a tube. He also collected specimens from patients and then returned to NIH where he inoculated eggs. Voila! He recovered the agent responsible for the disease. It proved to be rickettsial. In fact it was the last rickettsial disease to be identified.

PD: Was this very soon after he came to NIH?

RC: Yes.

PD: So was this indicative of his talent as a field epidemiologist?

RC: He had talent to burn but equally important was the fact that he had an open mind. Very few scientists have a mind that is totally open. He would listen to everything that was said to him and then he would evaluate it. He didn't turn away because somebody was an exterminator. This was one of his greatest strengths. He was brilliant and he could galvanize others to join him in a large collaborative effort.

PD: Shall I turn this off for a second?

RC: Yes.

PD: Okay.

RC: This will be out-of-order. This is a picture of Bob Huebner talking to or more correctly persuading the Deputy Minister of Health of the Soviet Union, Victor Zhdanov. This is Bob in a characteristic pose. He was charismatic and he was able to bring many people together to collaborate in a large research project.

PD: And what, about what era is that picture?

RC: This would be about, about mid-sixties.

PD: Dr. [Victoria] Harden may want to clarify some of these with you. I'll put this over there. Shall I just write it down?

RC: Yeah, sure, it's mid-1960s.

PD: Okay.

RC: What I'd like to do is give you is something you don't have.

PD: This is a letter that Dr. Sabin wrote?

RC: To Bob Huebner on his retirement. He was diagnosed as having Alzheimer's disease at that time, '82.

PD: Okay. It's an amazing letter.

RC: It's an amazing tribute by a man who really had very little good to say about most other scientists.

PD: It's only high praise.

RC: Yes. I wrote a letter at the time also.

PD: Okay, let me put these down. So these are letters from you and from Dr. Sabin on behalf of Dr. Huebner when he retired in 1982.

RC: Right.

PD: Tell me about some of the things that made him such a remarkable person and these letters allude to his ability to keep people in his laboratories . . . there wasn't a lot of turnover in his lab.

RC: No.

PD: How did he engender that sort of loyalty?

RC: He inspired, he energized, and he brought people together to, in cooperative endeavors, which were almost unheard of before that time. He was totally different from Albert Sabin. I had two different types of mentorship. Albert Sabin was the lonely scientific giant. He had very few people in the laboratory. When I arrived at his laboratory in 1950 he had a staff of two; himself and his technician. He knew everything about the various activities in his laboratory. Much of the work he did himself with his own hands. He also analyzed the data himself. In other words, he exercised total control.

In contrast, Bob Huebner was one of the great early entrepreneurial scientists, who brought a large group together and trusted each member of the group to do what they were supposed to do. He did this by including controls that allowed him to determine whether bias had entered into the equation or whether everything had been done properly. He had maybe twenty, thirty, forty people working for him and they were all loyal and dedicated. Usually, each member of the group performed beyond his or her expectations. He energized others and made them feel good about themselves. He made them feel that they were doing things that were important. Clearly, he was able to create a happy, harmonious sort of research environment. He was undoubtedly one of the first great entrepreneurial scientists. I say this not in a pejorative sense, but entrepreneurial in its finest guise.

PD: In the sense of getting people to bring out the best in themselves?

RC: Yes. His trust of associates came in part from the fact that he had taught these individuals how to perform in the laboratory and he had structured the studies in such a way that if something was a little askew, he would recognize it.

PD: That's a classic [looking at photograph of Dr. Huebner].

RC: Well, that's what he looked like much of the time. Also, as I mentioned before in the retirement letter, he often had haunting thoughts himself about whether things were going right or if he was doing the right thing. This picture typifies his period of haunting doubt.

PD: Tell me about that picture.

RC: Well, Bob is enumerating something and he's not quite sure of himself. But here, he's positive.

PD: Where was this taken?

RC: No, these were taken here in the lab. This was taken in Russia. This would be mid-1960s.

PD: May I write that down?

RC: Sure. Haunting doubts. Bob had more than a full life. He had enough life for a whole battalion. Here's a picture of Bob guiding an Angus bull on his farm. He and his wife and nine children lived on a 300-acre farm just a few miles from Frederick, which is about 40 miles from NIH.

PD: Yes.

RC: They had sixty Angus bulls. These were highly reasured breeding bulls, very valuable animals. Each of his children owned one or more bulls and the ultimate sale of these animals provided the money for their college education. His wife, a former nurse, worked on the farm full-time. This picture shows Bob coming back from the laboratory and exercising this bull.

PD: With a tie on, even.

RC: Yes.

PD: What era is that?

RC: This would be the mid-'60s, also.

PD: Okay, let me just write that down. On a farm just outside of Frederick?

RC: Yes. Very close to Frederick, Ijamsville, I think, is where it is.

PD: Talk to me more about his work as a field epidemiologist, because it seems that research has moved away from that exclusive fieldwork to more laboratory-based work.

RC: Bob emphasized combined laboratory/field projects. For over 60 years LID's research goals initially articulated by Armstrong—Bob Huebner's lab chief—and Huebner have remained the same, namely: (1) delineation of the etiology, pathogenesis, and epidemiology of medically important viral diseases, (2) identification of immune determinants of resistance; and (3) the development of means for control of these diseases which usually means making vaccines. Often these projects are not selected because of their public health importance. As a consequence the projects are commonly high risk as well as high yield. In the pursuit of long term goals, LID scientists are allowed, in fact enjoined to pursue an important infectious disease problem from beginning to end. This means they must master most or all of the approaches and technology required for successful pursuit of such a broad objective. Major projects have varied in duration from five years—adenovirus vaccine—to more than 30 years—RSV vaccine.

Such a continuum of research effort by LID scientists during my time in the laboratory resulted in the discovery of many important agents of acute respiratory tract disease, namely, the adenoviruses, RSV, the four parainfluenza viruses, and Mycoplasma pneumoniae—originally thought to be a virus. Equally important was the discovery of the etiologic agent of a major cause of hepatitis, namely, hepatitis A virus as well as the identification of two other important viral hepatic diseases, namely non-A, non-B hepatitis—now known as hepatitis C—and hepatitis E. Also, hepatitis D was shown by Dr. Purcell and colleagues to be a defective small RNA "viroid-like" virus dependent upon hepatitis B virus for its replication. As a consequence, a specific HDV vaccine is not required because immunization against HBV also protects against HDV. It should also be noted that Dr. Purcell provided the first experimental evidence that immunization with highly purified HBV surface antigen induced complete resistance in chimpanzees to HBV challenge. Other significant viruses were discovered by Dr. Kapikian, namely the Norwalk calicivirus family. These viruses were the first human gastroenteritis viruses and have proved to be the most frequent cause of epidemic, institutional, familial, or mollusc-associated, non-bacterial gastroenteritis.

The list of pathogens for which licensed vaccines or imminent vaccines have been developed in LID include: the epidemic adenoviruses, HAV, the human rotaviruses, RSV, parainfluenza type 3 virus, and influenza A and B viruses. In addition, 40 years ago we developed the first effective antibiotic therapy for M.pneumoniae. More recently, pooled human IgG—tradename "Respirgam"—and subsequently a humanized murine

monoclonal antibody directed at the RSV F glycoprotein—tradename "Synagis"—were licensed by the FDA for use in prevention of severe RSV disease in high risk infants and young children. This successful strategy of passive immunoprophylaxis had its foundation in observations we made in the mid-1980s during our study of immunological determinants of resistance to RSV in the lungs of cotton rats. During a strong inverse relationship between titer of serum RSV neutralizing antibodies in recipient animals and the level to which virus replicated in their lungs following RSV challenge. These studies also identified the amount of neutralizing antibodies required for effective passive immunity to RSV in the lower respiratory tract.

PD: I wonder if you could talk a little bit more about Bob Huebner's talents of bringing together all of those various facets.

RC: By the time that I arrived, Bob had already discovered rickettsial pox. Also, he had established the role of certain Coxsackie A viruses in herpangina—summer sore throat—as well as the role of Coxsackie B viruses in pleurodynia—an extremely painful infection of the pleura, the covering of the lungs. He and Wally Rowe had discovered and characterized the adenoviruses, which was the first group of viruses to be identified as being important in human respiratory tract disease since the discovery of the influenza viruses, around 1931. Also, Wally Rowe had been the co-discoverer of human cytomegalovirus. Then, when I came, Bob decided to turn over the responsibility for the respiratory virus studies to me and he moved into cancer research where he made a tremendous impact. For example, he articulated the first two editions of the oncogene

hypothesis. The third edition confirmed what he had predicted. Confirmation was achieved by Michael Bishop and Harold Varmus, who were awarded the Nobel Prize. They cited Bob as having played an important role in the discovery of oncogenes. They cited Bob as being very important in the discovery of oncogenes.

He also discovered what T-antigens that are tumor-associated antigens present in cells transformed by a tumor virus. These antigens are produced very early in the infection and constitute a signal that cell transformation was the result of infection by a specific tumor virus. These proteins are not part of the virus, but they are involved in the replication of the virus. Thus, they are considered to be nonstructural proteins. T-antigens played a very important role in elucidating many of the properties of virus-induced tumors. T-antigens and their antibodies can be tested for very quickly by immunologic tests. Thus, one can characterize a tumor and determine whether it is caused by this or that virus just by testing for the T-antigen because the antigen is only expressed by transformed cells that are cancerous. This strategy for linking a tumor virus to human cancers proved to be very helpful during initial attempts to link known animal tumor viruses to human neoplasms. All of these were negative and to the lack of an association was confirmed by more sensitive techniques. During these early studies he established complex collaborations with many other groups. He also set up many laboratories for other scientists all over the country. After a while, this system of laboratories became the core of the Virus Cancer Program of the NCI. Clearly, this very successful program was Bob's creation.

PD: So when you joined the laboratory, what are some of the specific ways that he had an influence on you? What happened after you got here?

RC: We talked extensively every day. Bob had a 45-mile trip from NIH to his farm, so many nights he slept over in his office. He had a big easy chair that pulled out into a bed. In those days, a guard visited our building every two to three hours during the night. The guard would turn a switch indicating that he was in the building and that everything was all right. Then he would continue on his rounds to another building. If the guard saw Bob asleep on his bed he would tap his feet, wake him up, and say, "You're not allowed to sleep in the building." And Bob would agree and then go back to sleep. The guards usually woke him up two or three times every night but he just rolled over and went back to sleep.

In the morning when I came into the lab, Bob would be in his shorts walking from room to room saying, "Who's got some coffee? Who has a cigarette?" Incredible guy. He had nine children, and he lived on a farm. As a consequence there was no formality in the laboratory or anywhere else for that matter. It was when Bob roamed the LID [Laboratory of Infectious Diseases]. It was a very joyous time. I never saw him in a vindictive mood. He was never vengeful or excited about what other people said of him. If they didn't believe what he was saying, it didn't bother him because he had great confidence in his abilities to generate, integrate, and evaluate scientific data. He elicited

extraordinary personal loyalty. Everybody liked and admired him. He was a wonderful, harmonious adult who also happened to be a world-class scientist.

PD: You started in this lab in 1957.

RC: '57.

PD: Did you start the same day as Dr. Kapikian and Dr. Parrott?

RC: Al and I came into the building the same day, July 1st, 1957. This was also the day Bob Parrott left NIAID to become director of Children's Hospital.

PD: Was that coincidental?

RC: At that time you entered the Public Health Service on July 1st.

PD: Okay, I see. So, after you joined the lab, you started working to develop, with Dr. Huebner, an adenovirus vaccine?

RC: Yes. I did that, but we also sought to discover other previously unknown viruses that might be the cause of important human diseases. Initially, we discovered the human respiratory syncytial virus (RSV) that proved to be the single most important cause of

severe viral lower respiratory tract disease in infants and young children, worldwide. And within a year and a half, we discovered the other two important parainfluenza viruses. Type 3 Parainfluenza turned out to be second only to RS virus as an important cause of serious lower tract disease. Para 1 proved to be the major cause of croup. Para 2 causes croup but is less important than Para 1. So we filled in 40 to 50 percent of the whole lower respiratory tract disease pie within a period of about ten years, from '53 to '63. Essential to the success of this program was the foresight of Bob Huebner and Joe Bell, who was the Chief of the Epidemiology Section, when they initiated a prospective study of the microbial experience of infants and young children who were residents of a welfare nursery, Junior Village. Joe Bell was Al Kapikian's mentor and a great epidemiologist.

These infants and children were wards of the court because their parents were either in jail, had been killed, were on drugs, or were not able to care for them for other reasons. So, from 1953 to '68, 15 years, this laboratory spent a tremendous amount of time and effort studying a semi-closed population in time of health and in time of disease. This prospective approach provided incredibly informative data because every infant or child that was studied was his or her own control. This long-term prospective study yielded a veritable cornucopia of new viruses and new epidemiologic insight for known viruses. Wally Rowe, the co-discoverer of the human cytomegaloviruses, defined many aspects of their natural history during the Junior Village study. Similarly, many basic features of the natural history of RSV and parainfluenza virus were defined during the Junior Village

study. At that time Leon Rosen discovered new reovirus serotypes, numerous higher type adenoviruses, and new interoviruses in specimens collected at Junior Village.

PD: And it's hard to set up that kind of a controlled environment?

RC: Well, I became Chief in 1968 and the Junior Village was dissolved by the District of Columbia and all the infants and children were sent to foster homes.

PD: The Junior Village study.

RC: Junior Village was incredible. We studied RSV outbreaks as well as paraflu epidemics and adenovirus epidemics.

PD: And it takes off from there?

RC: We discovered that even though adenoviruses manifest themselves predominantly as causes of febrile respiratory tract disease, after about a week or so, the virus is cleared from the respiratory tract, but it continues to grow to high titer in the intestines without causing symptoms.

PD: It just keeps growing there.

RC: These observations are particularly important when considered in the context of the failure of an inactivated adenovirus vaccine. An inactivated adenovirus vaccine had been developed for use in the military and it had to be withdrawn after about a year because there were virus contaminants in the vaccine that were potentially dangerous and these viruses could not be removed because the adenovirus could not grow in the cell culture that was used for vaccine production unless the major virus contaminant—SV 40—was present. SV 40 was required for replication of adenovirus in monkey kidney cell culture because it provided certain functions that contamination of the inactivated adenovirus vaccine by SV 40 required that the vaccine be withdrawn. So we were left without a vaccine for one of the major causes of severe acute respiratory tract diseases in the military. About half of the recruits lacked protective antibodies that are most important in causing epidemics. All of these serotypes susceptibles become infected during recruit training and 20 percent require hospitalization (i.e., 10 percent of the entire recruit population).

PD: So it's pretty debilitating to a large population.

RC: Yes. This important form of acute respiratory tract disease was studied extensively and shown to be the result of an infection with a filterable agent later shown by Huebner and Rowe to be an adenovirus. So, early in the morning, Bob Huebner, in his shorts, or maybe not in his shorts, and Bob Couch, and I would discuss this important febrile respiratory tract disease. Bob Couch had just come to NIAID and was to work over in

the Clinical Center, but his lab was not ready. So he was assigned to us for two years. The three of us would sit around in the morning and talk about adenovirus vaccines. The strategy that we agreed to was the result of a group gestalt. We sat around and talked and we reviewed the adenovirus epidemiology studies at Junior Village. It was clear that adenovirus, at least the adenoviruses that are important in respiratory disease, grow in the intestines for a long time without causing disease systems. So, we said, would it be possible to immunize with a live virus vaccine by introducing virus directly into the intestines where it would grow without spreading back to the respiratory tract and cause disease?

This seemed a possibility because of a detailed analysis of an ongoing study of the natural history of adenovirus infections in infants and young children in Junior Village indicated that most of these important respiratory tract viral pathogens also produced an extensive and prolonged infection of the lower intestinal tract without producing symptoms in this region. This suggested to us that it might be possible to develop a unique strategy for immunization based on selective intestinal infection. Adenovirus was enteric-coated so that it was not released until it passed through the stomach. Bob Couch and I actually enteric coated gelatin capsules containing adenovirus. We rolled the capsules in a shallow pan containing enteric coating solution. So it would be fair to say that our fingerprints were on the vaccines from the very beginning.

Adenovirus did indeed remain localized in the intestines and did not spread to the respiratory tract where disease ordinarily occurs. Also, infection was totally asymptomatic and did not spread efficiently to close susceptible contacts. We demonstrated that the live enteric virus vaccine was completely protective during large adenovirus epidemics in the military. It was licensed by FDA in 1980 and it has been given to tens of millions of military recruits without incident. Unfortunately, this success proved to be one of a kind because we were unable to apply this strategy to any other respiratory tract pathogen.

PD: Tell me, was this unique in . . . as a vaccine?

RC: Yes, it was unique and it remained unique because we tried it with every other virus and it didn't work, only with adenovirus.

PD: Interesting.

RC: Yes, so our first time at bat, we hit a home run.

PD: And it's still used or has it continued to be used in the military?

RC: We did studies in the Marine Corps down at Paris Island and we showed it produced a silent immunization, no symptoms. Virus was shed from the intestine and interestingly

enough, it didn't spread from vaccinee to close contact. In the military, they're really crunched together as recruits, especially in the Marine Corps. Nonetheless, under these conditions we recovered virus only from the vaccinees, not from the contacts. They had no symptoms and we never recovered virus from the upper respiratory tract. When these vaccinees were studied during a subsequent epidemic, they were completely protected against adenovirus disease. The vaccine was 100 percent effective. In contrast, Marine recruits who received the placebo were subject to a 10 percent hospitalization rate.

PD: And is this still in use?

RC: Well, it's a sore point. The vaccine was licensed in 1980. It took about ten years to license it. Before that, it was tested in all of the services under an IND—investigational new drug—submission. It was the biggest IND in the history of the world.

PD: IND?

RC: Investigational New Drug. In other words, you're studying it as an investigational drug and, of course, it was under the control of FDA. Studies were performed in all the camps and every person who had a respiratory disease was sampled. It cost the Department of Defense a bundle, but it was used routinely and there were not epidemics of adenovirus. The company that manufactured this material manufactured it in a defunct facility and

they were told repeatedly they were out of compliance and if they didn't shape up the license would be taken away from them.

PD: Oh, no.

RC: The product is licensed by the FDA and the facility had to be licensed also. A few years ago, the company discussed the problem with the Department of Defense and said, "We need help and will you pay for a new building?" And the DOD said, "No, we won't." This became a game of chicken. You know, it's when two teenagers come at each other in a car down the road and one of them has to swerve and if they don't swerve, they both die.

PD: Before you tell me the cliff hanger, let me switch to a new tape.

RC: Okay.

[End Tape 1, Side B]

[Begin Tape 2, Side A]

PD: So, you were telling me about this collision course.

RC: Well, what happened was the factory was abandoned by the manufacturer because it was out of compliance. The Department of Defense refused to support the building of a proper facility and for three years now, there's been no vaccine and there are major outbreaks of adenovirus disease in every recruit camp.

PD: So this has happened for the last three years because the production facility closed down three years ago?

RC: It had to be closed down.

PD: Oh!

RC: And the Department of Defense is now trying to find a contractor who will make the material in an approved facility. I went to an international meeting on respiratory diseases a few weeks ago and a Navy captain, who is a preventive medicine officer summarized the current status of acute respiratory tract disease in the military. He is just beside himself. The military has reverted to a position reminiscent of World War II.

PD: Is there a hopeful outcome to this story?

RC: A new Administration is in place and it is unlikely that they could repeat the errors of the previous Undersecretary for Health in DoD.

PD: Talk to me about what the building was like when you got here.

RC: It was very crowded.

PD: State of the art?

RC: It never was state of the art. The building was built because there had been a number of problem disease outbreaks in Building 5, which is right across the street. For example, there were several outbreaks of Q fever. This is a rickettsial that causes very acute and very serious disease. The organism is highly infectious when present in a small particle aerosol. And unless you have centrifuges that are absolutely tight and you preclude the possibility of aerosolization, you'll have such outbreaks in the laboratory where you're studying Q fever. Because of the large Q fever outbreaks that occurred in Building 5, the decision was made to construct an absolutely tight biosafety building. Building 7 was to be the ultimate showcase bio-safety building.

PD: Just after World War II, wasn't it?

RC: It was actually occupied in '48. About two to three months after the building was occupied, Joe Smadel—the man who was the head of infectious disease research at Walter Reed and was later to become Director of NIH for Intramural Research—came

into the lobby of the building, spent a few minutes there, and then walked out, came down with Q Fever shortly afterward. This was only one manifestation of a large Q fever outbreak in Building 7.

PD: That was right after it was opened.

RC: Yes. What they found was that individual laboratories were totally secured except for one oversight. They forgot to secure the space around the pipes that ran through the building and from one floor to another. The defect was identified by a very simple test that should have been performed at the time the building was accepted from the contractor. The NIH engineers set off a smoke bomb in the sub-basement and 15 seconds later, the smoke was in the attic.

PD: Oh, my.

RC: The water fountains had a number of water pipes that traversed the building and none of these pipes were secure. One of the engineers showed me how turbulent the air flow was under the water fountains. He held a lit cigarette lighter that had a strong flame close to the underside of the fountain and the flame was extinguished immediately.

PD: Everything was moving all over.

RC: Fast, very fast.

PD: Yes.

RC: Thus, this model biosafety facility was not a safe building. For this reason, we are allowed to work on Class 3 or Class 4 agents. We can only study agents that do not pose a serious threat to health.

PD: Somebody just put the . . . so what did they do? Were they able to retrofit the building in any way?

RC: It can't be done. You would have to tear the building apart. It's not the building that they thought it would be. It's just a BL-2 building, just like a biochemistry lab. We had huge incinerators in every cubicle in every room. All outflow air was incinerated. People outside the building were safe, but the scientists in the building were at risk.

PD: How ironic.

RC: So, we're moving into a new facility in May or June and we're going to have a P-3 facility. Three small rooms will be sealed so that nothing can go from one room to another and all that sort of thing. We will have a P-3 facility for the first time in 44 years.

PD: Tell me what a typical day was like for you back when you first joined the lab.

RC: Long.

PD: Come in at a particular time?

RC: I'd come in in time to see Bob Huebner wake up and stroll out looking for coffee and things to eat and we'd just, we'd talk then. And then we'd stay, you know, to five, six, seven o'clock, whatever. But it was fun. It was fun working in the laboratory. That makes a big difference.

PD: Were you mostly in the laboratory or did you, what percentage of your time was spent in the field or . . . ?

RC: Well, I didn't go to Junior Village because Al Kapikian was in charge of that. There were three nurses assigned to the project, epidemiologic nurses, and two helpers who took temperature changed the diapers. We provided all the care there and a pediatrician under contract was there full-time. So these infants weren't just notes on the nurses record. Every child was seen by the pediatrician daily.

PD: Okay.

RC: I would like to provide you with an extensive review paper that summarizes our observations and interpretations of the Junior Village study.

PD: So when you would spend a day in the laboratory, a typical day, was there lots of collegiality and comparing results?

RC: Yes, it was a very healthy, happy laboratory group. For example, I hired Doris Wong six months after I came to the LID. That was almost 44 years ago. Most of the people have stayed on and become old timers.

PD: Dr. Kapikian.

RC: Yes. Dr. Al Kapikian came to LID the same day I arrived. Bob Purcell came next, probably about '62, '63. And Brian Murphy came '69, while Peter Collins has been here for 16 years. Sue Emerson has been here for about 10 years now. She came because she married an NIH scientist. She had been a tenured professor at UVA. John Patton has been here for four or five years, leaving a tenured position at the University of Miami working on rotaviruses.

PD: But would you say the longevity is unique to this laboratory?

RC: No, it's not unique. There are other labs who have had people stay a long time.

PD: And then in 1959, you were promoted. What happened at that time?

RC: Well, it was a funny situation. Now the hardest thing in the world to achieve is tenure at the NIH. When I came in '57, anybody who stayed more than two years was tenured. I mean there wasn't any such thing as tenure. The lab chief just made the decision, this person is good, we'll keep him. Now, you spend half your time tenuring people. It's a very complex, very demanding, and sometimes a very arbitrary process involving letters from people outside, committee after committee.

PD: It's like the academic tenure process?

RC: It is the academic tenure process. A successful candidate must be equivalent to tenured scientists at Harvard, Yale, or Stanford.

PD: But when you were here in '59, that wasn't the case and so you became head of the respiratory virus section.

RC: Yes.

PD: Was that, were you singled out for your accomplishments or was that the natural advancement at the time?

RC: No, I was chosen because I was interested. Bob Huebner and Wally Rowe were directing their research towards persistent infection and tumor biology. Leon Rosen who was here, an absolutely superb scientist, who had an M.D. and Ph.D., was interested in entomology as well. Leon Rosen had an interesting history. He received his M.D. from the University of California, San Francisco and later received a Ph.D. in Public Health at Hopkins. He was interested in filariasis, an insect-borne worm disease. It is a very serious disease that can lead to elephantiasis. Before coming to LID, Leon spent considerable time in the South Pacific, the outer islands, the Marquesas—French Polynesia—Islands due north of Tahiti. Actually these islands are located 200 miles north of Tahiti. The Marquesa Islands are the northernmost islands of French Oceania. And in those days, the boat came only every couple of weeks. He met his wife there. She was French Polynesian, a very lovely woman. Leon first saw her when she was in a dugout spear fishing for her family's dinner. This was a real-life "Tale of the South Pacific."

She routinely caught her family's supper by spearing fish under water. They were living here when I arrived but she wanted to go back to the Pacific. Leon convinced the scientific director of NIAID to set up a Pacific Research Station as part of NIAID's Tropical Medicine Program. His base was Honolulu University but he traveled and did field work throughout the Pacific. Leon then changed his schedule to include quite a bit of time at the Pasteur Institute after he retired from the Public Health Service. Now he divides his time between the Pasteur Institute and three homes in France, one right in the

center of Paris, another one south of Paris and then one very far out in the country. He loves France.

His home in Hawaii is now occupied by his daughter, who is a Baylor-trained pediatrician. He is an extraordinary guy. He has never been wrong about any major aspect of his research. He is very low-keyed, but he is the rock of Gibraltar. Whatever he says goes with me, unless proven otherwise. He has an abundance of credibility. You can be the smartest, most wealthy, most handsome, most adored, most looked-up to person, but if you don't have scientific credibility, you don't have anything. You start with that. He is very credible. Albert Sabin was almost never wrong. He was credible. Al Kapikian is the same way.

PD: Really?

RC: Bob Purcell as well.

PD: Would you like to talk about your colleagues some? I wanted to bring us up to the time that you became laboratory chief.

RC: The reason I became a laboratory chief was that by 1968, Bob Huebner was receiving much more money from the Cancer Institute than NIAID. He had a phantom system of labs all over the country working with him, all supported by the NCI. As a consequence, he transferred to the Cancer Institute and then I became Chief of LID.

PD: But there was a reorganization in there, wasn't there?

RC: The lab had approximately a hundred people, so it was split in half and Wally Rowe received one-half and I received the remainder.

PD: How was it determined that you would follow in Dr. Huebner's footsteps?

RC: I don't know. Other individuals higher in the NIAID's administrative staff made the decision.

PD: Did he come to you?

RC: No. Possibly Bob Huebner talked to the NIAID Director but I really don't know for certain.

PD: Were there other notable events that occurred while you were head of the respiratory virus section between '59 and then '68 that you want to talk about?

RC: Oh, yes. Well, there's a whole other story.

PD: Do you want to talk about it today?

RC: Sure.

PD: Okay.

RC: And this has to do with a condition called cold-agglutinin positive primary atypical pneumonia. It's a pneumonia that's not caused by a bacteria, that is, not by a standard type of bacteria. It isn't caused by the pneumococcus or any of the other bacteria that were known at that time to be important causes of pneumonia. And there is a very strange antibody that develops in response to this disease. It's an antibody which will agglutinate red cells when the temperature is dropped. These are cold agglutinins. So the official name for this form of pneumonia is cold agglutinin positive primary atypical pneumonia—PAP.

This type of pneumonia was the other major acute respiratory tract disease problem in the military during World War II. It was called by many people "walking pneumonia."

Individuals with this disease often were able to walk about but they experienced considerable fatigue. This type of debility could be considerable and it often lasted for several weeks to several months. Patients who were hospitalized had x-ray evidence of pneumonia but the extent of the symptoms was not proportional to the extent of x-ray pneumonia. The x-ray picture was often that of a minimal pneumonia, but symptoms were much greater than would have been expected from that x-ray picture. In many cases, patients with this condition experienced considered debility. It was sometimes

weeks or months before they were finally back to normal. This was a major problem in the military during World War II and the Korean War.

The Army created several commissions during World War II to address important health issues. Albert Sabin was a member of the Commission on Virus Diseases that studied dengue, sand fly fever, and encephalitis. There was also a Commission on Acute Respiratory Tract Disease, and they studied two diseases. One was ARD, acute febrile respiratory tract disease, and the other was PAP. In both instances the ARD Commission established that the disease was caused by a filterable agent. This was established during studies in volunteers. In ARD, they collected throat washings from patients, passed them through a filter that would exclude bacteria, but would allow smaller particles to go through. They tested the resulting filtrate in volunteers who then developed ARD. This suggested that the filterable etiologic agent was a virus.

PD: Okay.

RC: The same strategy was employed in the studies designed to elucidate the etiologic agent of PAP. Volunteers who had a filtrate of throat washings from patients with PAP introduced into their upper respiratory tract developed PAP. This established that the etiologic agent of PAP was a small filterable organism. Most authorities took this to mean that a virus caused PAP.

Monroe Eaton was the only scientist who got it right. He was a member of the Rockefeller Foundation and assigned to the California State Health Laboratory. During his studies of PAP on the military he recovered an agent using cotton rats that he thought was the cause of PAP. None of the authorities accepted his work. And each of these authorities had his own candidate but none of these proved to be the cause of PAP. Eaton had considerable difficulty in passaging the agent because it became overgrown with endogenous agents present in cotton rats. At the end of the passage series, Eaton ended up with an agent endogenous to cotton rats. He solved this problem in an ingenious manner.

He characterized all of these adventitious agents and then immunized cotton rats with them before inoculating these animals with PAP specimens. In this manner the cotton rats were rendered resistant to these adventitious endogenous agents. Using this strategy Eaton was able to achieve serial passage of the PAP agent—later called Eaton virus by man microbiologists—in cotton rats. Eaton's observations appeared promising to me but I was admonished by some of the most reputable, creditable, and authoritative people in the field, to stop working with Eaton's agent because it was scientific suicide. I was told this on several occasions, but I decided that we would make our own appraisal.

At that time we received paired sera from Marine recruits hospitalized with PAP. Every patient had developed antibodies to the Eaton agent. Two days later I flew to Beaufort, South Carolina, with Captain James Kingston, the Deputy Chief of Bureau of Medicine

of the Navy, responsible for medical research. We set up a large prospective study of PAP down at the Paris Island and Recruit Training Center. We studied several hundred cases of atypical pneumonia and the majority of them were positive for this agent. At this time the Eaton agent was still considered to be a virus. There had been anecdotal reports about patients with atypical pneumonia who responded when treated with an antibiotic. One or two patients and no controls.

And so we decided that we would determine whether these patients responded by performing a double blind study with about 300 consecutive cases of pneumonia at Paris Island. After the code had been broken it was clear that a tetracycline derivative turned off the disease whereas the placebo recipients remained ill for weeks. It was also clear that the Eaton agent was not a virus because antibiotics do not inhibit viruses. After eliminating a viral etiology, the only remaining candidate was mycoplasma. These organisms are vestigial bacteria that lack a cell wall. As a consequence, they are very mobile and very flexible, allowing them to pass through a filter very easily. After considerable effort, we succeeded in growing the organism on an agar medium and demonstrating that it was a mycoplasma which we named *Mycoplasma pneumoniae*. Confirmation that *Mycoplasma pneumoniae* was a human respiratory pathogen was provided by the results of serologic tests that employed this mycoplasma as antigen. Additional confirmation was provided during later studies in which volunteers administered *Mycoplasma pneumoniae* organisms cultivated on cell-free medium

developed early lower respiratory tract disease that was rapidly terminated by tetracycline therapy.

PD: So much for career suicide.

RC: *Mycoplasma pneumoniae* turns out to be a major cause of pneumonia in the military, but also in college populations and within the general population. It has a very unusual clinical picture in that the incubation period is usually about three to four weeks, an interval that is considerably longer than most respiratory tract viruses.

PD: Over how long a period of time did you . . . ?

RC: We solved the mystery of PAP in four years. We hypothesized that if one mycoplasma can be as important as *Mycoplasma pneumoniae*, other mycoplasmas should cause serious disease in other organ systems. We never found another mycoplasma that caused serious disease, so we closed the program and redistributed its resources to ongoing virus programs.

It is interesting that the first mycoplasma was discovered in France at the Pasteur Institute at about the time the first virus was identified. The first non-viral filterable agent was recovered from a disease in cattle, which is extremely important economically. This disease was called pleuropneumonia. Infected cows stop producing milk. They do not gain weight or produce much milk. This country has not had pleuropneumonia for a long

time. Control and eradication is achieved by slaughtering every infected animal. The agent of pleuropneumonia was discovered by Roux and Nocard working with Pasteur in 1900. In fact, it was one of the first filterable agents to be discovered. Its official name is now *Mycoplasma mycoides*. The U.S. has been free of the disease for many years and if it was ever reintroduced, it would be an economic disaster. It's interesting that we just finished the first human virus being yellow fever virus identified in 1899.

PD: And that was discovered in?

RC: It was discovered about 1899, by Walter Reed during the course of volunteer studies in Cuba, just after the Spanish-American War. Yellow fever was a big problem in Cuba during the Spanish-American war. This disease stopped the French from digging the Panama Canal.

PD: Right.

RC: The beginning of virology was concurrent with the identification and study of mycoplasmas. Mycoplasmas had an advantage in that they could grow easily on cell free-agar medium. Initially they were called pleuropneumonia-like organisms, PPLO. It's interesting that Albert Sabin discovered a number of what was then called PPLO when he was at the Rockefeller Institute. These were indigenous in mice and they would occasionally flare up and cause an arthritis that was similar to rheumatoid arthritis. A

group of scientists at the Rockefeller, headed by a well-known microbiologist, believed that they had discovered the cause of rheumatoid arthritis when mice inoculated with diseased tissue developed a rheumatoid arthritis-like condition. Albert Sabin did not accept the proposed association of a PPLO—later designated mycoplasma arthritis—with rheumatoid arthritis. Of course he was right but this did not stop [missing off bottom of page received from Dr. Chanock]. One of them, who ended up as Chairman of Medicine at George Washington University, never learned or understood what he had done.

PD: Wow!

RC: Albert Sabin knew what had happened because he was passaging various microbial agents in mice at the Rockefeller Institute and some of these mice developed isolated mycoplasma arthritis and showed that he could induce arthritis in other mice by inoculating this mycoplasma. So he was also a pioneer in mycoplasma research.

PD: Did that make you especially well-known in the scientific community?

RC: Yes! Because this was the age of accelerated discovery. However, it was also the age of accelerated intervention because we had developed the live attenuated enteric adenovirus vaccine. The vaccine worked like a charm. We also developed the first successful therapy for PAP—pneumonia caused by Mycoplasma pneumonia. RSV vaccine that

was prepared in the same way as licensed inactivated influenza virus vaccine was produced. The inactivated RSV vaccine did not prevent infection with RSV during the subsequent annual RSV epidemic. But worse than that was the fact that the infected vaccinees developed a much more serious disease than if they hadn't received vaccine. The observed potentiation of RSV diseases by the RSV vaccine cast a pall over all our vaccine efforts. Fortunately, we were finally able to solve how this happened and then we redirected our efforts towards the development of a live attenuated RSV vaccine rather than the formalin-inactivated vaccine. That's a whole story in itself.

PD: Let me turn the tape over for a second.

[End Tape 2, Side A]

[Begin Tape 2, Side B]

PD: Tell me about these pictures.

RC: This is a picture with Bob Huebner in the lab about 1965.

PD: Yes, I believe that's in that article, that biographical article about you. May I keep this?

RC: Yes, these are all for you.

PD: And this was?

RC: 1965, in the lab. Now this is a wonderful picture. It's three chiefs of LID and Albert Sabin. The picture was taken at the Hungarian Embassy in 1966. Albert was presented with a diploma indicating his induction into the Hungarian Academy of Sciences because Hungary had just finished a countrywide mass immunization campaign that completely eradicated poliomyelitis from the country. The Minister of Health came to the United States specifically to present Albert with his diploma indicating that he was a foreign member of the Hungarian Academy of Sciences. And that's me, and standing next to me is Dorland Davis, who was Director of the Institute at the time, and Bob Huebner. Note the woman with a pronounced widow's peak in the background, unquestionably my wife.

PD: Yes. That's a great picture.

RC: Yes.

PD: Okay.

RC: So there were three chiefs of LID, past and present, and Albert Sabin, newly elected member of the Hungarian Academy of Sciences. Now I'll show you several other things of interest. In 1961, I'd been at HAI a little over three years. The Secretary of the Department, believe it was called HEW then, decided that American virologists should

meet with and establish collegial relationships with their counterparts in Russia. This was 1961, Cuban missile crisis time. But the Secretary felt that this was appropriate and that it might relieve some of the existing tensions. This was actually shortly after Albert Sabin had established a productive collaboration with Professor M. P. Chumakov. He had gone to Russia to seek help in validating the safety and efficacy of his live oral polio vaccine. This could be done in the United States because the Salk inactivated polio vaccine had been licensed and it was not ethical to ask a mother to withhold a licensed vaccine in order to test an experimental vaccine. So he had to do these essential studies elsewhere.

He went to Russia about '56—and he surveyed all of the prominent virologists. Wisely he chose Professor Mikhail Petrovich Chumakov as the man. At that time he was the Director of the Poliomyelitis Institute in Moscow. He was, in my view, the most credible Soviet scientist at the time and Albert felt the same way. So Chumakov prepared the oral vaccine in his Institute. And the man who was in charge of production at that time told us this story last year. Chumakov was a huge man. He was about six foot four and paralyzed on his left side. He had been a member of the team that recovered tick-borne encephalitis virus—TBEV—in Siberia. This is a very dangerous virus classified as a biosafety level IV agent. While studying TBEV in the laboratory, he became infected and developed severe neurological damage, hemiparesis, and almost complete deafness.

PD: From it or in addition to that?

RC: From the infection. But he was able to do business because he had an assistant, Bella Kaplan, who would shout in his ear what other people were saying, or if he had something to say, he would tell it to her and she would translate it simultaneously.

PD: Wow!

RC: And so we went to Russia.

PD: HEW then.

RC: Yes, this was to be the first visit of a delegation of American virologists to Russia, 1961. A week before the mission was ready to leave, one of the members died suddenly, Jerome Silverton, Chairman of Microbiology at the University of Minnesota. Bob Huebner came into my lab and said, "You're going to Russia with me in five days." I was the junior, junior member of this wonderful group. There's Chumakov . . . and I found him to be totally credible. He's a giant, he was wonderful. And this is, Alexis Shelekov, who was a white Russian born in Manchuria. He went to medical school at Stanford and he was a member of the professional staff of LID at the time.

PD: Interesting.

RC: And, of course, Alexis was bilingual. On the trip we became close friends and have been so ever since, actually. And this is Bill Hammon, who was the dean of the School of Public Health at Pittsburgh, Ed Lennette was head of the California State Health Lab, one of the great infectious diseases laboratories in the U.S. He died two weeks ago at age 91. He was a magnificent person whose counsel was sought by many national and international organizations. And this is Fred Davenport, who was a professor at the University of Michigan and myself. This is Sergei Drosdov, who is now the Director of the Institute.

PD: And where was this picture?

RC: The picture was taken just outside of Moscow in a little town called Vnukovno, the site of Chumakov's institute. The main building of the institute had just been completed. These were very exciting and potentially dangerous times, the height of the Cold War and circa Cuban missile crisis.

PD: What an historic occasion.

RC: Yes, we were also at the May Day Parade in Red Square.

PD: Well, what did you think of them? I mean, were you impressed overall with the work they were doing?

RC: We were very favorably impressed by Chumakov's institute. However, the other institutes appeared to be second-rate.

PD: Okay.

RC: We landed in Moscow eight hours late because of a delay in Copenhagen. We flew in an United States carrier and then changed to Scandinavian Airlines for the final segment of our trip. The Moscow airport has a big restaurant on the top floor. The directors of each of the major institutes of microbiology in Moscow and Leningrad were at the airport to meet us. They had been sitting for eight hours at the bar waiting for us. By the time we arrived, they were a very happy group. Nonetheless, we had an extravagant banquet.

Very quickly we learned the benefits of abstinence. Unfortunately, it was not possible to remain abstinent because most of the banquet was devoted to toasts to world peace, international collaboration, more cordial Soviet-US relations, etc. Once you drink the first toast, you were committed to go the whole distance. None of us knew this so there were many happy US virologists at the banquet. Fortunately, Bill Hammon was a teetotaler. He didn't drink. He had been a medical missionary. He was a very straight fellow, but a wonderful man nonetheless. He turned his glass over. Bill Hammon took charge of driving our long ride to the center of Moscow. Henceforth he was our designated shepherd and tour guide for the rest of our month-long visit.

There was a very interesting exchange before we had drunk too many toasts. Several members of the Russian group were very interested in Bob Huebner because, he was an atypical scientist, he lived on a farm, had nine children, etc. So they said, “Dr. Huebner, we understand that you have just been elected to the National Academy.” Indeed, he had been elected just before this trip. The Russian scientists stated that when you were elected to the Academy of Sciences, your salary is tripled, you receive a large apartment in Moscow and a dacha in the country, and a limousine and driver. They asked “What did you receive, Dr. Huebner?” And he said, “A bill for \$25 for the Proceedings of the National Academy of Sciences, which is our science journal. And they said, “Oh, no, no, this is not possible.” They did not believe him.

PD: That there were no perks.

RC: Another interesting thing happened before too many toasts had been taken. They asked Bob about where he lived and he said, "I live on a farm about 40 miles from the National Institutes of Health." "What?" "Yes, I live on a farm that has a farmhouse built before the Civil War, pre-1860." "How is this possible? You're an academician now." You know, being an academician is really something in Russia. So, they said, “Tell us about it.” And Bob said, “Here, I’ll show you an advertisement that appears every month in the *Angus Cattle Breeders Gazette*—we breed these animals that are very valuable. He showed them the picture that appears monthly in gazette. It is the then advertisement for his farm, “The Bull in the Kitchen,” and it’s a picture of his kitchen in this old farmhouse

where two of his daughters are dressing, combing, and brushing an Angus bull prior to showing the bull at a agricultural fair . . .

PD: Grooming?

RC: Yes, grooming a bull, which is to be exhibited at an agricultural fair. And the purpose of this, was, obviously to present the bull in optimal condition so that it would be awarded a prize therefore increasing its sale price. None of the Russian scientists believed Bob. "Bull in the kitchen. We do not believe it." So, two years later, the Russian delegation visits NIH and Bob Huebner takes them out to the farm and has a bull in the kitchen awaiting their arrival.

PD: Were they beside themselves?

RC: They just all broke up and they finally understood what Bob Huebner is about. Now I gave you this picture of him running with a bull in scientific executive attire.

PD: Yes, in his suit. Not that one. Hold on, it's down here.

RC: He's exercising the bull.

PD: With his suit on.

RC: Yeah, that's right. You've got it.

PD: And here he is.

RC: When they saw the bull in the kitchen they really broke up. It was incredible. On the whole, our trip was very eventful. We visited all of the major institutes and our schedule was the same everywhere. We would go to the director's office and drink many toasts to international peace. It was hard to stay awake in the afternoon.

PD: Scientifically, would you call it a success, or was it more of an icebreaker?

RC: Well, we learned that Chumakov was the most credible person, which Albert had already established several years before. And we found that the level of science wasn't really that great. It was very disappointing.

PD: Okay.

RC: But we met wonderful people. We met one man who had actually studied at the Pasteur Institute just a few years after Pasteur had died. He dressed in a typical Louis Pasteur costume. He wore a small box-like hat that covered his head. This is the way Pasteur looked and he did his best to approximate this image. He did not speak or understand English. Alex Shelekov translated but he wasn't picking up what was happening.

Apparently, he was the most prominent scientist in the field of rickettsia research—Q fever and related organisms. We were with him for about an hour and nothing is happening. Then someone said to Bob, “Huebner, what do you think?” “Huebner!” He jumps up, he grabs Huebner, he kisses him on both cheeks, he says, “Oh, Huebner! The Father of Rickettsia Pox.” And then about half an hour later, someone said, “Lennette, what do you think.” He went, “Lennette!” He was one of the great figures in rickettsiology and Q fever research.

PD: Did he hug?

RC: Ed Lennette was much shorter than Bob, so our Russian colleague almost sent Ed into orbit. This was a most unusual experience because it was a nothing encounter until he heard a name and he suddenly realized who his visitors were. He started to jump for joy. Then hugged Bob and Ed and gave each the mandatory kiss on each cheek.

PD: Who he had in front of him.

RC: Yes.

PD: That’s great. That’s great. Gosh. Let’s look through these other pictures here, or did you want to finish up.

RC: Yes, I think I've told you most of the story. At a later date, I traveled to Russia several times and also met with many Russian scientists who represented the Russian Academy of Sciences. In 1988, I was asked to join the Subcommittee on Biologic Weapons Control, of the U.S. National Academy of Science, a subcommittee of the Academy's Committee on International Security and Arms Control—CISAC. CISAC was primarily involved in nuclear weapons control, but it was clear that biological weapons were also a real problem.

The Russians had sworn that they did not have a program for development and weaponization of biological weapons of mass destruction. But they did have such a program. And so for this reason the subcommittee was formed, mainly on the advice of Joshua Lederberg, then President of Rockefeller University. So I became a member of this subcommittee in '88 and about two years later, we traveled to Moscow to meet with our Russian counterparts. The Russian committee met with us for about a week. We then traveled to Leningrad—now St. Petersburg. My wife was with me on this trip.

PD: 1990, yes.

RC: Initially we met in Moscow at the Institute of Bioorganic Chemistry, whose chief was the Chairman of the Russian Committee and President of the Russian Academy of Sciences. Suddenly, on the last day of the meeting, they told us, "We're taking you to Leningrad." This turned out to be a very eventful meeting in Leningrad because a tremendous amount

of transparency had been generated, you know, regarding what the Russians were doing and BW was at all defensive and so forth and what the United States was doing.

And we met primarily with scientists who were not in the military. We met with civilian academicians and so forth, that were heads of institutes, that were relatively transparent. They published a lot of what they did and so forth. And we were, and we made, we were friends, we became, you know, close friends with some of these academicians who were very credible people. But nobody in the U.S. knew what was happening in the military institutes. And, for the most part, they still don't to this day. This is a major problem that impedes the control of biological weapons. We don't know whether an offensive biological weapons program is going on within the military institutes. And no one had ever gotten into a military institute.

When we arrived in Leningrad, they transported us to the Institute of Military Medicine. Incredible. We did not know where we were going. But when we arrived we knew where we were. We spent a day there and it appeared to us that they really weren't doing anything illicit or illegal. I'm here in the picture taken at the Institute. This is Alex Shelekov, there's Josh Lederberg, this is John Steinbruner, who you often see on television. He's a political scientist at The Brookings, whose specialty is the prevention of biological warfare, This is the Russian general who is the director of this institute. We're sitting with him and the requisite bottles of whatever it is in the center. Also, note the flags, the Russian and U.S. flags. And there we are again standing outside. There's

Josh, John Steinbruner, Tom Monath, myself. And we just couldn't believe what was happening. Here we were inside. We asked questions. They took us to places that we wanted to see and so forth. And I did not think they were doing anything illicit myself.

PD: You don't think that they were putting on a show of nonactivity?

RC: No, I think it was a very underpowered sort of organization.

PD: Okay.

RC: That was our impression. Based on discussions with many Russian scientists it is likely that our hosts had never had visitors before. Our Russian colleagues had never been inside such a closed institution. Indeed, they did not know of anyone who had ever been allowed entry. And here we were.

PD: And here you were, in them. You had a day.

RC: Yes, so we were leaving and we were going down the hall and the general comes up to us and says, "Stop, we must go to the end of the hall, we're not finished yet." And he opens the door to his large dining room and there was a banquet set up for us.

PD: Oh, how divine.

RC: This trip was also very eventful, because my wife was mugged while we were in Leningrad. We decided to see the Kirov Ballet because we had been there before on another trip. It was a trip circa 1966, which I made for WHO and she went on that trip with me and we had a wonderful time, mostly in Moscow and in Leningrad. On this trip, we secured tickets for the Kirov at the Intourict Bureau of our hotel. With our vouchers in hand, we went to the opera house to claim our tickets. My wife was about 30 to 40 feet ahead of me. She was just about to enter the opera house when a fellow wearing tennis shoes came racing by. He grabbed the strap to her purse, which she had over her shoulder and started to run off. She created an opposing force by pushing hard against the wall of the opera house. Within milliseconds it was clear that he could not run off with her purse so as he released his grip he pushed her toward the ground. She went right down on her face onto the concrete. Her face was abraded and quite raw. Although she was very shaken she insisted that we attend the ballet, which proved to be therapeutic. He picked on the wrong woman . . . but we saw the ballet.

PD: You did?

RC: She was messed up.

PD: She was injured and yet you stayed for the ballet.

RC: Yes. So that was quite an event. The only picture we have from that trip was one taken by a member of CISAC, which was meeting with Russian nuclear scientists at the same time that we were visiting the Institute of Military Medicine. A member of the CISAC contingent saw her the next day and took her back to the opera house where she had been assaulted. He photographed her, back to the wall, and looking very defiant.

PD: Yes, apparently. Well, we should probably start to wrap this up, but I wanted to get your recollections of what's in these photos on tape. So are there other pictures there you wanted to show me? We went through those.

RC: We had been studying mutants of influenza A virus that might be effective as a live attenuated vaccine for about six years when this picture was taken about 1970. The man on the podium was a famous Congressman, Paul Rogers. He was the head of the Health Appropriations Committee for years, wonderful man. He represented Palm Beach in Florida and Lee Chalab—seen in the picture as nearest to Rogers—was one of his constituents. His father also knew Rogers quite well, so the congressman invited our influenza virus research group to present our research findings to his committee. This report was printed in the *Congressional Record*.

PD: There's a photo of you in the *NIH Record*, I believe, with these three people getting ready to give testimony, isn't that true?

RC: Yes. And this is Brian Murphy, now, who's almost bald. Very few people have recognized him in this picture. He'd been at the lab for about two years at that time.

PD: Okay. And who's the woman?

RC: She is Sandy Nusinoff. She came from Brown University. I hired her to be Brian's technician. I saw her transcript subsequently. She had a 4.0 grade average. Many people say they're 4.0, she really was 4.0. After she had worked with Brian for several months both of us said, "Sandy, you're so damn smart that you should have a Ph.D." So she went to night school at George Washington University, completed her course work, and ended up at the top of her class. The research she had done with Brian was to be her thesis.

PD: Dissertation topic?

RC: The subject of her dissertation was to be the research she had performed under Brian's supervision. Also, all she had to do was write it up. And one day, she came into see me and she said, "Oh, I have to leave. I've just been accepted to medical school." And she occupied an office with three post-doc MDs and she decided she would rather have an MD degree than a Ph.D. She had a Ph.D. locked up but was willing to start over. She was extraordinarily self-confident.

PD: And she walked away from it.

RC: She went back to Brown, which was a two-year school at that time and completed her studies—i.e., the last two years—at Harvard Medical School. Then she trained in pediatrics at Duke Medical School. She was married during that time. Her married name is Sandra Nusinhoff Lerman. And she had three other sisters just like her, all 4.0'ers.

PD: That's great.

RC: After completing her clinical training she was appointed to the faculty at Duke. GlaxoWellcome hired her and she was one of the people who did the most, made the most important contributions to getting AZT into the AIDS pipeline.

PD: Interesting.

RC: Oh, she's . . . Sandy was just incredible.

PD: Can you tell me about some of these other photos?

RC: These are pictures of me at different times. I was a Public Health Service Officer for 31 years. I wore a uniform twice—this time and when I had my retirement physical at the Naval Medical Center in Bethesda.

PD: So this picture is from?

RC: Oh, this would be back about '70 or so, and this is, this would be '88. My military bearing is obvious. I gave the Dyer Lecture here, which is one of the big NIH lectures. This is John Seal, who was our intramural director, a wonderful man. And DeWitt Stetten, who was the deputy director of NIH in charge of Intramural Programs. There it is, the Dyer Lecture. Dyer was a highly regarded microbiologist, who later held several high administrative positions at NIH.

PD: Really?

RC: Yes. And there's Stetten.

PD: That's a great picture.

RC: And I think it's, . . .

PD: Those are tremendous.

RC: There's the date, '78.

PD: So you're donating these to the historical office?

RC: Well, I've made copies of everything.

PD: Okay, that's great.

RC: And these are my military pictures here. These are just at different times. You can order these, probably, by just looking at them. This is a favored picture that was taken outside of Building 1. My wife likes it because I'm so relaxed.

PD: That's a good picture.

RC: Yes. And then here's an alumni meeting of scientists who had worked here and come back in many instances from quite a distance. Meera Gharpure traveled from India, and Helmut Brunner, worked on mycoplasma, returned from Germany. And there's my wife and myself. And you can see she's not large so that it's astonishing that she was able to fend off the purse thief in Leningrad. Probably, all her dance training and practice paid off.

PD: She held her own.

RC: Oh, yes. And here's a . . . this is Bob Purcell, who's head of the Hepatitis Program. He discovered, let's see, four of the hepatitis viruses, A, C, D, and E. He is head of the Hepatitis section. Needless to say he is a world-class, world-renowned international

figure. With Bob is Vanessa Hirsch who was in our lab for awhile. And here's Bob Purcell when he first came to the LID. [Laughter].

PD: Oh, my!

RC: I tell you, Sandra couldn't identify him.

PD: When was that picture taken?

RC: Oh, this would be probably about '67, '68.

PD: Okay.

RC: I've got a wonderful picture here I wanted to show you. This is Bob Huebner and Albert Sabin on the porch of Bob's farmhouse.

PD: On his farm in Frederick, outside Frederick.

RC: Yes, and they're very relaxed and pensive.

PD: That's just tremendous.

RC: And Albert's resting.

PD: Yes.

RC: One of the big events was Albert's 80th birthday party. It was held at the Lister Hill Auditorium and the dinner was at the Cosmos Club.

PD: That's a great picture. Was it a festive?

RC: Eightieth Birthday. Oh, yes, colleagues such as Frank Fenner came from Australia and Jorgen Siim from Denmark and Fred Robbins and Tom Weller were also there. And John Enders' widow was there. Thus, the three polio tissue culture Nobel Prize awardees were in attendance or represented. This was a happy occasion for Albert because John Enders, Fred Robbins, and Tom Weller were among his strongest supporters.

PD: Oh!

RC: And this is my after-dinner speech. I was very excited. And here's Albert [Sabin].

PD: That's right outside the Cosmos Club.

RC: Yes. Just coming. And there, I'm right behind. There's Frank Fenner's head.

PD: That's your son, isn't it?

RC: That's Stephen, yes. How'd you know?

PD: There's a picture of you and your family right over there and I read your biography.

RC: Yes, that's Debbie, Albert's older daughter and there's Frank Fenner and myself.

PD: And isn't your son is also a scientist?

RC: He's a tenured scientist in the National Cancer Institute at NIH.

PD: Okay.

RC: And after dinner, we had a big present to give Albert. This is Tony Fauci, the Director of NIAID.

PD: Okay.

RC: Albert and Jim Hill, who was the deputy director of NIAID at the time. And I'm reading the inscription at the base of a Steubenglass reconstruction of polio virus.

PD: Oh.

RC: The man who determined the three-dimensional structure of polio virus at the atomic level, Jim Hogel, was at the birthday party as well. I received over 300 pictures of Albert's friends, but these were the ones that I thought might be of most interest. Tony Fauci presented his congratulations and there I am still talking. [Laughter] Then I come off the podium and am greeted by Heloisa [Albert Sabin's wife], who is an absolute angel. This picture includes Saul Krugman, who was Albert's first cousin. Saul's mother and Albert's mother were sisters. Saul, a professor at New York University, was one of the leading hepatitis virus researchers.

PD: Where is it?

RC: This picture was taken just before the speeches. This picture was taken as I came off the podium.

PD: Okay.

RC: This is 1993, when I received the Bristol-Myers Award for research in infectious diseases, and there's Stephen and Lizette, and the four munchkins. Sabrina is now over five feet, 99 percentile for her age group. Nick is now six foot two and growing. He's going to college next year; Alexander and Chris and myself. And you can see my wife's face is not scarred from her encounter in Leningrad.

PD: She recovered nicely. That's a great picture.

RC: Yes.

PD: Well, we should probably wrap up this session and I really appreciate your taking time.

RC: Well, you have to pay overtime on this, 20 minutes over, you know.

[End of Interview #1]