Dr. Morris Schaeffer, MD. PhD. Former Chief of the Virus and Rickettsia Section, CDC. Interview Date: 1984.

Interviewed by Jim Paine.

Link to Interview: http://globalhealthchronicles.org/MCDCB/morrisschaeffer

Summary of Interview:

Dr. Schaeffer described his years with the Virus and Rickettsia section as the happiest in his life, despite working as the New York City health officer and as the present director of Drugs and Biologic Services for the FDA. He loved it for the excellent people he recruited, ease of procuring equipment and money, proximity to an officer's club, and camaraderie of a journal club. Dr. Schaeffer discussed his birth in Russia and childhood in NY, where he studied with William H. Park and earned his MD and PhD. In 1942, he was hired to work at the virus lab in Montgomery, AL, where he studied rabies and avian viruses, including encephalitis. He was proud to help states and be associated with (possibly) the first center to offer a diagnostic virology course. He also helped with the poliovirus, and believed Sabin may have used their LSC strain.

Notable Quotation from Interview:

On the Sabin polio strain*: "We gave that strain to several other investigators including Albert Sabin. By the way, the strain that we gave was called 'LSC,' ('L' for Li, 'S' for Schaeffer), and we had four lines of the virus, four different variants: LSA, LSB, LSC, LSD. The LSC was the avirulent strain that we believe is now the so-called Sabin strain. Although, I regret to say that Sabin does not admit that this is a strain that he put into his vaccine." – Dr. Morris Schaeffer*

On the use of monkey kidneys: "*That [monkey kidney tissue] was, at that time, a newly developing procedure for having large amounts of cells. The kidney of a monkey would be removed, surgically; the monkey could be kept alive for the use of a second kidney much later. That kidney would be minced and, with the use of enzymes, the cells would be shaken and separated into layers. Monolayers of those monkey kidney cells served as the culture for many viruses. And of course, that helped a great deal, not only in diagnosis but also in research in virology. It was a new technique that, and again, our laboratory was right on top of this new technology and using it very well.*

On virology training at CDC: "*We were, I believe, the first in the United States or in the world to provide a diagnostic virology training course…. In a matter of ten years, we extended the ability of states from three in the United States to something like 23 or 24, who are now capable of doing some basic diagnostic virology. Previously they had to depend upon CDC to do for them."*

Key Terms in Interview:

Virus; *Rickettsia*; polio; rabies; encephalitis; psittacosis; Montgomery, Alabama; New York; diagnostic virology; Coxsackie virus; Newcastle Disease Virus; equine; ornithosis; fluorescent antibodies test; Habel test; LSC strain; HeLa cell, negri bodies; journal club; Carworth farms; 'Cutter Accident'; Swiss Webster mice; NIH [National Institutes of Health]; Walter Reed Viral and Rickettsia Laboratory; Rockefeller Institute & Foundation

Key People Mentioned in Interview and memoirs of 1990:

Chamberlain, Dr. Roy
De Kruif, Dr. Paul
Francis, Dr. Thomas Jr.
Goldwasser, Robert
Habel, Dr. Karl
Howard, Beatrice
Hughes, Thomas B
Jensen, Dr. Keith
Kissling, Dr. Robert
Lacks, Henrietta
Li, Dr. C. P.
Miller, Dr. Seward E.
O'Connor, Basil
Park, Dr. William H.
Robinson, Dr. Roslyn Q.
Roosevelt, Franklin D.
Rubin, Harry
Sabin, Dr. Albert B.
Salk, Dr. Jonas E.
Siegal, Michael
Shepard, Dr. Charles C.
Sudia, Dr. Daniel
Tierkel, Dr. Ernest S.
Webster, Dr. Leslie

Dr. Morris Schaeffer

Jim Paine (interviewer): Welcome, Dr. Schaeffer.

Dr. M. Schaeffer: Thank you Jim, it's always a pleasure to be back here.

Jim Paine: I think there's been a lot of water over the proverbial dam since we first worked together in 1952 and I don't really know whether I should introduce you as the former Director of the Virus and Rickettsia Section in Montgomery, Alabama or the former New York City Health Officer or the present Director of Drugs and Biologic Services for FDA.

Dr. M. Schaeffer: Well, of all the titles, the Chief of the CDC Virus Laboratory appeals to me most. That was the best period of my life. It was the most exciting, most productive and the happiest in my career.

Jim Paine: With a career like yours, that's quite a compliment. We thank you for CDC. You got the BA degree and a MA degree from the University of Alabama, but was your place of birth in Alabama?

Dr. M. Schaeffer: No it wasn't. In fact, I think most people would be surprised to know that I was born in Russia and came to the U.S. in 1913 at age five. However, I was raised on the sidewalks of New York, where I consider myself basically a New Yorker: an old New Yorker.

Jim Paine: And subsequently, you got a PhD degree and a MD degree from…

Dr. M. Schaeffer: New York University. I studied with the famous William H. Park (who was the American Pasteur of his time) and then subsequently went to medical school because I felt that that background was necessary for the work in public health that I was interested in doing.

Jim Paine: And then, after your MD degree and prior to coming to CDC, what was your work in, sir?

Dr. M. Schaeffer: Yes, well, I followed up the MD degree with an internship with Boston City Hospital. Then went to Western Reserve University, now called Case Western Reserve, where I actually trained in pediatrics and taught microbiology and ran a contagious disease unit at the city hospital of Cleveland. So, that was an interesting prelude to coming into the Communicable Disease Center. I was getting ready for the work here.

Jim Paine: You came to the Virus and Rickettsia Lab in 1949?

Dr. M. Schaeffer: Yes, it was May of 1949.

Jim Paine: How did you happen to accept that position?

Dr. M. Schaeffer: Like many things that happened to us, it was just an accident. I happened to be attending a medical meeting in which one of the CDC people had given a paper on the developments of the CDC program. I talked to the gentleman afterwards and he said, "I think Seward Miller, the Director of Laboratory, might be interested in you with your background in virology. Why don't you drop him a note?"

I did that and then received an invitation from Seward Miller to come to Atlanta to give a lecture where I think they had everybody of importance in the shop, giving me the once over to see if I would do or not. Subsequently, I received an invitation to join and take charge of the virus laboratory in Montgomery, which I did very enthusiastically.

Jim Paine: Did you establish that laboratory sir or was it in existence when you moved in?

Dr. M. Schaeffer: Well, the laboratory was already going. I think it had been operating for a couple of years. There was a very nice lady by the name of Beatrice Howard, who was in charge and Robert Kissling that was working with her there, a few others, and there was Ernie Tierkel and Harry Rubin doing some rabies work. The laboratory was primarily a station for processing specimens that came from the outside, other than a little bit of research that was carried on by Ernest Tierkel and Harry Rubin, mainly in dogs with rabies. It was obvious that that laboratory needed some considerable development. I knew that we would be able to do that because Seward Miller was very keen on getting that done and was an enthusiastic supporter of getting us everything we needed and wanted.

The first thing I did was to find a good sanitary engineer and it happened to be Earl Arnold, who later became, as it were, the deputy. I think we called him the Executive Officer but he was involved in more than just administrative work. With his help, we constructed a number of barrack type buildings and put up Quonset huts, animal facilities were improved, we brought in some excellent people; we were very lucky in getting Roy Chamberlain, a first rate entomologist. Robert Kissling was sent to New York to get some training in virology and he later did some very outstanding work in both pathology and virology. We had a succession of people who helped us in developing the diagnostic laboratory.

I think the most important thing we did in the beginning (in addition to establishing better facilities and bringing in the people needed to embellish the potential) was to start a training program. We were, I believe, the first in the United States or in the world to provide a diagnostic virology training course. It was set up initially for state laboratory personnel, but we had many students from universities and from many foreign countries. No sooner was the announcement made of the course [than] it was filled. We gave them twice a year for a three-week period. In a matter of ten years, we extended the ability of states from three in the United States to something like 23 or 24, who are now capable of doing some basic diagnostic virology. Previously they had to depend upon CDC to do for them.

Jim Paine: You mentioned recruitment, was it difficult to recruit people in 1949? Was that a problem? The technical people, both the professional staff and the technical people?

Dr. M. Schaeffer: Well, I thought it would be almost impossible, but I found myself wrong. I recruited the best secretary I've ever had to this day from Montgomery, Alabama: a lady by the name of Robbie Kelso. She was so superb. Our technicians were as good as you could find anywhere. Yes, we trained them, but they were readily trainable and very dedicated. We were able (as our reputation grew) to get some very excellent people to join us. So, it wasn't very long before our laboratory developed a national reputation. As I told my crew at the beginning, I said, "You know, we will know that we've succeeded when Ed Lennette, the Director of the Virus and Rickettsia Laboratory for the State of California," (which was high reviewed), "comes here or sends his people to see what we're doing and when Joe Smidel from the Walter Reed Viral and Rickettsia Laboratory" (which was the greatest in the country at that time), "comes looking to see what we're up to. We'll know we've arrived." And by golly, it didn't take very long before they came to see what we were doing.

Jim Paine: Very good. You've talked about personnel and you've talked about staff, I suppose the next logical question is procurement of equipment in these years following World War II. Was that a problem?

Dr. M. Schaeffer: Well, no, it wasn't because we were in that period in public health when the Congress was giving good support to NIH and other public health service units. I think we were in the (maybe the) golden age of the time. And so money was available and we were able to get the equipment and the supplies and all and the financial support we needed.

Jim Paine: I believe it was in 1949 that Western Equine Encephalitis and Eastern Equine Encephalitis antibodies were found in the virus of birds. I guess that became our link.

Dr. M. Schaeffer: Yes, our laboratory played an important role in working out the interrelationship between birds; mosquitoes; mammals and human in the cycle; the virus cycle for eastern, western, [and] St. Louis encephalitis; and subsequently some work was done with Venezuelan encephalitis. We had Roy Chamberlain and Dan Sudia as the entomology key people. We were also fortunate enough in getting a young veterinarian by the name of Donald Stan, who was a superb field man. His father had taught him to hunt and fish and he knew how to trap birds. He loved the field and helped develop. He and Kissling and, of course, Arnold played important roles. As I said earlier, he was more than an administrator; he helped develop an excellent program in which we were able to develop important data with regard to the birds, the mammals, and the species of mosquitoes that were probably the key elements—vectors and reservoirs and so on. Perhaps later, Arnold will tell you a little more about that. I understand you will be talking to him. But this was one of the important contributions of the Montgomery Virus laboratory to the so-called arthropod-borne virus knowledge.

Jim Paine: Where was this early…you mentioned bleeding and trapping of birds and the mosquito related. Where was this work done, in what part of the country?

Dr. M. Schaeffer: Well, a lot of it was done out in the field. Initially some in Alabama, but most of it was done in the Louisiana Bayou area, which was sort of a hotbed for that sort of thing. They had a lot of the species involved. Don Stan worked out a method of using Japanese mist nets, very fine nets that could hardly be seen to trap birds, bleed them and then release them again. In the previous work it was necessary to shoot the birds and then you could just get that one bleeding. But Don was able to trap, for instance, one bird as many as eight times during a summer and autumn season and see the development of antibodies against one of the encephalitis viruses, then indicate that these birds were bitten by mosquitoes harboring that virus, then they [the mosquitoes] would develop a virus titer high enough for other mosquitoes to pick up the virus in them and transmit it. We were able to get different levels of both virus development and the viremia stage so that we could tell whether it would be important or not in the transmission cycle. And, of course, the antibodies would tell us about when the virus entered (INAUDIBLE 13:08) antibody develop and indicated even though the virus is no longer there. That animal had been in contact with the virus for some time. If we could watch the rise and drop in titer, we would get some idea of the timetable in which this took place.

Jim Paine: Were you also working on Newcastle [virus] at that time?

Dr. M. Schaeffer: Yes, that was, at that time, a virus of some interest because from an economic point of view it infected chickens, but it was also primarily…and it was an easy virus so primarily was a research tool to a large extent.

Jim Paine: And Coxsackie? Were you all working on the Coxsackie virus too?

Dr. M. Schaeffer: Yes, well, you see, because Coxsackie was polio-like virus and occasionally would produce a disease like polio, it was important. This was a period in which these viruses were coming along as they were discovered. And so it was necessary to know the ecological relationship of these viruses and the role they played in human disease.

Jim Paine: And I also believe that the lab got involved in some horse transmissions of equines about that time.

Dr. M. Schaeffer: Yes, it was interesting to find out the susceptibility of horses because initially the encephalitis was seen as a disease of horses and then there was the spillover into man. It was thought that perhaps humans got it from the horses. It later turned out that the horses were only an index; that they never developed a sufficient viremia, a sufficient level of virus in their blood to be carried by mosquitos to humans. But in the vicinity would be birds with higher titers that might possibly be the source of say, a mosquito infection and then the mosquito to man. We did find, however, that with Venezuelan virus, horses could pick it up by contact. If you infected a horse with Venezuelan virus in the saliva and in the respiratory tract, contact from one horse to another would transmit this disease.

Jim Paine: This is by mosquito?

Dr. M. Schaeffer: While mosquitos could do that, it was also possible for contact…

Jim Paine: For contact.

Dr. M. Schaeffer: In other words, it was a contagious disease as well as a mosquito borne disease.

Jim Paine: I see. And I believe in 1952, things sort of warmed up for polio. That was one of the bad years for polio, I believe, in this country.

Dr. M. Schaeffer: Yes, well, polio of course was a problem, had been a problem up until the time Salk developed the vaccine. Our laboratory was always heavily engaged in working with poliovirus. We were fortunate enough to get a Chinese physician scientist, Dr. C.P. Li, to come to Montgomery and to work with us. He had been successful in transmitting Type III virus to mice. Up to this point, only one virus could be transmitted to mice, otherwise you'd have to use monkeys or chimpanzees. The Type II virus could readily go in mice, but Types I and III did not. Li was able by intraspinal inoculation to get Type III in mice and I brought him to Montgomery to try and do it with Type I. He and I worked together on this problem, [him] mostly of course. He spent most of the time in the lab with it since I had other administrative duties, but I would pop into the lab from time to time and occasionally talk over experiments and so on. Now, he was able to get Type III going and then from that we were able to get it going by intra-cerebral. We had all three viruses transmitted by intracerebral injections, which were easier to do. So now we had a way of testing for all three viruses in mice instead of monkeys, a big difference in cost, of course, because at that time, a mouse was four or five cents apiece but a monkey was $15 or $20 dollars apiece. Li did a lot of manipulating and was able to get the virus to lose its virulence for mice [by] further manipulation and tissue culture back in mice through one route or another and so on.

We're not sure exactly where or how it happened, but it happened that we now had a Type I virus that was no longer virulent for mice, no longer virulent for monkeys, [and] no longer virulent for chimpanzees, but could grow in tissue culture, so we knew it was there. That virus, when fed by mouth—and he and I and several lab workers were the volunteers, we found it would multiply in the gut and we would excrete a virus for a period of time. We then went back again to chimpanzees and found [that] while it would multiply in the chimpanzee intestinal tract, it did not infect the chimpanzees when inoculated into the nervous tissue. So we felt we had an avirulent strain that looked like it might be a good candidate for an oral vaccine. There had been others who had such strains, but not quite as avirulent. Cox and Koprowski, particularly, already had some human tests, but that virus was still of sufficient neural virulence to be considered possibly dangerous.

And of course with the accident with Salk's vaccine, the so-called 'Cutter Incident', where some large number of children were paralyzed from taking the vaccine, one had to be careful about going on with that type of thing with a neural vaccine in humans. Well, the Cutter incident caused us to have…well, we were asked to stop working. We already had human trials ready…

Jim Paine: Yes, I participated in those if you recall.

Dr. M. Schaeffer: Yes. But we were asked not to do that. I can understand their fear. And so, we gave that strain to several other investigators including Albert Sabin. By the way, the strain that we gave was called LSC, 'L' for Li, 'S' for Schaeffer and we had four lines of the virus, four different variants: LSA, LSB, LSC, [and] LSD. The LSC was the avirulent strain that we believe is now the so-called Sabin strain although I regret to say that Sabin does not admit that this is the strain that he put into his vaccine. He claims to have given it some additional passages and that the strain he's using is different.

Jim Paine: That work was very interesting. I recall the intensity of the operation at that time in 1952 and '53.

Dr. M. Schaeffer: They were exciting years and, regardless of who gets the credit, we are proud of the contribution we made in polio. By the way, we participated also in the vaccine trials. The Salk vaccine trials were countrywide and we took care of the southeast area (a portion of the southeastern area) [in] which we did a great deal of the serologic work: virus isolations and all the things that were necessary and to contribute to the study, which was an excellent study. Of course, Salk deserves a considerable amount of credit for carrying on this large study and carefully [doing the] study, which without question showed that that vaccine would protect. Now we have two good vaccines there: the inactive Salk type (inactivated polio vaccine), which is injected and the Sabin type (the so-called oral polio vaccine), which is given by mouth. Both have their place and both have contributed to the eradication of polio in most of the western countries of the world.

Jim Paine: You mentioned the Cutter vaccine problem with the contaminated caps; I believe that resulted in some 75 cases of polio. But did we…did the laboratory in Montgomery participate in any of the work with that?

Dr. M. Schaeffer: Yeah, well…

Jim Paine: Resulting from those cases?

Dr. M. Schaeffer: We helped with the testing of specimens (and so on) that were necessary. It was not possible to be sure and we did not have the technology to tell which strain of virus was a vaccine strain or a wild-type virus. There was some question as to whether these cases may have been coincidental: that a wild-type virus was infecting the individual who happened to be vaccinated. But I don't think there was any question that there was virus in the vaccine, inactivated it was a…technical error. I think there was a process of filtration prior to inactivation with formalin that was not carried through by the manufacturer. And this was later corrected. There have been no further problems with inactivated vaccines since that time. Although, I must say that with the oral vaccine, the live oral vaccine, there is an occasional individual who comes down with polio particularly…not so much the initial individual, but let's say a parent or relative in the household of a child who has received the oral vaccine who sheds now virus, which may become a little more virile than it was initially. Or it may be an individual with a very low immunocompetency that may not be able to keep the virus from getting into the nervous system. Everyone that takes the oral vaccine has the virus multiplying in their intestinal tract, but it doesn't get into the bloodstream; or if it does, it doesn't attack the nerve cells in the brain or cord. But one in three million or eight million figures vary, or will come down. It's a small price to pay for the broad protection; however, that is still an undesirable affect.

Jim Paine: That is a polio-like disease that you sometimes assume also? Or you sometimes receive from the polio vaccine?

Dr. M. Schaeffer: I uh…

Jim Paine: I say the polio-like diseases that sometimes show up.

Dr. M. Schaeffer: Yeah, some of the cases said to be due to the vaccine are not, but it's hard for judges and juries to understand the technical aspects of that. In many instances, awards for damages were given where it was clearly not due to a poliovirus but some other cause for the paralytic disease that ensued.

Jim Paine: I don't think any discussion of Montgomery would be complete without discussion about the rabies work and the dogs that we had at Montgomery. You mentioned that a little bit earlier but uh…

Dr. M. Schaeffer: Yes. Yes, the studies done there were…well, two veterinarians who were actually not members of our staff, but collaborated with members of our staff. That was Ernest Tierkel and Harry Rubin, who belonged to the…worked with Dr. Steele, who was the Chief Veterinarian…

Jim Paine: Dr. James Steele.

Dr. M. Schaeffer: James Steel, yes.

Jim Paine: Right.

Dr. M. Schaeffer: Now we provided the space there. We had lots of room. We were able to keep dogs for long periods of time. These studies required follow-up on dogs vaccinated for several years. There were four groups. There were, at that time available, four types of rabies vaccine and it was to determine which of these were the best. So Tierkel (initially with Rubin) subsequently continued after Harry left and went to work with a famous virologist by the name of Stanley in California. Those facilities there made it possible to carry out this study in large numbers of dogs.

Jim Paine: And Dr. Karl Habel, was he involved in those?

Dr. M. Schaeffer: Yes. Dr. Habel from NIH was a virologist who had contributed a good deal to polio as well as rabies. As a matter of fact, I got Dr. Li from Habel's laboratory. He had worked with Li on the Type III in mice, Type III polio. Habel was also interested [and] developed a so-called Habel test for rabies, a method of evaluating the potency of a rabies vaccine. And he also was a collaborator with Tierkel in our group, in these dog studies, on the vaccination of dogs.

Jim Paine: I believe we also did a good bit of work, it seems like to me we received hundreds, maybe thousands of psittiacine birds or birds suspected of being…

Dr. M. Schaeffer: Yes.

Jim Paine: …having psittacosis at one time.

Dr. M. Schaeffer: Yes. There were lots of birds. Parakeets seemed to be very popular and they were harbored. A virus known as psittacosis and psittacosis virus was transmitted by people who bought these parakeets or other psittiacine birds. Because of the widespread occurrence of psittacosis in humans in contact with birds that they had purchased, we were asked by the quarantine service or the Public Health Service (since we had the space) to keep these birds and test them. Of course we found them loaded with that virus; eventually they had to be destroyed.

Jim Paine: This also came out of turkeys, I believe, the psittacosis came out of turkeys at some point in time.

Dr. M. Schaeffer: Yes. The same virus when occurring in other birds was called ornithosis virus and it involved other birds and particularly turkeys. Yes, cases of this infection occurred in the people working in the processing of turkeys and at one time we received oh, I don't know, several hundred specimens of these to test, and we found not many, but an occasional bird would be infected. This is a recurring problem from time to time.

Jim Paine: You mentioned early on that one of the things they were doing when you arrived at the laboratory in '49 was diagnostic testing. I presume that continued in the entire field of virology during the period that you were there Dr. Schaeffer.

Dr. M. Schaeffer: We were part of the Bureau of State Services and one of the things that CDC did then (and is still doing) is be available for help. When a state has a problem that it can't handle itself, it can call on CDC. Now one of the ways in which we helped was to provide the diagnostic services. Regardless of the situation, whether it was an animal problem, human problem, and sometimes we'd even go in there and help with surveys of a situation. So this gave us, of course, an excellent knowledge of what was going on around the country. It provided services to the states so they could better handle their health problems. And to us, it also provided a tremendous amount of research material, which enhanced our own knowledge and ability to deal with these problems.

Jim Paine: Some of these were completely unknown, weren't they, when you received them?

Dr. M. Schaeffer: Yes. Very often we'd get specimens completely unknown. We'd try to get as much background information as possible so that we could go more directly to look for what may be the cause. Otherwise, you have to go through so many different procedures and sometimes it is a fruitless approach. But we generally ended up by finding out what the problem was and providing reports. Unfortunately it took a lot of time, sometimes weeks or months before we could give the answers.

Jim Paine: I'd like to mention just some words and let me get your comments or your reaction to some words that occurred as we lived in Montgomery. One of them was 'white mice', maybe the CFW strain.

Dr. M. Schaeffer: Oh yes. When we began our work in those days, people just used mice or chickens or a dog without paying any attention to their genetic background. There would be so much variation from results from one place or another or the results from the same place at different times. Now, there was a man at Rockefeller Institute by the name of Webster who decided to breed a genetically known strain and they call them the Swiss Webster mice. An acquaintance of mine who developed an animal production program, commercially (Carworth and called it Carworth Farms) then went into this genetically known breeding and had a mouse called CF- Coward Farm, W- Webster. We received (initially) the breeders from Webster's laboratory at Rockefeller. We then felt that we should buy these mice, either from them or to get breeders from them and breed our own. We did a little bit of both. It made a big difference in the results; they were more uniform, more reproducible and more dependable. Of course today, nobody just works with any old mouse or any rat or any animal. There is, today, more known about the genetics of the animals that are used or the cells that are used in tissue culture. In those days, it was rather haphazard. So, we were right on the ball, we were right on top of things and went on with the new, the newer, better ideas.

Jim Paine: I recall that we bred, at one time, as many as 5,000 white mice a week.

Dr. M. Schaeffer: Yes. Yes, we had a big need for them. We also bred monkeys, by the way. Inadvertently, we had needed a lot of monkeys for polio and we found that they would breed pretty well in captivity so we always had a good number of baby monkeys available. Today, of course, the wild monkeys are now disappearing and most of the monkey stock for research depends upon breeding.

Jim Paine: While we're talking about monkeys: the monkey tissue or monkey kidney cells?

Dr. M. Schaeffer: That was, at that time, a newly developing procedure for having large amounts of cells. The kidney of a monkey would be removed, surgically; the monkey could be kept alive for the use of a second kidney much later. That kidney would be minced and, with the use of enzymes, the cells would be shaken and separated into layers. Monolayers of those monkey kidney cells served as the culture for many viruses. And of course, that helped a great deal, not only in diagnosis but also in research in virology. It was a new technique that, and again, our laboratory was right on top of this new technology and using it very well.

Jim Paine: Another cell material was the HeLa cell.

Yes, well the HeLa cell was a cell, sort of what they call an immortalized cell, a cell that continues multiple dividing and dividing, and it's really a cancer cell. The name HeLa came from the name, the first two letters of a woman's name, her first name was Helen and the other La for the last name. However, such cells could not be used for making vaccines because of the concern about the possibility of cancer. So we used normal cells in vaccine production, except for diploid cells, which multiply (you can use diploid cell, see) the monolayer cell for monkey kidney. You only have one or two multiplications and then that's finished. You have to go back for a fresh kidney. Diploid cells can be used for maybe 10, 15, 20 reproductions and there are such cells from human sources that are used for vaccine production. But the monkey kidney cells seemed to be quite acceptable for vaccine production.

Jim Paine: We also used a number of chimps at Montgomery.

Dr. M. Schaeffer: Yeah, the chimpanzees were necessary for the final stages of the testing for neurovirulence in the live virus vaccine program, which we had. And so those chimps were there so we could determine the neurovirulence. Actually, we were so convinced that our Type I was avirulent that some of us had our own children vaccinated with our strain of LSC. Later, Sabin, when Sabin's vaccine came along, it was not until 1960, '61 that that was finalized into use. Of course, everyone began to give it to their children. But we had some young pioneers there that got that live virus polio vaccine before that time.

Jim Paine: We also used day old chicks.

Dr. M. Schaeffer: Yes, they were susceptible to some of the encephalitis viruses and were used in testing. Otherwise, chicks were used in the embryonic stage; we used chick embryos of different ages, seven day old or up to 14 day old. The different viruses or Rickettsia would grow in different portions of the embryo and the amniotic sac for influenza virus and the yolk sac for Rickettsial agents. So, we had a lot of fertile eggs incubating there and for certain studies we would let them hatch out the day old chicks, who had a high susceptibility and they were quite useful for studies of that sort.

Jim Paine: I also recall the very early model of the RC Electron microscope that we installed down there in the early '50's.

Dr. M. Schaeffer: Yes, now that brings to mind a very important person who we brought to Montgomery. His name was Charles Shepard and he was a Rickettsiologist. We were so busy with virus work that we didn't have time to think about Rickettsia for a few years. Well, we thought about it but we didn't do much about it until I found that Charlie was available. I brought him. Now, we (I forget now) couldn't justify bringing in someone for that purpose, but there was a need for somebody to study leprosy. And so he came in to study leprosy until we could get the Rickettsia work underway. Well, Rickettsia work got underway but it was secondary to leprosy because that's another contribution that the Montgomery laboratory made. Charles Shepard and his very, very important advances that he made in trying to grow the lepra bacilli in person/HeLa cells (and other cultures later) in the footpads of mice (and of course other studies subsequently) which he did here in Atlanta. But anyways, pioneer work of leprosy began in Montgomery. By the way, there was an Israeli visiting scientist who came for his sabbatical year to work with us and he worked both with Bob Kissling and Charlie Shepard. [He and] Bob Kissling developed the fluorescent antibody test for detecting rabies [with] the Negri bodies, which are really little blobs of virus in a cell. He was able to apply the fluorescent antibody techniques for readily visualizing the rabies virus or the Negri body. Before that, it required special staining, ability with staining techniques and knowing the cell by cryoscopy and the cell structure and so forth. It required a good deal of training, only a few people could do this well and reliably. But [with] the fluorescent antibody, they just made those little things shine and if you did something right, there it was. That technique, by the way, became the standard technique for our rabies diagnosis, both in animal and in human, pleura rabies *(INAUDIBLE 43:26)*. That was Robert Goldwasser and Kissling. Now Robert Goldwasser also worked with Shepard in doing some studies with Rickettsia, he was also using fluorescent antibody and other serological techniques. It was important to differentiate between the different strains of Rickettsia, typhus…murine typhus and epidemic typhus and other Rickettsia strains. And so some modest contributions were made there with the Rickettsial agents.

Jim Paine: I recall a study, seems like to me it was in 1954. It was a polio study using monkeys and curare and using a respirator that we borrowed.

Dr. M. Schaeffer: Oh yes. That was just sort of a little lark. I'll tell you how that happened. I had been down to Mexico. They had a polio meeting and I was invited down to talk about our work. I met a young Mexican pharmacologist who wondered whether, if you paralyzed [or] relaxed the individual or an animal with polio paralysis, they might not have a better chance of recovery. He thought possibly curare would be the thing. Curare is a poison the South American Indians used to use to paralyze their opponents. They shot darts and the curare would relax the muscles completely. Of course, if you give enough of it, it also kills the individuals so it has to be carefully dosed. Well anyway, I invited him to come to Montgomery to try out his experiments and we had to build special respirators (or adapted respirators for monkeys) so that we would have a monkey paralyzed with polio, given curare then followed through [with] and so forth. It didn't work, but we had an interesting Mexican scientist there for a while and I thought it did a lot, something at least for good international relations.

Jim Paine: I remember you were coming back from a meeting (I'm not sure where this meeting was) with a check made out to me for $100,000.00 from the National Foundation of Infantile Paralysis.

Dr. M. Schaeffer: Well, I sounded pretty generous. Yes, it was an interesting situation. As you recall, I mentioned earlier that we helped with the Salk vaccine studies.

Jim Paine: Right.

Dr. M. Schaeffer: Now we contributed quite a bit to that in help and materials and so on. The polio foundation enlisted a dozen or more laboratories around the country to work during that couple of years in which the Salk trials were going on. There was no way ([or] mechanism) for paying us because if they gave the money to the government it would go into the general treasury and we'd lose it. So, we thought the easiest thing to do would be to have a check made out to you and then you could buy equipment, material and supplies to make up for what we contributed. We badly needed some deep freezers. We wanted a new IBM typewriter, which the government wouldn't let us buy. We hired some additional help, which we couldn't get because there were times when budgets got tight. So, we felt (well, perhaps it may have been a little irregular) [it was] with the good intentions and for the good of the service [that we] accepted that money and enhanced the capabilities of the laboratory.

Jim Paine: I think it worked to the benefit of the (INAUDIBLE 47:57)

Dr. M. Schaeffer: Yes, I'm sure. I'm very pleased with… There were times when we got impatient going through bureaucratic procedures and we sometimes, well, broke a little rule here and there, but as long as there was no personal gain, that didn't bother me.

Jim Paine: What was the name of the gentleman that did a lot of the publicity work for the National Foundation?

Dr. M. Schaeffer: Oh yes, he was quite a character. His name was Basil O'Connor, he was a former law partner of President Roosevelt. Roosevelt had polio (as most people know) and his friends used that to start raising money for polio research. It started as the President's Birthday Ball parties, when the President's birthday came around, they would have balls all around the country and people would pay $100.00 or something to attend. That money was then used to provide research funds or treatment money. The original guy in charge of that was Paul de Kruif, a famous microbiologist and author: the author of Microbe Hunters. Later, when Basil O'Connor was brought in by the President, he and Paul de Kruif didn't get along. One of them had to go and, of course, it wasn't Basil. So de Kruif was out and Basil really built that National Foundation for Infantile Paralysis up into a tremendous organization that supplied large amounts of money for research. They did a great deal of work to identify that there were three types of poliovirus. They, of course, sponsored Salk's work and so on and they also put the seed money out for the current Salk Institute. They did a good job there, even though Basil may have been a bit of a dictator, [and] they accomplished their purpose.

Jim Paine: I believe you instituted a tradition in Montgomery known as the Journal Club for the uninitiated, would you explain that?

Dr. M. Schaeffer: Well I think that was one of the highlights. There are only a few people left to remember it. You know, that was over 35 years ago [when] we started all of that and some of our friends are no longer around, unfortunately. My experience with Journal Club was that they sounded good and everybody wanted them, but they would never last. We would go to someone's home one evening a week or every other week and have coffee or tea and talk about current literature. Somehow or another they soon fell apart and they were gone. Well, Earl Arnold and I talked about that and Earl suggested something, which I think was a key to it. Let's invite the gang in for a journal club meeting and provide a dinner. Well before the dinner there would be a drink or two, later we would have wine and we started a wine tasting. I'd bring some wines in, cover them with paper bags and put numbers on them. And of course, most people didn't know very much about wine at that time; we were still pretty young in this. But anyway, that camaraderie, the food, the drink… By the way, it was necessary after a while to limit drinks to two. Nobody could drink more than two drinks because the meeting would get too lively so we limited [it]. We would have mostly social [time], but there was some discussion of journals and (INAUDIBLE 52:12) and of course, our visitors that came through there, if they happen to be there on a Thursday. It was every Thursday night from about six to eight. I think some of the men had an excuse for not coming home one night a week and it worked out just beautifully.

Jim Paine: As I recall, this has made it a point to be there and some of us became pretty good cooks as a result of the Journal Club.

Dr. M. Schaeffer: Well, that's right, but after a while, we got tired of cooking. It so happened [that] one of our animal attendants that worked down at Pullman was a cook. We now chipped in and paid him to do the cooking and washing the dishes.

Jim Paine: That must have been after I left.

Dr. M. Schaeffer: Because that was a problem always (who was going to do the dishes) and that was solved by bringing in professional help. But it was absolutely delightful to have the group and it helped build up a- that spirit, you know, that so called esprit de corps- that was rarely seen at other places. And as I say, that Journal Club had a marvelous reputation around the country and even internationally.

Jim Paine: We haven't talked a great deal about influenza although work went on at the laboratory.

Dr. M. Schaeffer: Yes, with one area of virology we had not quite developed to a point that was necessary and that was the respiratory virus diseases, influenza. There was some new ones coming along and it so happened that I met a young man by the name of Keith Jensen who had been working with Thomas Francis [who] was the Mr. Influenza in the U.S. He was at the School of Public Health in Michigan. I was able to induce Keith Jensen to come join us and we set up a new respiratory virus unit and then I found an Air Force officer at Gunter Field. We didn't mention, I think, that our laboratory in Montgomery was outside of the city limits and adjacent to Gunter Air Force Base, and many of us were commissioned officers so we had an illegal gate between our little compound and the Air Force Base to the Officer's Club. Students for the courses would get to live in bachelor's quarters for $1.00 a night. They would have the club facilities available to them and on that basis we were able to make them come back evenings for evening work as well. Anyway, we got to meet a lot of the military. This was kind of a medical orientation unit there and we met a young lieutenant in the Air Force by the name of Roslyn Q. Robinson, who we got to work with Keith Jensen in respiratory viruses, he did a superb job. And as you know, Robby Robinson later became the Director of the Bureau of Laboratories here in Atlanta after our group moved to Atlanta. I was fated to do other things when the move to Atlanta came. We were preparing for it in 1959 [but] I had my arm twisted to go to New York (back to the City of New York Health Department) as Director of their Public Health Laboratory Program.

Jim Paine: Let me mention a couple of names and get you to react to them, some employees that we may or may not get a chance to interview, Dr. Schaeffer. I believe you've mentioned Bob Kissling and Roy Chamberlain, but how about Mike Siegal?

Oh yes, Mike Siegal was a virologist from Philadelphia. He was working with the Henley's who are famous virologists; they are retired now. Mike was brought in to run the routine diagnostic laboratory. That was one area I always had trouble with because nobody wanted to do routine virology. So Mike was there with us for a couple of years, but he did a very good job of perking it up a bit. I started initially with another former Rockefeller Foundation person, Thomas B. Hughes, but somehow or other, that didn't work out. Mike Siegal stayed two years, then I had Seymour Coulter, that didn't work out and then we finally gave it to Andrew Fordjore (57:26), who is a little bit more…what shall I say? A little bit more low keyed and willing to produce a service without the glamour of the big research.

Jim Paine: Dr. Schaeffer, I think we're about to run a little bit close on time and I do want to say some things before we close this out. I want to say how much I enjoyed working at the Montgomery Station and how I think the work down there was of such importance to CDC and to the field of virology and how we very much appreciate the effort and the time, the 24 hours a day so often that you put in to see that the job was well done. I thank you very much for coming to talk to us and I hope we can continue this at some later date.

Dr. M. Schaeffer: Well Jim, I can tell you how much I enjoyed talking about a period of my life that I think was the greatest. It was the highlight of my career and I'm proud of what we were able to do. And proud, not so much only of what I did, but how well the people that worked with me contributed, how dedicated they were and how enthusiastic. It was just one of these things that happens once in a lifetime. And I was very fortunate to have that experience and I'm so pleased that other people see that as we did, that this was, this was really a great thing.

Jim Paine: We all benefit, thank you, Dr. Schaeffer.

Thank you.


Morris Schaeffer during interview of 1984 by Jim Paine

END

Recollections of the Early Years at the CDC Virus Laboratory

(Written 10/8/90)

by

Morris Schaeffer, Ph.D., M.D.

Director, Virus & Rickettsia Section, CDC (1949-59)

DR. MORRIS SCHAEFFER

“Remembrance - all the joy that is left to us now;
a poor joy, but our own.”
Sean O'Casey

After the end of World War II, it was decided to convert "Malaria Control in War Areas" into the Communicable Disease Center. Highly competent in parasitology and entomology, it now required additional capabilities in the other important disciplines of infectious diseases. By good fortune, Seward Miller, a perceptive PHS clinical pathologist, was selected to direct the Laboratory Branch and soon became its inspiring organizer and effective leader. He was unique in his ability to recruit an outstanding group of scientists who continued to dedicate their talents to CDC objectives long after Miller was transferred to other PHS positions. First among the impressive group of microbiologists which were brought to CDC were Martin Frobisher and his bevy of capable ladies from Johns Hopkins, [with] Phil Edwards and Jim Ewing, Marty Cummings, Libero Ajello, etc.

The early work was conducted in temporary quarters in office buildings in downtown Atlanta and in the barrack housing of a former army hospital in Chamblee. Since these facilities were not considered safe for virology, the temporary virus laboratory was located in the outskirts of Montgomery, Alabama, adjacent to Gunter Field AFB, a site which had been established some years earlier by the State Health Department with the help of the Rockefeller Foundation to study rabies, then a vexing problem in the southeastern states. Harold N. Johnson and Charles N. Leach had conducted several important studies there which later helped to reduce the incidence of the disease. That work done and the laboratory abandoned, the State of Alabama deeded the land and its two small wooden buildings and the animal house facilities to PHS in 1947.

Virology was still young, the technology primitive and but few virologists were available at the time. Miller managed to persuade Beatrice Howitt, a superb technician, to come from California to take charge of the fledgling lab. She had previously worked for many years with the famous K.F. Meyer at the Hooper Foundation and had published significant reports with him, including one describing the isolation and identification of the first arthropod borne virus in the U.S., eastern equine encephalitis, from the brain of a horse in the San Joaquim Valley in 1930. Eight years later, Howitt was the first to demonstrate this virus in the brain of a human case of encephalitis.

Beatrice was an excellent supervisor and kept her group of well-trained technicians busy processing specimens collected in the field by CDC workers or sent in by state health departments for diagnostic assistance. Research activities were limited at the time to some minor studies by Howitt, but more interesting investigations on rabies and encephalitis were begun by a recently appointed veterinarian, Robert Kissling. There was also underway a long term evaluation of the comparative efficacy of several currently available rabies vaccines inoculated into large groups of dogs. This was conducted by Ernest Tierkel and Harry Rubin, veterinarians from the Epidemiology Branch, in collaboration with Kissling and his assistant Martha Edson (the future Mrs. Kissling).

At this juncture, enter M. Schaeffer, assistant professor in the Departments of Pediatrics and Microbiology at Western Reserve University, 14 years post Ph.D., five years post M.D. and currently Physician in Chief, Contagious Diseases Pavilion, City Hospital of Cleveland. It soon became evident how well this laboratory and clinical experience had prepared me for the job at CDC. Indeed, during my first year, prior to the arrival of Alex Langmuir and his Epidemic Intelligence Service, that knowledge was of considerable help on the many calls for epidemiologic assistance I was asked to answer. In one such investigation, the illnesses following a swimming party in a creek in rural Alabama, initially presumed to be of viral or rickettsial etiology, turned out to be caused by L. pomona from infected pigs watering in the creek and led to our report of the first water borne outbreak of leptospirosis described in the U.S.

As with so many significant events in our lives, I came to CDC by chance. A chance look into a concurrent session at a microbiology meeting where someone who was relating current activities at CDC aroused my interest and led to my writing to inquire regarding opportunities for research in virology. A prompt and enthusiastic reply from Seward Miller resulted in my acceptance of an invitation to visit and give a staff lecture. I chose "Infections of the Central Nervous System," having had extensive laboratory acquaintance with several of the involved microbial agents and, during the past five years, having seen hundreds of cases of poliomyelitis, scores of bacterial meningitis and many of Guillain-Barre' and encephalitis, including one rabies.

There was a large turnout for the lecture, with most of the senior staff and some consultants in the audience. Among the latter were two of my acquaintances, Herold Cox, chief virologist at the Lederle Laboratories and William Friedewald, Chair, Microbiology Department at Emory University. The audience listened attentively and questioned searchingly. I suspected that they had most of the answers and judging from some of the flattering remarks offered afterwards, it appeared I had passed the "inspection" with flying colors. Cox and Friedewald congratulated me warmly and urged my favorable consideration of the job offer.

Frank Reider, another former acquaintance, now Miller's first assistant, escorted me through a grand tour of the scattered Atlanta facilities and the next day Wilton Fisher, another assistant, drove me to Montgomery to see the facilities and chat with the staff. At this time the employees numbered about 25, of which 10 were technical, the remainder support personnel. It was obvious that much was needed in terms of additional staff, adequate physical facilities and an organized research program, but the prospects for these looked bright and the situation was inviting. Accordingly, I accepted the challenge and reported for duty in May of 1949, fired with enthusiasm and optimism.

On the list of things needed immediately were the construction of a suitable water and sewerage system, an incinerator of adequate capacity for safe waste disposal, the replacement of the few army surplus quonset huts recently installed by more useful structures for office space storage and animal quarters. We sweltered in the uninsulated huts and mice died in the intolerable summer heat with only an electric fan to provide a modicum of relief. It took a lot of persuasion to convince the bureaucrats of those days that it was cheaper in the long run to provide proper housing and air conditioning for mice which otherwise died in large numbers. It took even longer for them to realize, something now taken for granted, that humans also functioned better at ambient temperatures below 98.6o F.

We needed a good sanitary engineer to help us with these matters and happily one of the best, Earl Arnold, agreed to accept reappointment in the service and join us. He not only performed magnificently in this aspect of our endeavors but also played an important role in the organization and direction of the scientific activities, serving as an able executive officer and deputy director for the ensuing 8 years.

Other excellent additions to the staff followed rapidly. Roy Chamberlain, an imaginative and productive entomologist and his able assistant Dan Sudia were soon at work in their insectary, breeding mosquitoes and determining their role in their transmission of several of the country's prevailing arbor viruses. Donald Stamm, a veterinarian, came as Kissling's assistant and was to become a prominent member of the arbor virus team. He was a wonder in the field, with an inherent love and knowledge of nature. The team of Chamberlain, Sudia, Kissling, Stamm, and Arnold published accounts of their pioneering field and laboratory studies, depicting the role of birds, mosquitoes, mammals and humans in the arbor virus cycle. This attracted national and international attention and the CDC virus laboratory began its ascendancy.

Tom Hughes had worked for many years on yellow fever in Brazil with the Rockefeller Foundation. When he returned to the U.S., he consented to come to Montgomery as chief of the diagnostic unit. He left the next year for another overseas assignment and was replaced by Michael Sigel, who was replaced after four years by Seymour Kalter, who was replaced by Andrew Fodor, who finally remained in this position until illness forced his retirement. We had poor luck with incumbents in this position because, in contrast to the high research productivity of other units, increasing demand for the processing of large numbers of diagnostic specimens permitted little opportunity for other investigations. Fodor was not as competitive as his predecessors. Instead, he was content in his calm and patient way to develop a steady and reliable diagnostic laboratory, an important service which was not always properly appreciated.

In 1950, at New York University, Mark Adams and Efraim Racker offered one of the first courses available in bacterial and animal virus technology. We sent Kissling and Chamberlain to improve their academic and technical appreciation of this rapidly developing discipline. Harry Rubin followed later and he so impressed his teachers that the next year he was off to work with Nobel laureate Wendell Stanley at the California Institute of Technology, where he contributed outstanding work in basic virology, particularly the concept of "helper" viruses.

That year we inaugurated our own course, the first in diagnostic virology to be given anywhere. It was offered principally to state health personnel but we had many applicants from industry, academia and foreign health organizations. In addition, we accepted students for bench training to work for weeks or months beside the staff and their assistants. We continued to present the course, with constant upgrading as new knowledge developed, on a semiannual basis. Teaching was done by our own staff and prominent scientists were invited as visiting lecturers. Because of its rising popularity, we often could accept only half or less of those wishing to attend. Among the many positive aspects of this training was the significant increase in the number of states that provided some type of virus diagnostic service. This number rose from 5 to nearly 20 in ten years' time.

In addition to the students, we had a flow of visiting scientists from the military, other agencies, local and foreign governments, who came to work with us from a few months to a year or two. One such outstanding visitor was Robert Goldwasser from the Israeli Institute for Biological Research, who, during the year he worked with Kissling, succeeded in applying the fluorescent antibody technique for the detection of Negri bodies in rabies infected tissue. Following their report, published in 1958, this improved approach was to become the standard method for the laboratory diagnosis of rabies as well as a useful research tool.

One of the highlights in the events at our laboratory was the addition of Chen Pien Li, a refugee from Communist China, former faculty member of the Peiking Union Medical College, a youthful companion of Mao Tse Tung and Chou Enlai and onetime general in Chang Kai Shek's Nationalist army, where he was in charge of the preparation of smallpox vaccine. He had been in the U.S. before, in 1927-28, when he worked with America's foremost virologist, Thomas Rivers, at the Rockefeller Institute. An important accomplishment at that time was the successful cultivation of vaccinia virus in minced chick embryo and Tyrode's solution.

Li returned to the U.S. in 1949 and worked until 1951 with Karl Habel at NIH, where he was able to adapt type III poliovirus to mice via the intraspinal route of inoculation. At the end of two years, Habel was unable to keep Li in his lab because of a regulation limiting the tenure of foreign scientists. My administrative assistant, heeding my admonition not to cite five negative regulations but to find one that would permit us to have what was needed, found an exception in the rules for the appointment of foreigners who had special qualifications not otherwise available. We could then lure Li to come to Montgomery to work with us on the adaptation of type I poliovirus to mice. The results obtained went far beyond our expectations. We not only had the ability now to use mice instead of monkeys for work with all three types of poliovirus, but in addition were able to manipulate the virus through passages in cell cultures and in mice and end up with a series of type I variants of considerably lowered virulence from the original highly virulent Mahoney strain. These were named Li-Schaeffer (LS) strains and the principal ones were LSa, LSb, LSc, and LSd. The most important of these was LSc which had lost its neurovirulence for monkeys and mice but grew well in monkey kidney cell culture. This strain also infected monkeys and humans via the gastrointestinal route without producing any clinical signs or symptoms, yet generated a good antibody response. It appeared to be the perfect type I oral vaccine candidate.

We now made ready to conduct a field trial on the safety and efficacy of this strain as a component of an oral vaccine when the "Cutter Incident" ended our hopes. This was the occurrence of a number of cases of paralytic polio between April 22 and May 27, 1955, shortly after the vaccine was licensed, among children who had been inoculated with batches of vaccine produced commercially by the Cutter Laboratories. The Surgeon General temporarily suspended all inoculations with commercially produced vaccine pending testing, and we were ordered to curtail any thoughts of human trials, indefinitely. Albert Sabin obtained our LSc strain and subsequently showed that subpassages of it were safe and effective in large scale tests in Mexico and Russia. An oral vaccine containing LSc progeny was licensed in 1961. It is still the one and only safe and effective Type I strain which has been amply tested for oral immunization. Thus, an accident, at an unfavorable time for us, deprived CDC of general recognition for its most important contribution to the control of poliomyelitis.

In this connection, it is of interest to note that our laboratory had earlier made a substantial contribution to the development of Salk's inactivated poliovirus vaccine. We were one of the collaborating laboratories which participated in the 1954 field trials. These were conducted on about 750,000 school children in 34 states across the country. We covered several thousand vaccinated and control children in Alabama and Florida and processed stool specimens for the presence of virus and blood specimens for antibody rises. The laboratory work was supervised by Allan Bernstein, a very reliable and hardworking young virologist brought from Philadelphia by M. Sigel.

We were doing well with the viruses but little was happening with rickettsia in Montgomery because we lacked a qualified proponent. Then chance presented us with an opportunity to

acquire a distinguished one. Charles Shepard had a personal problem which created a serious rift between him and the administration at the Rocky Mountain Laboratory in Hamilton. This ultimately resulted in his transfer to the CDC Toxicology lab in Savannah. When we learned he was not too happy there, we called him and asked him to remain patient until we could find a way to get him to Montgomery. We knew that our administration was keen on starting a leprosy research activity and eventually convinced them that our tissue culture and animal facilities provided just the place for that type of work. Shepard was brought in to develop the activity with the thought that later,
there would be a gradual shift to rickettsial work. But the leprosy work went too well and the rickettsia were to remain secondary, although a few interesting experiments were occasionally performed with success. Shepard's methodical approaches, starting with the growth of tubercle bacilli in HeLa cells, as a model, and culminating in the propagation of M. Leprae in the footpads of mice, made him an international leader in this small, but important, field of endeavor.

In 1957, the pandemic of influenza brought us additional responsibility and further expansion of staff and structure was necessary to enable us to deal with the respiratory viruses. We became the WHO Influenza Reference Laboratory for the western hemisphere with Keith Jensen who came from the laboratories of Thomas Francis, the country's leading influenza expert, to take charge of our respiratory virus work. We also recruited RQ Robinson, an Air Force lieutenant at adjacent Gunter Field, to serve as Jensen's assistant. Robbie succeeded Jensen, who left us a couple of years later for a pharmaceutical house interested in establishing a vaccine development program. Robbie was perceptive, disciplined and well organized and this eventually led to his becoming CDC's lab chief.

From its early inception it was clear that the CDC virus laboratory was destined to become an important national and international resource. As we were growing, we kept our eye on the country's two older and more prestigious counterpart laboratories - one at Walter Reed Army Institute of Research headed by Joseph Smadel and the other at the California Health Department under Edwin Lennette. I admonished my staff that while we were moving up we would not have really "arrived" until Joe Smadel and Ed Lennette took notice of our accomplishments and sent their people to see what and how we were doing. We were thrilled when that came to pass, relatively early in our history. Two things that impressed Joe Smadel were our new approaches to arbor virus work and the availability of mice to substitute for monkeys in neutralization tests with poliovirus. Lennette sent his assistants to observe our work in rabies and encephalitis. Some of his staff also participated in our courses and he came to lecture to our staff. This was but the beginning of a constant flow of visitors which continues to this day.

There comes a time when one is called to change direction and that call came to me in 1959, when the City of New York eagerly sought my help in their effort to rebuild their once famous public health laboratory. My decision to leave CDC was made with reluctance and some sadness. The past ten years had been the most productive and gratifying of my career; the happiest time of my life. Since I had my start in the New York laboratories, the sentiment as well as the challenge ushered my return. This time it was the experience at CDC which had prepared me well for the difficult task in New York and it was that background which stimulated my initiation of the crusade for improvement in the clinical laboratories under our jurisdiction in New York. This attracted national attention and helped ultimately in the recognition of the need for increased laboratory proficiency and quality assurance (CLIA 1967, 1988). I am proud of that accomplishment, but not half as much as I have been about my association with CDC and its estimable virus laboratory.