Q I would like to begin today by asking you a general question. Could you search your mind and tell me your impression of where virus research was circa 1940. That is the things that had become known by then and the vast problems that were still to be solved.

A Virus research in 1940 was perceived predominantly in two fields: that is medical virology and plant virology. Very great advances had been made in plant virology and in using plant viruses as models for studies. The most important which comes to mind is the crystallization of tubaccomosaic virus and the revolution that has brought about. I will not discuss this aspect of the problem.

The third aspect of the problem, viruses and bacteria had really not gotten out of the most empirical kind of exploratory kind of phase. Although much was done on the so-called five. It really didn't start until after 1940 when physicists entered the field. So we will get.

I will concentrate on medical virology. There is no question that in medical virology, Dr. Rivers was a very important godfather who, although he himself had certain very limited objectives, nevertheless established very strict criterion for working with viruses of importance to medicine. By 1940, the methodology was predominantly one
in which experimental pathology was a most important tool. In other words, viruses were recognized by what they did to animals and tissues. Tissue culture was as such in its infancy. The animals that lent themselves most readily for exploring viruses were mice. Of course there were used a great deal. And polio in monkeys. Remaining virus diseases under investigation were of course poliomyelitis, yellow fever had already reached the point of mass application as a result of a most remarkable series of investigations in the Rockefeller Foundation Laboratories. Influenza was of course, well advanced, although even now it still isn't well advanced, but it had made a great deal of progress so that actually, this was the disease, that was virus diseases that was most on the minds of the people who were thinking of the role that virus diseases could play when millions of men and women would be brought together in the armed forces, so that when I stood back track a little back. I spoke of the tools: the experimental animals, the techniques, the viruses of importance and of course, serology, serologic technique, intralization, complement fixation were important tools. Electronmicroscopy was only just coming into some use. So that it was one might say in the early stages of development in which a great deal had already been learned about many important virus diseases. Medical importance. I left out the encephalytities which had been undergoing studies for
probably six, seven years.

Q How large a community of virologists were there in the United States, circa 1940?

A I obviously have no information. The point was, there were virologists scattered in many academic institutions around the country. There were people who were trained in certain subjects, or self-trained like myself. I was a self-trained virologist. Although I exposed myself to important virologic research in Britain, and in the United States, there were many. There were not as many as there are now. But--

Q In a sense, you all knew each other.

A Well, we didn't all know each other because again, there were divisions. People who worked on influenza, for example, formed a club. People who worked on polio had a definite club. People who worked on encephalitis had a certain club. Yellow Fever was by its very nature limited to the Rockefeller Foundation because there were not many other places in the United States where you could work with it. So that, I would say, the people within the clubs knew each other. But it was not at all unusual that people of various clubs of virologists did not know each other except through the literature and meetings.

Q The reason I raised is because I wondered how large a club there were in neurotropic viruses.

A Well, the club of neurotropic viruses consisted
predominantly of those who had been working on polio and had become involved in such diseases as encephalitis, there is Japanese encephalitis, St. Louis encephalitis, Western equine encephalitis, Eastern equine encephalitis, and oh let's see, of course there is a city Corio meningitis was one of those that had been investigated considerably, so that basically, of course rabies had been in the club for a long time, so that there were a number of people around the country--the number was not too great to that circumstances, but there were quite a number of people involved in studies on encephalitities, polio,--

Q Were you surprised when you were asked to join the Neurotropic Virus Commission?

A I was not surprised because of course I had spent--I came into virology through my work on polio, through my work on "B" virus, through my work on the encephalitities during my five years at the Rockefeller Institute, and because the Commission on Neurotropic Diseases was formed by Dr. Paul whom I had known since 1931, and again a group at the Rockefeller. It was in part, a small club. I was not surprised because I was in the club.

Q You know--

A I am usually in the club. Without any sense of restriction. I would say club is the wrong word to use because club sometimes implies exclusion. This was more a
Dr. Sabin; 6/6/73; 1.5

consortium, rather than a club. There were people who were working in certain fields, and they knew each other intimately.

Q  Now, it is clear that one of the driving forces in Neurotropic viruses disease commission was John Paul. Can you tell me something about Paul as you remember him circa this period?

A  It is very difficult to suddenly say something about a man with whom you have been intimately associated for over thirty years. There was a very close and warm relationship between us. We started out in a very formal way and Dr. Paul himself. He and Trask started to work on polio during a polio epidemic in 1931. And I was asked--actually invited by Dr. William H. Park to start working on this. And we got to know each other then. I don't remember how much older Paul was than I--it was eight or ten years. Something like that you may check. But of course, he was a man with a national reputation, very established, high in academic position. I was just a beginner then. I was just out of medical school. But this relationship that started in 1931 continued on and became -- grew, more or less in the later thirties when I entered the field of polio too. Again, you see, he entered the field of polio. He was a I don't know what word to choose for that. But to say that he had what is often described as aristocratic aloofness. I think that that would apply. He is not a person who readily opened up even to the people that he liked very much. And we
did not really become intimate until we shared an apartment in Cairo in 1943 when as member of the virus commission, we--he and I and Cornelius Phillip lived together and worked together and got to know each other better that--. He was an out-standing person, very, a person of very high standards, but I would say if there was one problem, one factor that stands out as his extraordinary fairness in evaluating a problem. He might have had pre-conceived notions who was without them. But he is always very fair. He was fair to the point where others whose convictions ran a little stronger than his had the impression--probably incorrectly--I would say, that even handedness sometimes seemed. I would emphasize the word seemed. Like one elephant and one mouse having equal weight. But this was the extent of his fairness to the mouse in relationship to the elephant.

Q  Physically, he was a small man--almost slight.

A  Well, yes of course, this is a problem of inheritance, and besides that, he had a certain old illusion--something which prevented him from going into uniform during the war years; but he was certainly extraordinarily active in the fields of highly respected by all. Our own relationship was extraordinarily warm over the years, and as the years went on, it was always warmer and warmer with much--with a tremendous growth in
mutual respect.

Q Now the Commission almost immediately fixes on encephalitis as the one field to concentrate on, initially. Was there--problem--

A There was a reason for it. Well, this happened at a time when there were some outbreaks of Western equine encephalitis. I think in the northwestern United States and Canada. And there was a question of the extent to which it might become a problem in training camps in the United States where it might invade—in some training camps where especially western equine encephalitis might occur—the other reason for a certain concentration on encephalitis was that of course, Japanese encephalitis loomed as a disease for which presumably the whole American population was without any immunity and might in the longer range planning, find itself exposed to possible infection and consequences that were unforeseen. Of course, poliomyelitis was always kept in mind although one didn't quite know what to expect until numbers of cases larger than expected began to occur. But I would say by and large, if I look at it now from the larger perspective, in the minds of the planners, the problems of importance to the armed forces, especially in regards to disease caused by viruses, it was a sort of side issue. Something that you had to do. Somebody had to know something about it. But it really wasn't very important. What everybody
was worried about--what everybody was worrying about. The board that it established was worried chiefly about influenza that it was--influenza with the memories of World War I. And that is why, when the board was first established, it was the board for the investigation of influenza and other diseases, you see. So that, the major emphasis was on influenza. This is what one worried about.

Secondly, one worried about yellow fever because one didn't know just where one would have to operate. And therefore, very soon, came the unexpected outbreak of hepatitis, and those who were vaccinated with the yellow fever vaccine--it was produced by the Rockefeller Foundation just for the use of the armed forces because Killen serum was used to stabilize the virus in the dried state, and it was a time of the potentials of human blood products, or transmission of hepatitis. And a great deal of work was done along those lines. There was also concern, I must say, about virus diseases that might be encountered, that we didn't know much about. That this was a global war. But the emphasis was on influenza. Yellow fever was a matter of applying something that you know to protect yourself against. To get the best possible vaccine against influenza was considered important. And beyond that, it was to have what you might call a cadray of trained people--virologists, entomologists, pathologists, who would be able
to get to a problem quickly and put all the competence—the command of the United States virologists to work on it.

Q One of the things that I find interesting is looking over some of the first things that you did to the Neurotropic Virus Commission. I note that Dr. Paul was very concerned to have autopsies of people who died of certain virus diseases. And it appears to me that you did.

A Well, it wasn't so much Dr. Paul. Dr. Paul was chairman of the commission. But one of the problems in encephalitis that I did not mention was the question of encephalitis in the army—particularly after vaccination. Because everybody was to be vaccinated with small pox. The problem of post-vaccinia encephalitis loomed as a potential of millions. Because ultimately we had ten million people. Now, I was trained as a pathologist, among the other things I trained as. And so, in the division of labor, within the commission, I drew the responsibility for following up all cases of so-called reported encephalitis in the army. I drew that lot also for another reason. Not just because I was trained as a pathologist and did hundreds of autopsies at Bellevue hospital and because I used it as the tool for studying and understanding the behavior of neurotropic virus diseases. But because when—-but because after arrival in Cincinnati, and starting my new work in December 1939, and then 1941, of the jobs that I set myself doing was to try to
try and find an ideologic explanation for every case of encephalitis that was admitted to the hospital. Admitted to the Children's Hospital, admitted to other hospitals. So I was studying encephalitis as it was occurring in man. And I had already come to realize that it was quite a mixed bag. Encephalopathy caused as a secondary thing for certain bacterial and other infections and post infectious encephalitis, and so on. It is for this reason that I thought we ought to have an encephalitis monitoring activity as part of the neurotropic virus commission. And it fell to me to follow up every case of reported post small pox vaccination. Post vaccinial encephalitis, and other cases of encephalitis that might occur anywhere. And the idea was that I would be called upon to go and do the autopsies myself so that I would be able to obtain material that was not contaminated with something else. You know, in an ordinary autopsy, you can take out brain and it be contaminated with feces and other things around. And because I had already developed a program of doing autopsies on human polio in a way that would be needing. So it became my lot to do this. And as chance would have it--I was going to say luck. But it is not the right word. As chance would have it, the first case of encephalitis that occurred after vaccination when the commission was operative, was practically next door to me. Across the river in Ft. Thomas, Kentucky. That was a
soldier who died of encephalitis nine days after the vaccination and twenty four hours after onset of high fever and convulsions. I did the autopsy and worked it out very carefully. Interestingly enough it was the only one. Really. When I say only one. I've got a chapter on encephalitis in the history of preventive medicine in World War Two in which I describe my experiences with reference that can be obtained but I do not recall having to do such autopsies later on during the war, there were many reported outbreaks of encephalitis which were really not encephalitis. Post infectious encephalopathies but that did not become a big problem. until the outbreak on Okinawa where again I went as a pathologist-virologist.

Q I am going to stop.

Q Now, we were talking about the pathological work that you did. I take it then that you--. How did army people react to civilians. Because you were then a civilian coming in and doing these things. Was it a point of conflict?

A There were no problems. Of course, don't forget the pre--actually the mobilization of the armed forces before Pearl Harbor--certainly which followed after it was a civilian army predominantly. We had relatively small professional nucleus. So we were dealing with out own people who went into the forces and the relations were good. No problems.

Q Now, if one looks at problems of encephalitis,
one sees that by 1940, 1941, differentiation had really been made between different types—not all types—but certainly St. Louis, eastern equine, western equine and Japanese—would it be fair to say that the whole question of transmission was still being worked out at that time?

A Well, certain things about transmission were established. I mean, these particular encephalities you are talking about eastern, western equine, St. Louis, Japanese. There was no question they were transmitted by mosquitoes. Perhaps not all factors. Not all mosquitoes that could transmit were known. What was not known was precisely what the source of the virus for the mosquitoes was—where they were getting it. So I would say it wasn't so much actual transmission as regards the question of the mosquito to man. And that man to man was not involved in this. But what was not clear and was the subject of a great deal of study was the natural history of these encephalitities. What other animals were involved in nature? What were the conditions required to reach levels of mosquito infections that constituted a danger. What was the, for example, the ratios between subclinical infection and clinical infection. There was a great deal about the natural histories of these that had to be learned. So that I think those were the problems at that time.

Q Well, that is interesting when we say that had to be learned, and at the same time, it is clear that the research
had a mission. You also had to produce vaccines against such diseases.

A Well, historically, it isn't always necessary to understand completely the natural cycle before you may have an opportunity for preventing it. Although sometimes it is absolutely essential. But the problem of vaccination against such encephalities was one that was studied at the time with the best tools you could have. In other words, you would have to have a source or large amounts of virus, inactivated. And then see how quickly you could produce an immune response. The issue there was one really, chiefly, that if you could produce demonstrable neutralizing antibodies in the blood stream, the assumption that virus that would be brought into the bloodstream by biting mosquito would encounter such neutralizing antibody. If you had neutralizing antibody, you had protection. The measuring rod, so to speak was the neutralizing antibodies. And from the point of view of the military, the issue was rather different. As it still is, I think, from the problem of preventing against an endemic disease which may be a threat for the rest of a person's life, living in the area. Because in the military, you are presented with the problem of trying to prevent something for a limited period of time. So even if you had an ordinary inactivated vaccine, which might provide immunity for six months or a year and then with a booster, carry over for a limited period, that already would be interpreted.
Although I must say that work on possible vaccines was started without previously trying to evaluate precisely the conditions under which you would use them. But one thing was clear. That because of the unpredictability of the occurrence of encephalitis, and the variation of different regions of the United States and the world, it was not the kind of vaccine you would use just in a blanket fashion to vaccinate everybody in the army against it on the chance that he'll be encountered. That was definitely out. I mean, it was not the sort of situation where, let's say yellow fever was used widely. Or small pox vaccine was used widely because you always wanted to be ready to send somebody anywhere in the world. And if he had to go into yellow fever and endemic zone, let him be protected. Okay.

So the problem really was this. As a mission-oriented research activity.

Q Yes, this is fine.

A One visualized the situation in which you had either in the United States a training camps located in endemic areas, and suddenly cases begin to appear perhaps first of all in the surrounding civilian population. There may be a case appearing in the armed forces personnel and you can identify the virus responsible for it. And then you have to make a decision. Vaccinate the people in that area. Try to protect them.

So the problem was not only to develop a vaccine that let us say, might become effective after a certain
number of boosters and four, six, eight weeks. Because by that time the epidemic would be gone. These epidemics are short-lived. And that is why, in planning my own experiments, in animals and on human volunteers, and working with vaccines St. Louis and Japanese encephalitis. And earlier, with western equine encephalitis vaccine. Because, in 1940, there was already such a vaccine available for use in horses but obviously the one that could be used in horses even with the chicken feathers in it, had not been removed--was not a vaccine used in man. The objective was to develop, to study a type of dosage schedule which might within one week, produce protection, that would be effective. Because if you already had to wait two weeks, three weeks, it might be too late. And that is why in all my work I concentrated on a dosage schedule: dose one, three days later, another, and then see what happens within seven days. So that potentially if such things occurred, you would, theoretically, be able within one week to confer protection on those military in the zone of danger. This was the major objective.

Q Would it be fair to say--listening to your descriptions that almost the first thing that you would need--or have a need for--was for presumptive evidence of a neurotropic infection. You would have to know when such a neurotropic infection was occurring. What kind of test could you do for that?
A  Well, I would say rabbit (rapid) tests were not available at the time. But there were several things that were done. When somebody died, as very often happens, that if you can get to do the autopsy before the body is embalmed, that isolation of a virus could be a matter of only of days. See, in mice and then identification by sterilizing method. So that would take ten days to—or maybe a little longer. There was no short cut. The other way was to determine the antibody that developed in patients diagnosed as having encephalitis, if enough die. And then, see which viruses in the stable which you had, they develop antibodies for. If they developed antibodies for western or eastern or St. Louis, and that might take again a couple of weeks because you take the first blood specimen as soon after onset as possible. You've got to wait at least a week or ten days till you get the second blood specimen. And by the time you do the neutralization tests, you will have to wait another week, you see. So there were no rapid methods for determining.

Q  Let me ask—did everyone do neutralization tests in the same way?

A  No, I mean. Look. There always—in every scientific activity particularly in the developing fields, problems of proper techniques. So, since the neutralization tests was an important means for identifying the agent, the virus responsible for an outbreak, one had to develop more standardized
Dr. Sabin; 6/6/75; 1.17

techniques. You know, there are hundreds of details one has to attend to in dealing with a larger problem--

Q Well, this is--

A This is just one of the--that one had to attend to and--

Q The reason I raised this is that neutralization tests were done in different laboratories. And one can imagine neutralization tests done when Colonel Plotts was in charge of the only medical school or when Leslie Webster is in charge at Rockefeller.

A Well, alright. The record shows that we spent some time on standardizing them and we had some of that. And this was done. But I would say that this is a pedestrian problem, that critical people realize it has to be worked out, and but I wouldn't spend too much time on it.

Q I don't want to spend time on it, but you know when you say pedestrian problem. It is only a pedestrian problem in retrospect. It is not a pedestrian problem when it is occurring. One of the most important things is how to get labs to work together and to do standardized testing.

A Well, of course, standardized techniques to be used by laboratories and general hospitals in military, in units this is one of the functions of the medical department of the army, the navy, and so on. And of course, we had to help with that. But from the point of view of doing something in the face of an epidemic, you wouldn't rely on somebody who doesn't do it every day. And that is why you had a fire
department. These commissions that were established on, under the board were in effect, fire departments. And if anything suspicious, you see it would go in by telephone or by telegram to the office of preventive medicine, and the surgeon general and bingo another telephone to the fire department. And somebody would be out there on the next plane. It was a good organization. I think I have come to admire many things in the organization of the army medical service, which has left an indelible impression on me from the point of view of subsequent applications in organization for doing a complex job.

Q Now, Dr. Sabin, the really first vaccines that you develop or against St. Louis encephalitis and Japanese bee encephalitis. Can you search your memory for the development of these vaccines?

A As I said, the reason for that was, again, the division of labor, and the view was that from when I'd have to use the Japanese encephalitis vaccine sometimes in the States. In the Pacific, and St. Louis encephalitis could break out somewhere in training camps in the United States. Again, as I said before, it was a question of developing the optimum inactivation techniques; optimum techniques for quantitative measurement of effectiveness. Not merely taking a standard dose without being able to say here are--here is a quality control. That if we have to ask a pharmaceutical company to make this vaccine, how are we going to control whether this has potency x or 100x or 1000x because everything
depends on potency. These things were not done very well. We were not at a stage where quantitative techniques for evaluation of vaccines had reached the sophisticated state. So I had to work all these things out and find out the best ways to inactivate and the best materials to use and then ultimately to go and study this in human volunteers who were medical students.

Q Now, what material did you use?

A Well, I mean, for St. Louis and Japanese bee encephalitis at that time, the only thing we could use to yield a high quantities of virus required for inactivated vaccine was mouse brain. Young mouse brain.

Q And did you inactivate by--

A It was with formula and it was done at different temperatures, and different durations and then the index for decision had to be a double one. First, innoculating very very large numbers of mice to make sure that it is inactivated. And then continue it for a longer period beyond hypothetically beyond the point where you really inactivated most. There is no such thing as an absolutely inactivated vaccine. Because inactivation curve with formula is such that if you use 500000 or a million, it is possible that there may still be something there that you can't detect. There is still an erroneous conception even now they speak of Salk polio vaccine as a killed virus vaccine. Most of it is killed, if you use ten monkeys to determine whether it is killed, it is killed. If you use ten thousand monkeys, you may find the one that hadn't been killed.
The Cutter accident--this all came after the War, but we already had the theoretical conceptions then and long before polio vaccine came along that inactivation was not an all or none thing, that you were trying to inactivate, obtain sufficient inactivation so if you were beyond the potential of having one in a 100,000 or one in a million. But all this had to be worked out. And you could determine by the rate of inactivation and the amount. But that which inactivated most rapidly and surely you had to measure also by potency. Because if you lost potency that wasn't very good. So that, if you left the formant in, you might continue to inactivate, to lose potency. You had to measure all sorts of parameters and for me, this was an activity which I approached with a great deal of care from the point of view of parameters. It just wasn't cook book chemistry. Of taking so much virus and adding so much formulan, and we need one temperature and cooking it up. No, because I carried out many tests to determine the optimum way to get both inactivation and to prevent destruction of the antigen to determine the potency remains. It is one thing to have a certain potency, as measured in mice obviously then, protective capacity, on day x; and then you are not going to use it until day 100. x plus 100. And what would it be at x plus 100. You had--there were any number of details that had to be attended to, and this is what we did.

Q  This--

A  And we use students ultimately to determine activity
in man because we cannot—there is no way of transferring by weight one man multiplied by weight or something like that.

Q It's--this is your first experience into vaccines which is the reason I raised the question. It is also something very different than your lab is doing at the time. Was there much disruption in your laboratory as a result of this?

A Well, I mean, there is a deviation of concentration obviously this was a period when the main interest of the laboratory, the various people that I had in the laboratory, it was one, to determine the natural history of the disease as it occurred in human poliomyelitis. This was a continuation of my work at the Rockefeller and also experimental studies. So that poliomyelitis was the number one. Number two, we were in a period, again, of an extension of my interest at the Rockefeller Institute of trying to determine the role of micoplasma or pleuropneumonia like organisms we called them then, and human rheumatoid arthritis rheumatic fever and so on. Toxoplasmosis, entered into the field by accident because, as I said before, I was studying all kinds of cases of encephalitis, and again as chance would have it, the first two young children—seven and ten years old, I think, with encephalitis turned out to be caused by toxoplasma and I discovered toxoplasma as a cause of acquired encephalitis, not congenitally transmitted. So there were many things goin on in the laboratory at the time. We also engaged in studies on chemotherapy of experimental arthritis in mice
that simulated human rheumatoid arthritis so closely that it was absolutely incredible. So there were many activities. Therefore, when we had to begin to study also the response to western equine encephalitis, developed Japanese study of vaccination by St. Louis and Japanese encephalitis, obviously it meant a deviation. But this was something that--

Q You just had to do.
A That we did, and this was wonderful. We didn't just hire additional people. We de-emphasized some things and emphasized more. The War effort required answers for these questions ahead of the others.

Q It is very interesting when I look at the vaccination programs that were carried out. You did the experiments on medical students with the St. Louis encephalitis vaccine, and with the Japanese bee. And then Robbie Ward in 1943 tests out the Japanese bee vaccine on workers in Kern County agricultural workers--

A In Japanese? You don't mean Japanese--are you sure it is Japanese. You probably looked it up.

Q Or western equine.
A I think it is western equine, or something like that.
Q And he also did tests--Ward did tests in prisoners at--
A Before we go any further on that, stop this.
Q Dr. Sabin, at this time, in this early period, you were still just a member of neurotropic disease commission,
but you are essentially a civilian. Had you been thinking of ways of joining the army, so to speak, and what problems were there to that?

A I used the expression before the commissions were in a sense comparable to fire departments. Groups of people trained for a specific fields, in specific fields to be called upon by the armed services when the need arose for getting the most competent people to a place where things were occurring. To fulfill this objective further, there was established within the office of a certain general, a special group of officers under General Baine Jones, who was the deputy chief of preventive medicine under General Simmons, who was chief—a group of people who would be commissioned in the army, and who would then have the special task of going out in various places in the field around the world where there were problems of importance to health problems of importance to the armed forces. I think when I try to look back of why it was that I wanted to get into uniform instead of continuing to work as a civilian with the army, was on the one hand that in order to be able to work better in the field, it was important to be in the line of command. It was the military. Individual. And secondly, there was a certain feeling that you were not doing the utmost if you were not in uniform. But besides that, I was losing a number of my people because of the draft, and because of the need for people who were trained as physicians.
One man, for example, even though he had a Ph.D., Dr. Joel Warren, who was working with me. He had to leave because he was called up by the draft board and he was drafted as a private. And fortunately because of his work with us, he was through proper offices, put in the Walter Reed Army Medical School, to work. And then gradually worked himself up to a commission. Similarly, other people, if they qualified for medical service could be very be called upon to go as regular let's say, army surgeons, into general hospital, or a field hospital. Now, in 1940, let's say 1941, I was 35 years old. I was living in the age, 42, 36, etc., 43. I was a couple of years older. And General Baine Jones realized that as, with a certain number of others, that since I had no basic physical disabilities that would prevent me from being taken into the armed forces and being put anywhere, which was the case with some of my other co-workers, like Dr. Ward, for example, he couldn't serve. He had an old hercules lesion--even though it was healed, he couldn't serve. Since General Baines Jones didn't want to lose the basic nucleus, he established this special group of officers, and it was decided best--it would be best--that I become--I think it was one of twelve. I was, only, I think in the second or third commission into that group, and then be available on call to work in a very special way. I would not be assigned to any one unit in the field. I remained on the control of a certain general's office of the army, specifically under General Baines Jones, and I was able to
work wherever, in his judgment, it would be best to achieve the mission assigned. In that way I could be sent to North Africa or the Middle East and then return to Cincinnati and work in my own laboratory. And then wherever the job could be done best to transfer and develop the unit at the Rockefeller Institute at Princeton and then go back to the Pacific--always available--and to be in touch with military units around the world. To do what could best be done.

So that was the reason really why it was decided that taking everything into consideration, that it would be best for me to become a member of this unit in the office of the surgeon general. And that is why in February of 1943 I was commissioned a major and first sent off to, for training at Carlisle barracks.

Q Now, when they say training, what training did they give you?

A This is the training that is given to all candidates for what is called the medical service school. Of course, you get training because first of all, since you have to work with military, you have to understand something of the basics of an army officer, the people with whom you are going to be working; operations in the field; basically such medical service schools had to train battalion surgeons, people would go out with the fighting forces in the field. People who would serve at different regional units or even in general hospitals. So that it was a basic kind of training that all
medical officers would get. Whether they would be inducted as captains or majors or colonels—they all got this basic military training. Which of course included field operations, simulated let's say battalian surgeon activities, map reading, compass getting, fighting in a place, et cetera. And of course, basics of military hygiene problems, and the problems of the medical field officer.

Q It is interesting almost at the end of your training period you are immediately sent off--

A No, not at the end. Several weeks before the end. I forget whether it was the sixth week or eighth week basic course—but long before that, a message came through from the surgeon general's office that I was selected to serve on a special mission to work in Egypt on problems of importance to the area. I remember being called in by the commanding officer at Carlisle Barracks, full of envy because in February 1943, there were very few people assigned for service abroad. This was the period of tooling up, of training, and so on. Everybody wanted to be outside of the continental limits of the United States. You were just wasting your time presumably—at least that was the concept. And full of envy, he says to me "I've just had orders to release you from here and have you go on a special mission. Go and report to the surgeon general's office in Washington for orders et cetera, et cetera." He says, "Are you sure you wouldn't want to try to manipulate things so you'd get my job here and I'd get yours?" Well, at any rate that was the beginning of work
abroad with a small commission, which included Dr. Paul of course. He couldn't be commissioned in uniform. And Cornelius Phillips.

Q He went then as a civilian.

A He went as a civilian because he had a physical, medical problem which prevented him being commissioned. But he was the chairman of this mission. We went off to Egypt and set up a--

Q Now, who else was in that?

A Well, Cornelius Phillips who is a well-established, well known great medical entomologists in the United States. He was commissioned a major. I was started as a major. He was a major. But he was not assigned to the office of a--to this special group of officers. And the three of us started on a really quite an adventurous trip--to go at that time. This was May, early May--

Q Can you reconstruct or write out to the end?

A Well, you could not go directly from the United States to Cairo at that time because the German submarines dominated the area and were shooting down planes. Planes couldn't fly high enough to escape that. So it took us five days to get from Washington to Cairo. And we had to do it in small steps. We went from Washington to Miami. From Miami to San Juan, Puerto Rico. From San Juan, Puerto Rico to certain places in the Caribbean that I forget now except that they were hot and humid and scruffy. And
from there to Berlin in Brazil. And waited in Berlin for a few days until a plane could take us to Ascension Island in the middle of the South Atlantic. Again, we didn't have planes that could make long hops. From Ascension Island to Attron, West Africa. The place where Cornelius Phillip had worked, incidentally, had worked before with the Rockefeller Foundation Yellow Fever Commission. In Attron after a wait over night and plenty of mosquitoes around and biting Cornelius Phillips and ultimately giving him malaria which knocked him out—we took a most interesting trip to Kartuhm. Flying fairly low with the small planes which were available at the time and seeing the wild life in the velts of Africa. And then when we got to Kartuhm, I forget what the distance is 1800 miles or something you just go right up the Nile to Cairo. So we took five days to get to Cairo. The area of course, at that time was predominantly under British control. But there was already an American unit there with an American hospital. The 38th general hospital, in a desert outside of Cairo. That's a long story in itself. But that is where we set up headquarters and had a special unit built not only for laboratory work but also special units for use of human volunteers from the U.S. army for the study of Santron fever which was my science.

Q Now, did you know before you went what work, what areas you would work in or did you--
A Well, the work of the commission was partly three-fold. The main reason for sending it over was the data that had come in to a certain general's office on the potential threat of sand fly fever, a phlebotomous transmitted disease, with which the United States Army had no experience at all. With documentation of the very high probability that during the fall of Tolbruk, the first time Rommel attacked the British forces. That really basically, one of the causes at least, the important causes of its falling was that 70% of the force holding Tolbruk was rendered non-effective by sand fly fever. They were sick. Now, it was realized in the further operations in the Middle East. By that time Rommel had already been cleared out of North Africa. But of course the basic preparations were for the invasions of Italy. Sicily, Italy, Darnel, beyond the belly. And it was well known that there were in those areas phlebolomite. And there was probably sand fly fever. The experience of the Austrian army in the Adriatic had been documented earlier and before World War I. Dr. Dore had done work on human volunteers. Of course there was also the experience of the British army with this disease. And India. In the whole Mediterranean, Near East there was a belt in which phlebotomide were available, or were present prevalent. Presented a potential problem. So there were two things that had to be done. One was to find as a fortranage measure the best possible, the best available let us say, repellant that would protect against bites of these phlebotomist flies. And as a
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means of protecting against this and then to learn something about this disease. Because the knowledge--there was no virus available--although people had transmitted it to human volunteers, there was nothing on the shelf which you could compare to. It had not been grown in anything. There was no experimental animal for it. So it was necessary to begin to learn about it from scratch, by modern virologic procedures.

Q  Good. I have to--

END OF TAPE