Q I wonder if you would tell me something of the initial volunteers that you used on coming back to the United States for your experiments on sand fly fever. Who were they?

A Before arrangements were made at Longview Hospital. It was a mental institution which I will mention later. I was especially interested in the question of testing persistence of immunity. And there were two of my colleagues at the University of Cincinnati Medical School who previously resided for different periods of time in Palestine. One was Dr. Helen Glick, and the other was Dr. Irwin Dunsky. Dr. Helen Glick had given the history of having had a first attack of sand fever, or so diagnosed, five days after she landed in Palestine in the summer of '32. After a number of years, she returned to the United States in 1934, lived in the United States until '37 when she went again went to Palestine, and after a trip to Syria, she again experienced a pheboreal illness of two days duration associated with ecupenia, which again was diagnosed as sand fly fever. So that was now a second attack after an interval of five years. Well, she came back to the United States in '39 and volunteered for an immunity test in 1943, that is six years after her presumable second attack.
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Dr. Dunsky had had his attack diagnosed as sand fly fever in 1934. And then, while in Jerusalem. And then he had a similar attack of somewhat diminished severity during the summer of 1935, while he was living in Jerusalem. He returned to the United States in 1936, where he had resided continuously thereafter, and again in November 1943, he volunteered. Now, in order to have some meaning to testing them for resistance to the virus that I brought back with me from the Middle East, it was obviously necessary to know whether the virus I was going to give them had any activity without any such history. Otherwise, you see it would be an anecdotal experience. It would not be scientific test. So actually these tests were not carried out until I had made arrangements to carry out some studies in a facility near my laboratories in Cincinnati, namely, the Longview State Hospital. This hospital was an institution for psychiatric patients, and at that time, one of the procedures, one of the therapeutic measures, you must remember, it was 1943. Was fever therapy, of different types. And I asked for permission from the authorities of the hospital, the medical director, whether or not they would be willing to have sand fly fever virus which had been tested extensively in human volunteers before, and in U.S. army personnel, in Egypt. Used in patients who obviously had never been out of the country, and had no immunity to sand fly fever. Patients who would receive
other forms of fever therapy. This was done only after the families, or guardians of the patients were fully acquainted with what was being done, and when permission was obtained since the patients themselves were not in the position to give permission, we obtained permission to do such tests on patients at Longview State Hospital, in Cincinnati. So that when it came to do the tests for persistence of immunity on the two physicians, the same material which my notes, records, shows, was a passage three virus in human beings. It was human serum carefully obtained and all kinds of tests done on it beforehand, was inoculated simultaneously into four patients at Longview Hospital, and into comparable dose from the same lot into Dr. Glick and Dr. Dunsky.

Well, all four at Longview Hospital, developed typical sand fly fever, with the white blood cell changes, and--and interestingly enough, Dr. Glick developed an attack at the same time, the same way, despite a history of having had two previous attacks diagnosed as sand fly fever. Dr. Dunsky, on the other hand, had only a very transitory feeling of about four hours of being mildly ill. He had a temperature elevation of 1.5 degrees fahrenheit but it returned to the normal level, there was a slight drop in his white blood cells. And whether it was a modified attack was difficult to tell. But, I mean--this depicted a story. Immediately showed me that the problem was somewhat like this. The history of
these two physicians showed first of all how difficult it obviously is to interpret the significance of presumably repeated attacks in individual patients. When one is dealing with a disease like sand fly fever, for which there is no specific clinical identifying sign, or laboratory test, in other words, they may or may not have had sand fly fever before. Now, thus I was faced with several possible explanations. One was that one or both attacks of the natural disease diagnosed as sand fly fever were really not sand fly fever. And therefore, I couldn't conclude from the test that I did that there is no lasting immunity. The second one was that the immunity to sand fly fever may not persist. But as I said, there was no basis really. It could be that, but it could be something else. The third one was the possibility that there are multiple immunological types of the virus and that what Dr. Glick was exposed to before were not the same types as I isolated in the Middle East before, just because they came from the Middle East doesn't mean they are the same type. And that whether Dr. Dunsky's very mild transitory experience of malaise for four hours at the right time to be sure, after the right incubation period, represented a very transitory mildly infection, there was no way of telling or proving. And it was quite obvious that the only meaningful tests on the resistance to reinfection would have to be done on Americans who had never been out of the country, never had
an opportunity to be exposed to the disease, under carefully controlled conditions—when one virus was given, derived from one source and another one from another source. And it was this sort of work which was subsequently carried over to the Trenton State Prison Hospital, and I think I mentioned it before—that established that the virus which was occurring in American military personnel in Naples was immunologically totally different. That which was encountered in Sicily, and that which was encountered around the Cairo area so that the one was that I isolated in Sicily happened to have been similar to the one in the Middle East. The one isolated in Naples was not, but at any rate, one conclusion was possible, that there are different immunological types of sand fly fever, which is now—in subsequent years been confirmed, although we still if you ask me now whether we can say that there are only the two types that I identified, I don't know. Because there hasn't been enough field study in this subsequently with, incidentally, the mouse-adapted strains that we now have—for both the Sicilian and the Naples strains, to be certain that there aren't other types of sand fly fever.

Q Dr. Sabin, as a general thing, I wonder if you would comment about using psychiatric patients as volunteers. As a general proposition.

A In the first place, volunteers for psychiatric patients is itself a misnomer. Because if they are
sufficiently disoriented. I mean, not all psychiatric patients are disoriented so that they don't know where they are or what is going on. Then they have a certain capability for deciding for themselves yes I will have this treatment, or not, and I know that this is an experiment. But psychiatric patients for whom a decision has to be made by somebody else, I would say that only therapeutic kinds of experiments, in my judgement would be indicated. In which a treatment that has not yet been established but could be of benefit to them, and it has to be tested--the only way you can ultimately find out is by testing it on them. I would say that kind of experiment, that is, the therapeutic substance which might be of benefit to them, as well as to others, that I would say yes.

But to use such patients for substances that for testing substances that couldn't possibly have any impact on their condition, I would say no. Now, it should be recalled that when I did these test for sand fly fever, and subsequently with Dingue fever, in such patients, it was because fever therapy was a mechanism that was used, starting years earlier with malaria, of influencing the course of certain psychiatric conditions, like schizophrenia. However, those experiments were interpreted, rightly or wrongly, whether they were designed properly to permit a conclusion, is beside the issue. There was nothing else in the armamentarion, and therefore one of the reasons why
the authorities and the medical director of Longview State Hospital, Dr. Goldman, I am sure agreed with this. That if it should turn out that a much milder febrile illness than malaria with all the problems that it gave rise to, could perhaps have an effect at least comparable to that which had been previously reported for malaria, then it was a therapeutic experiment. It was a therapeutic test at the time which on the one hand permitted us to learn about the behavior of sand fly fever virus in non-immune human beings, and on the other, permitted the psychiatrists to determine the effect of still another type of febrile illness. So I would say this fell into the category of a therapeutic experiment.

It turned out that much better way of treating schizophrenia came into being at the time, but I think that under similar circumstances, it would still be justified today. In other words, nothing was done in 1943, that under comparable conditions could not be justified today.

Q Now, how about using soldier volunteers?

A Soldier volunteers is comparable in my judgement to using prisoner volunteers, except that it represents still--and student volunteers--it represents still another kind of commitment. In the first place, you can explain the situation to them. Now why there is a student or a soldier or a civilian prisoner who cannot serve in the army during the war volunteer for such tests. Basically, I think
in these particular situations, it was out of that feeling that exists in many of us, of wanting to help others. Because the soldiers received no special inducement. During the period that they served as volunteers for example, in our unit in the desert outside of Cairo, they were precluded from going outside. They lost their liberty. They were not relieved of unpleasant duties because these were not combat--assigned to combat. As a matter of fact, they sacrificed certain liberties and freedom, but the appeal was that here was a disease that was important to U.S. and Allied forces--we didn't know much about it. We needed to learn more that could be helpful. Would you be willing to volunteer. Some did. Others did not. They received no special remuneration. When it was over, they went back to other duties, not necessarily to combat. It was not an escape from combat which made them volunteer. They were told they were going to get sick. They were told that experience previously had shown that people don't die from this, so we could tell them that. We couldn't be absolutely certain about that. They were taking a risk. A couple of them of course risked getting hepatitis as a side issue which made them quite sick, and we'd tell them. Nobody'd die. Nobody--but I am convinced that they did it because it presented them with an opportunity of service in a larger way than their service behind the lines permitted them. You know, many of our soldiers during the war did not serve in combat--
the major portion never sees combat. There are all sorts of support activities. And some of them have guilty feelings. When I go back and I have to tell my son, my child what I did in the war and I tell them that I worked in the kitchen, or I had to work in the service of the supply and doing things--how am I going to explain to him that it was a necessary part of the overall effort. I couldn't tell him stories that I was not there facing death for my country. So here at least, I mean, this some of the thinking that came out of our discussions. When I asked them why did they--they said well it gives me an opportunity to be of service in a larger framework than just working in a kitchen or working in an office or doing other things. I am not a combat soldier, but I want to do more. Alright. Now that's soldiers. How about students.

The students during the war were studying under a special program of training based on the conception that doctors were needed in the service and it was more important that they complete their medical studies than go and serve as a fighter. So they too were responding to an appeal to do something that would be of a help in the total war effort. They also felt that somehow or other, they were getting the best part of the bargain. Because others were away from their families, away from home, abroad, either exposed to dangers that could lead to death, whether they were medical or others. Or working on the terrible conditions. They were
studying medicine. And so when they were asked to volunteer, explaining to them the problem that this presented to the armed forces. They did it. But student volunteers were used only later on in experiments on vaccination, that would not interfere with their work. Because they had to continue doing their work. We really didn't begin to use student volunteers until we were testing the vaccines which by previous work had been shown not to have the capacity of being a danger to the community because we had to show that insects that might be around wouldn't pick up a case of it wasn't a danger--when it was circulating in the blood let's say. So only when we were working with vaccines with these particular viruses did we use students. Now, the prisoner volunteers in the Trenton State Prison.

There is again a feeling of wanting to do something for the country. You may say that when we took them into the special hospital unit, to which they were confined, and lost all freedom of movement, that maybe they escaped for a number of days the more unpleasant conditions obtaining in their cells. But they weren't in their cells all day. They were working in shops, they were outdoors. When they were brought in here, the doors were locked. Because we didn't know what we had. We couldn't take the chance of them either being exposed to New Jersey mosquitoes until we carried out studies of the potential capacity of such mosquitoes to transmit what we were studying and we certainly didn't want them circulating among the other
prisoners. They had to be under observation all the time. We didn't want them to take things we didn't know about. And they knew. We told them that they would get sick. But it was a sickness, however, that would experience at least of the army and the previous armies before, it was not fatal. We could tell them that, but they would get sick. Of course not all volunteered. But large numbers volunteered. I had long lists of applicants.

Q Did anyone volunteer and then after being exposed to being ill say I want out?

A No. It never happened. Of course I had many letters from volunteers who wrote to me in subsequent years but basically when I asked them, because I never accepted them after explaining the whole thing. I spoke to the group and then to individuals. Before they signed, that they understood what I told them. I never took one on without asking him why are you volunteering? We are not giving you anything. Invariably the answer came, look, I have committed a crime against society. I stole an automobile--. Some were in for other crimes, some more minor, some bigger. This--my country is at war; some of my pals are out there. Some have already died. I'm in prison. I can't do anything. Now you are telling me that these diseases which you are studying in the judgement of the people responsible for the medical services in the army are a danger, that they actually
have already caused considerable disease, and it is necessary to learn more to try to prevent them. Well I want to help.

I want you to know that obviously I can sympathize with this feeling. Because I also wanted to go into uniform. But I was working in the lab. I wanted to be of more direct service than to carry on the experiments on poliomyelitis that I was doing at the time, or arthritis. I wanted to get in and be part of the effort. So I sympathized with you, I can sympathize that. But, if you have any notions that this wonderful spirit that you are displaying is going to influence parole, when you come up for parole, I think I must tell you now that I was told it will have no effect on your parole, that you are doing this entirely--Doc, now please believe me. I am doing this because of God's sake.

I had all kinds of interesting manifestations of the many facets of an individual's character who commits crimes against society. Feelings of guilt, feelings of wanting to have an opportunity to do something, somebody. And whenever I had to do a procedure that was not part of the original experiment that I explained, I would have to try to explain why I would do it because I would ask for volunteers. For example, one of the unexplained factors was the rash in dengue when I worked on dengue later. There were two kinds of rash. There was the rash that was like a measles rash that came on within a day or so, two
days after the fever rose. And then there was another one that was a hemorrhagic rash, that came on during the end of the disease, as the temperature was coming down. And, some of this hemorrhage took different forms. It was mostly on the shins of the legs, and so on. But in order to learn what the difference was, really in order to get understanding, and much of the research on infectious diseases is understanding--without reference to whether it has an immediate basis that will help you ultimately to treat or to prevent.

So I had to do biopsies. I had to cut pieces of skin off in order to study it histologically. And I was beating around the bush, and I had a bunch of these volunteers who had these rashes, or were going to get these rashes. And finally, one of the prisoners who was listening to me he caught on what I was driving at. He stopped me. He said, look, doc, you can stop now as far as I am concerned. I think I know what you are driving at. Look, you want a piece of my hide, take it. You see. I am telling this to show how I worked with these prisoner volunteers. They actually they grew up an unusual good relationship between these representatives of the underground of New Jersey, and myself. I was the colonel to them, and for many years subsequently, I could have had the whole New Jersey underground come if I called. My beck and call. Call anything you want, you let us know, you see.
We have friends that will take care of you. You call on us. Anything you near, you hear?

Q Well, later what I will do, there are some very touching letters that I will read into the tape, later.

A Some are part of the form of the National Academy of Sciences that was held on the use—and incidentally, I just got the published proceedings of that.

Q Oh, maybe we can read a letter in—

A Well, I had those letters as part of my article.

Q Could we—

Dr. Sabin, at this point one has to ask you how do you feel about volunteering?

A I know that this is a very complex subject. But I summarized it recently at the National Academy of Sciences Forum after looking at different aspects of the problem. And I developed the following dictum: among the various inane, inalienable rights to which, with which we are endowed, I said that the right to volunteer for medical experiments carried out under properly supervised, and enforced guidelines and conditions. And I want to emphasize these supervised and enforced guidelines and conditions. That is a very important thing. But under these conditions, the right to volunteer in my judgement, is one of the human rights that should not be denied to anyone because of service in the armed forces, imprisonment for a crime against society, unemployment, poverty, boredom, or a
sincere desire to help others. And in the case of the prisoners with whom I worked at the Trenton State Prison in the particular experiments we were just talking about, I think the dominant feature at that time, during the war, and under those conditions, I would say a sincere desire to help others stood out above all other considerations.

Q Dr. Sabin, could we have some documentations, excerpts of letters that you might have received from prisoners subsequently, for the record.

A Well, as I said before, I established that I had a wonderful, gratifying, heart-warming rapport with a large numbers of prisoners who served as volunteers. I have some letters here which I might read. Now, this is one that I received in October 9, 1944. From one of the prisoner volunteers. So this was still during the process of activity, he was still in the midst of it, and he was in a prison. This man wrote to me saying:

Recently I received a letter from the Reverend L. Harold Heinrichs, rector of the Grace Episcopal Church in Nutley and chaplain of the Diocese of Newark. I would like to quote part of the letter. Says this prisoner to me. Quote. Now the second letter comes to me with the news that you are to participate in the dings experiments. This is a fine thing you are doing for your country and for the sake of humanity. I feel very proud of you. End quote.
From his minister. Then this prisoner goes on writing to me. I do hope with all of my heart that your colleagues and yourself find an answer to combat these fevers. I want to see a project a success every bit as much as I want to see the day I will again be called a free man. I wish I could express the feelings I have about this and could explain my thoughts as I would like to. However, I will say this. In volunteering, and being accepted, I sincerely believe I have taken a much-delayed step in the right direction. I am indeed pleased that the only real friend I have in the world, Reverend Heinrichs, is proud of me.

Q That is very touching.
A Oh, here is another letter during the course of the experiments, in 1944. From another volunteer. He says. I noted in our fall issue of the Viewpoint—that is the publication of the prisoners—edited here at the Institution. That two of the other inmates have expressed their desire to continue with your experiments when they reach the outside. Which means outside prison. I am aware, he continues, of the existing prejudice which society has for the ex-convict, thereby placing the one great obstacle in his path. His services are not wanted. The fact that I have lost about everything that was ever worthwhile, I haven't much else to lose. At least I can help to stem the tide of human sorrow and pain by here offering my service—gratis of course. I am willing to participate in any experiment, even of such as of a doubtful nature.
I do not know just which procedure to take after my release. Whether I contact a local hospital, university, or Washington, D.C. It has indeed been a pleasure to work with you here, in the isolation ward, and I sincerely hope that you achieve success in everything you undertake and that all of your experiments bring forth gratifying results.

Here is one on the experiments were still in progress in 1945. And still another. He said. At this time I should like to express my sincere appreciation for your considerate attention during the time I was in the ward. If at any time in the future my body can be of further use to you in the experiments, I want to impress you that I am ready, willing and anxious to comply with your least desire. Perhaps this one and another one, especially interesting because it was written two years after the war, and after the experiments had been completed. But I continued to get correspondence from prisoners. This one is from one of the prisoners who served in serveral experiments. And he writes as follows.

After reading the Newark's Star Ledger for January 27--that is 1947--I thought that I had better drop you a few lines. Perhaps by this time the Army board for investigation of epidemic diseases has gotten touch with you for a statement in regard to the statement that the Nazi doctors made at their trial in Nuremberg last week. They charged--that is the Nazi doctors--that their system of using human beings for guinea pigs was not half as bad
as you used on prisoners at the New Jersey Prison. A doctor, parenthesis, if he can be called a doctor, Werner Liebrand, professor Ahrling in the university said, that he had good proof that the experiments were conducted under force. He also added that no laymen were there, and the prisoners were bribed. Well, after I read that statement, I became hot under the collar. I don't know where they got that story. It must have been another Nazi dream, and like all the rest, it was blowing right up in their face. Because I went through three experiments at Trenton. And at no time was I forced until anything, nor was I given any bribes of any kind. I had made a parole in October of 1943, which was before I ever heard tell of you. And even though I was going out in eighteen months, I took part in your work because I wanted to do something to help. Because a guy feels like a heel sitting by and letting others do his fighting. And the treatment that I received from you and the other doctors and nurses was not to my liking. Because you treated us like heroes instead of just guys that were doing their conscience a lot of good. And what I did, I would do over again. And anything else that I could do, to help those fellows that are now lying in hospitals all over the country. I was ready then and am still willing to give as much as any man that lies over there in some grave. At least I can give as much as he did. After all, they have the right to ask that much from us and while I
broke the laws, I still feel I am an American first. So let those guys on trial in Nuremberg think up a better story than the one they are trying to pull now.

Well, doc, while I'm at it, I had better give you my report on my health. I am feeling fine, and just hope that you are feeling as well as I am.

Q Those are very touching--
A I have these original letters by records, and they are only a few of many that are part of my archives.

Q Dr. Sabin, one thing struck me in --

Q Dr. Sabin, did these volunteers receive anything for their services, any recognition.
A At the end of their service, I had certificate of service. Which I signed, on behalf of the army epidemiological board, as the officer in charge expressing appreciation for what they had done. And that is all. It carried no reward and nothing else with it.

Q Now, one thing interests me particularly about sand fly fever, is that it is almost in one sense, a military disease. At least, the early physicians who were concerned with the disease are invariably army physicians. Now, some of the early work done on sand fly fever was done by the British army, particularly a Dr. Short, who had worked with the British army in India and elsewhere, and at the 1939 Congress of microbiology--international congress--which was held at the Rockefeller Institute, Short gave a paper
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in which he claimed that he had successfully propagated the virus in embryonated eggs, and then gave the figure for the size of the virus at about 160 millimicrons. Now, all of this later proved to be in error. The question of overcoming error is an interesting one. Did you spend much time repeating experiments, trying to propagate the virus in eggs?

A The answer obviously is yes. And Dr. Short, General Short he was at that time was a person of considerable stature, and we naturally were most interested in hoping that the things he reported would turn out to be correct. But it turned--they turned out to be incorrect. I would like to make point that some of the earlier studies on this disease, and you are right in saying that it was invariably investigated by army medical scientists, was done along the Adriatic, by the Austrian army. Dr. Dore, who became one of the leading virologists in the twenties and thirties. Before World War I, he was actually carrying out experiments on it--transmission. He did transmission in human beings, and he did many of the fundamental experiments. But Dr. Short was working between 1934 and 1939 at a time when there were already new techniques available for studying virus. This was twenty years after Dr. Dore. The experiments he reported were very intriguing, although inconclusive, and we certainly spent a lot of time trying to determine whether or not it was possible to do this. To confirm the conclusions, but it was
mostly that Dr. Short was not using the critical criteria for when he actually reproduced the disease. I see some notes here in front of me in a communication at the third international congress, which you just mentioned, 1939, summarized their studies as follows by saying that this virus has been maintained in culture in the chorealentoid membrane of chick embryos, and in tissue cultures for sixty and forty one passages respectively. This cultured virus has been to possess no pathogenic properties for man and laboratory animals whatever the route of inoculation. This virus has been demonstrated in the bloodstream of monkeys inoculated subcutaneously, up to a maximum of 19 days. In human cases of the disease, occurring naturally, the virus is usually demonstrable in the bloodstream for seven days, and in one case it was demonstrable for twenty eight days. Well. The point was that it was all based on certain lesions, on the chorealentoid membrane, which are non-specific. Then I go on and say without reading all the other things that was said about this. And furthermore, he said that he developed a vaccine of this. He said two doses of this vaccine made from this great passage material, with a week's interval between doses were given and the inoculated persons, the presence of virus circulating through periforal blood was demonstrated five days after the second dose of vaccine, thirty five days after the second dose of vaccine, the sera or some of the vaccinated persons show the presence of neutralizing antibodies while those of other similarly vaccinated failed to do so. Infection of the vaccinated
persons and controls with infected serum from sand fly fever, cases gave inconclusive results. Thus, although the cultured virus was not pathogenic for man, and the infection experiments in persons vaccinated with this cultured material gave inconclusive results. These investigators did not entertain the conclusion that they might not be dealing with the virus of sand fly fever. Furthermore, although an experiments on human volunteers, the virus of sand fly fever had not been found beyond twenty four hours after onset of the fever. The chorealentoid membrane technique seemed to reveal the presence of lesion-producing agents whether it was virus or not--for as long as seven days and in one instance, the twenty eight days. Well, while the experiments of Short and his co-workers, with chick embryos were thus inconclusive, they were nevertheless followed by reports of Russian workers, in 1940. In 1941. Which however did become available to us until there were abstracted later, in April 1943. And that report relates that not only that the sand fly fever virus, that a serum from patients with the disease produced lesions in the chorealentoid membrane which could be prevented by immune serum but also that the whole membrane, and the whole embryo produced typical phlebotomus fever in eight human volunteers. It was added, however, that "injection into seven volunteers, suspensions of pieces of chorealentoid membrane taken near the site of inoculation failed to produce
infection or immunity. Well, without going through this thing, the literature in many instances is mucked up if I may use not a very nice expression. But I think all who do not have the proper discipline of scientific investigation, I mean, there are all these intriguing things when we started. The one thing that was a fact was that serum of patients who had a certain type of illness in an area where phlebotomus flies were present could transmit the disease after proper filtration of bacteria free to others. And it could be transmitted. It also had been established that phlebotomus flies feeding on such and certain times could transmit. That's all we knew about it. There was no facilities at the time. This work was done either in India or previously, or preserving serum so you could compare one with another. And so you just had to start all over again. Naturally we hoped that some of these things might be true. We did it in a disclined way and it all unfortunately turned out not to be true. And it was only later, and by very laborious experiments it proved possible to adapt sand fly fever virus to new born mice, and by very when we speak of adapt, it means actually to select out virulent particles for the nervous system of mice. Did one finally obtain an experimental animal in which you could study this, and which you could then prove that this material, the possessed immunogenic properties, also that it lost its infection. All that was done later.

Q  Dr. Sabin, after the war was over, was it discovered that the Germans had done anything with sand fly fever?
A No. The Germans had not done anything with sand fly fever to the best of our knowledge, but when we come to discuss dengue later, the Japanese had done a great deal which was another terrific source of headache.

Q We will keep that in abeyance.

A We should cover it today, though. It is only twelve o'clock. And I think this is the time to cover dengue and get the war behind us.

Q Well, we will cover dengue, I just wanted--

A Dengue was a much more important disease than sand fly fever. It still is.

Q Alright. I know that, and maybe it would be best to take up your experiences after returning to the United States, and moving essentially to Princeton University.

A Well, I mean there were a number of months of work in the Cincinnati laboratories and then moving to Princeton for the reasons--

Q Let me ask. Was there any way of transmitting this inflammation to other virologists--

A Well, there were publications.

Q Were there any meetings of the neurotropic virus commission?

A Oh, well, we had many meetings with the neurotropic virus commission which were held periodically and in which the information hot off the griddle still incomplete was always discussed. That is why I speak of a virus commission
as being in part a club, and in part a consortium in which information was carefully and critically evaluated. For example, there were extensive discussions by the whole board before permission was granted for me to go in and work on the state prison. These were not decisions that were made just by me or just by Dr. Paul and myself. There were many things that had to be waived because it was the responsibility not only of my own, because I was in uniform. As an officer and the office of the surgeon general--

Q So in essence the moving--probably the moving figure in all this was Stanhope Baine Jones.

A He was--I mean this--you had feedback mechanisms. Like reactions. Ultimately, Stanhope Baine Jones as the general who was the deputy chief of preventive medicine, he had the basic responsibility for the whole army immunological board. And the interaction with the problems in the field. He was at headquarters. And things really had to move through him. And naturally, legal responsibilities in the army had to be considered before human tests were done. He would pass it on to the proper legal divisions. Things were done very carefully. I mean, if you think that a present day human experimentation committee's in universities and those required by National Institutes of Health et cetera, followed certain regulations, it was much tougher in the army because you had to go through all levels before permission
was given to go ahead to do certain things.

Q Was it difficult for you to move from Cincinnati to the institute at Princeton.

A In the first place, I don't know whether I am going to respond directly to your question because that is not the issue. The important issue is why move. And secondly what was involved in the move. Whether it was difficult or not. The need for moving became evident that the numbers of volunteers that would be necessary to pursue the work not only on sand fly fever, because when I had returned from the Middle East, from Egypt in 1943, to Cincinnati, the problem of dengue was brought to my lab. When American air force personnel brought back dengue virus to Hawaii, one of the big Hawaii epidemics, dengue resulted transmitted by mosquitoes that were not Haiti egypti but most Haiti albipickas breeding in palms, palm trees, you know, where water was trapped and mosquitoes were breeding and the business of dengue was made my responsibility. I was the only one in the army with that responsibility. And, material was sent from Hawaii. And I inoculated again some of the patients at Longview Hospital and isolated an agent which reproduced the disease, but it was obvious that many things had to be done which couldn't be done there. We--because suddenly now we had to have insectories with Haiti egypti, and we did set up insectories in Cincinnati, but it was obvious that the number of people, volunteers that would be necessary it would be impossible to do it there.
And also, the facilities at the, my laboratories in Cincinnati were not enough. So, the question was where else to work, and I had to find different places. The important decision of where to go was where there would be enough volunteers. And the Trenton State Prison was selected because of the hundreds available there. And it was within about fifteen miles or so from the Rockefeller Institute at Princeton. Where it would be possible to set up a proper laboratory with new personnel to do the work on the one hand in the prison ward, and on the other hand, in the prison laboratory. So all of this was worked out as an alumnus of the Rockefeller Institute--it didn't hurt have that as an entree, but basically this was an arrangement with the surgeon general's office and the Rockefeller Institute at Princeton where quarters were assigned to me. Animals quarters, laboratory facilities, I had new personnel, I had gotten an army entomologist, Dr. Slessinger was commission and then came to work with me. He was commissioned as a captain. He had worked with Dr. Oletsky at the Rockefeller Institute before, and I had technicians, and I was able to bring over my secretary who was a good executive assistant, from Cincinnati. Well, the move once all of these arrangements were made, did not involve any problems because we could set up a very excellent facility. Because in addition to the dengue work and sand fly fever work that I was doing at Princeton, there was also continuing work of developing
proper standards for the Japanese encephalitis vaccine, which was being prepared on a large scale. So that became my center of operations for the reasons that I just mentioned.

Q Now, to take up the dengue thing, why don't we begin like we began with sand fly fever. What was the nature of the disease and what work had been done previously to your attacking the problem. In other words, what was known about dengue?

A Well, dengue has an old history which almost ran parallel in one way or another with yellow fever epidemics. Because in countries like the United States, during the previous century, and in previous centuries, when Haiti egypti were very prevalent, and with shipping going on, there were occasionally introduced in major ports both yellow fever and dengue. So there are historical records of dengue epidemics and yellow fever epidemics almost occurring in comparable areas of the United States. The common denominator being that they were ports, that there were haiti egypti breeding there. Sometimes even haiti egypti might have been brought in on some of the ships that came in. They were breeding in the water and they were infected. But at any rate, there are records in the United States of things that had been called dengue because it was so different from yellow fever. It was a disease called also break bone fever that had about on the average five to seven days of fever. It was a rash that was clean-cut. People didn't die from it, and when it came it
produced thousands upon thousands. As a matter of fact, I think the last one was in the Gulf states where I think about a million were reported to have developed dengue in the United States in the twenties. There were of course also dengue epidemics. They were mostly imported one. A big one in Greece in the twenties. So dengue has a long history, and it had been established especially by Australian workers, and by U.S. Army workers in the Phillipines because dengue was a considerable problem to the U.S. armed forces in the Phillipines. Considerable problem only relatively. So the Phillipines, the Phillipine science board, the so called army science board became the place where really very fundamental experiments had been done by Siler, by Simmons, and there are of course their monographs published on the subject. What they basically established was a) the virus nature of the infectious agent by experiments quite comparable to yellow fever, the famous Walter Reed experiments were prototyped for that. They were repeated with dengue. It could be shown that haiti egypti, and then haiti albipictus would be able to transmit it, and certain basic characteristics of the disease were studied clinically, and when the virus could be isolated again by tests on human volunteers. At the time of, when World War II broke out, our knowledge of dengue was limited really the important knowledge to what had been learned in these experiments in Phillipines and Australia. There was
no virus preserved because it had not been transmitted to any laboratory animal. There was no way of identifying with certainty that a febrile illness with a rash having the let us say the pattern of previous big outbreaks, there was in effect dengue, and then the problems on it began to appear. Because it began to appear from the American point of view in the Pacific. Because the Pacific is one of the endemic areas. There are many endemic areas on the islands of the Pacific, and some areas that are free. So the first American experience with dengue was really in Hawaii, when some flyers from outlying islands came back in, and there was a fairly large epidemic. I don't recall the numbers of cases reported at that time. But again the thing that gives its epidemiological clinical characteristic which gives it a very high probability to its diagnosis of dengue is that it occurs in large numbers, and that it follows a certain pattern, certain disease pattern, and there was this epidemic in Hawaii. And I was immediately called upon by General Baine Jones to undertake studies of it. I received specimens of blood that were brought--I don't remember some of the it may have been brought as serum in dry ice from Hawaii, flown in to Cincinnati and I transmitted with the serum specimens, having individual pools, if I remember correctly. And the typical disease was reproduced in patients at Longview Hospital. Now, there were many things that immediately arose. I noticed the hemorrhagic type of rash, then the question was, well, is
it possible that there was more than one agent. The hemorrhagic aspects of the disease were not very well documented. So I carried out the studies, the filtrations through membranes with known pore diameter to make certain what was being transmitted. It became immediately obvious that the studies that would be necessary, the studies with mosquito transmission, the studies on the nature of virus, the attempt to find a host, that it just couldn't be done with the small number of persons available at the Longview State Hospital.