A From the beginning assuming—I don't know what to assume how can I know where it stopped.

Q Just go back. What was the last sentence you read.

A Alright. I will go back to the preceding sentence. If the passage of time should prove that immunity resulting from the killed virus vaccine supplemented in at least some individuals by natural infection is indeed long lasting there might never be any need for considering the use of the live virus vaccine. If, however, time should prove that the immunity conferred by killed virus vaccine is a relatively short duration in a large proportion of individuals then consideration might be given to supplementation of the way immunity by feeding the best available attenuated vaccine. A procedure that may be expected not only to reinforce the humural immunity but also to induce that resistance of the intestinal tract to reinfection which could provide the only means for the possible eradication of poliomyelitis. On the other hand in countries where mass application of a killed virus vaccine is not feasible and where polio viruses of varying degrees of virulence are already known to be undergoing extensive spread in the population there would appear to be sufficient justification for initiating at this time trials of the currently available tested lots of highly attenuated polio virus vaccine and such trials are now actually in progress. Finally I should like to say that it
is obvious at least so it seems to me that more work, time and experience ultimately will teach us the best way to eliminate poliomyelitis as a threat to human well being. Closed.

During the discussion that followed the presentation of papers of this congress there were many interesting points of view expressed by knowledgeable virologists and epidemiologists in this field and Dr. Dorothy Horstman working with Dr. Paul reported on their first experience of the use of the aliquot of type 1 vaccine that I gave them and indicated provided very interesting data of the spread of the attenuated type 1 vaccine strain among contact with persons infected following ingestion of the virus. That the study was made in a situation in an institution where this thing could be studied for the first time in a way that we up until that time could not study. And it is interesting to note that she speaks that the speed of spread as indicated by the fact that contacts were already found to be excreting virus four to six days after the test subject had been fed. This was in an institution of mentally retarded children where this, even more readily achieved than in an ordinary family. Then as she presented the data that she and Dr. Paul had accumulated she spoke and also considered the conditions under which further knowledge could be obtained let me read one paragraph here. She spoke that because of the ease of spread of attenuated viruses the problem of the stability of these strains becomes important for the community as for the individual fed. Evidence had been presented today and our experience agrees with this that even the most highly
attenuated strains in certain individuals may show some tendency toward increased neurotropism although by comparison with wild strains they remain still highly attenuated. I am continuing from her. And she continues to say one argument is that it would seem desirable to replace the wild virulence strains which are so widespread in many areas at all times with highly attenuated strains. This argument has logic and force but the practical problem of how to go about doing this and how to evaluate the results adequately is for the proponents of this plan to provide. In highly endemic areas where the wild strains are most prevalent, the incidence of paralytic poliomyelitis is very low and the disease is confined to the infantile age group. In this situation it would be necessary to feed attenuated viruses to enormous numbers of children over a long period of time to achieve statistically significant results. It would also be difficult to administer such a program successfully in underdeveloped areas requiring as it does careful followup and reporting of cases and extensive testing of excreted virus to find out if the attenuated strains are actually replacing the virulent ones and if so for how long.

I want to interject here these are very valid observations and I read them into the record to show just how one had to think at that time. That it wasn't a clear thought black and white decision to make. She went on.

Nevertheless in spite of these difficulties, I for one hope to see such experiments carried out in the near future. Perhaps one way to minimize the difficulties would be to use the attenuated strains in areas where epidemics
are making their appearance for the first time. If attenuated virus vaccine could be administered at the beginning of epidemics particularly those in relatively small population groups, perhaps some islands and evaluations of the procedure could be achieved. Although only after repeated trials of this sort.

I want to interject here that obviously this became not only possible in many places but it became a necessity in many places because even in the United States where a Salk vaccine was used maximally, it became necessary before production, commercial production had provided material for cities like Syracuse and Atlanta and others that I don't have in front of me now who used some of these large lots of vaccine that I had prepared in the face of a developing epidemic with incidentally remarkably successful results. This all was prior to the general use of the vaccine, not in the face of an epidemic of course this also occurred in Israel and Czechoslovakia and many other places with comparable results but this was some years after what Dr. Horstman just said.

She said another possibility. Now I am quoting again Quote. Another possibility would be a trial in a small country which has had and may expect to have epidemic poliomyelitis and has not yet used killed vaccine on any scale. There is much to be said for such a trial at this time. Using the most attenuated, most stable and most antigenic strains. This is a bold program but no more so than was the field trial of the killed vaccine. And now as then it would seem that the right mixture of boldness and
caution are needed. End quote of Dorothy Horstman. Now, let me continue up to the further point. At this stage then in July 1957 we have a situation where the safety of a live virus vaccine, the best strains that I have developed, is logically and reasonably being questioned because of the fact of nature that multiplication in the intestinal tract brings up some changes in the level of neurotropism in a proportion of the total virus population. But the assumption that this would present a danger that was any greater that was already in existence in the spread of the naturally occurring paralytic strains was of course not based on a solid foundation. But the problem was that here we were only two years after the beginning of the use of killed virus vaccine and the decisions were a difficult problem. Actually I think during the period during the summer and on until about November I think of '57 these other people that I had listed were continuing their studies in family groups in such widely divergent places as the University community in Leiden Holland. Children in Mexico, children in institutions in this country, studied by Horstman and Paul and Fox. And in countries like Italy and so on to say nothing of the Soviet Union itself. And by it was quite obvious in that the vaccine committee of the National Foundation was not going to take any action. Its action was dominated by what Dr. Rivers had said earlier. Let's forget it. We've got a vaccine that we know is protected. It is killed. It is killed. It's safe. And we must concentrate on that. But the world health organization called together an advisory committee which Dr. Paul was chairman. I was only one
of the participants. Other participants. There were people from all around the world. And without going into details of the discussion, the main question that was addressed was whether or not the time had come for tests on increasingly larger numbers of people with aliquots of the lots of vaccine that had been so extensively characterized already in the laboratory in trials, in families, in the trials that I had done. The discussion again was pretty hectic on both sides. I think the minutes of that meeting would show how it went. And it adds to the drama of decision which I think everything that was said by everybody was justified sometimes in not very balanced scientific terms. But it was justified. In other words, if it were all done all over again I would be for a similar situation. But, a very important decision was reached by that committee which was not reached by the committee of the National Foundation. And that was that the time had come to conduct such studies under very carefully controlled conditions. By investigators who would have the capacity to carry out very careful studies not only on antibody development, clinical followup viruses isolated etc., what was happening on a larger scale. This resulted really in breaking the ice because although gradually the numbers were increasing through these studies from what we had hundreds to thousands, to some several thousand in Mexico and places like that. The first request that really in my judgment was important was a request that came in from Czechoslovakia from the Ministry of Public Health that requested that I send them 200,000 doses for use in vaccinating children under 14 years of age with a very
careful followup and study. This was to be done during winter months and with an organization that appeared to fulfill all the requirements that the world health organization committee had required. Of course the vaccine was sent there. And a most excellent study was carried out with a public health organization that was as good as any that you'll find anywhere with excellent people doing good work in the laboratories of the Institute of Epidemiology and that showed many very important things. It was very large numbers of children were bled prior to administration of about 150,000, 140,000 or 150,000 doses. It was the largest one. I am trying to think now whether that was in '57 or in '58. I think it was in '58, if I am not mistaken. You will have to check that date because prior to that there were studies in larger groups being carried out in the Soviet Union which influenced I think the--yes it must be in '58. I'll tell you why if you will stop moving the pages. Because the meeting of the WHO committee was in November 1957. The report didn't come out till late and the trial in Czechoslovakia started I think in December so it must have been the following winter. In the meantime in 1958 something like 200,000 or more maybe more received the type 2 vaccine during an epidemic in Singapore as a means of determining whether a type 2 attenuated virus would interfere with the type 1. And as I said there were uses on increasingly larger numbers of people. There were studies on larger numbers in the Soviet Union both in the Leningrad and Moscow areas. I was very intimately involved and very active during this period, so that by I think it was
December 1958 Czechoslovakia was ready to mount a very important test. And that was important because only part of the population was vaccinated because a large portion of Czechoslovakia, the contact in the home, were without antibody and it was possible to see in the field whether under these conditions it would spread from vaccinated to unvaccinated. And because only a portion in each community received the vaccine whereas another portion was left for control to see what would happen spontaneously and what the spread would be. That yielded results of the very greatest importance. So that when the data were in. I think that there was another experience before that when Czechoslovakia had an outbreak maybe it was in the summer of '58 in which some of the vaccine. I can't remember all of these details. I've got the field trials. But we will come to field trials later. I think this is an appropriate place at which to stop the--

Q Any number of questions to ask you. One of the questions I would like to ask you has to do with the people who were in your laboratory at the time and the work that they did. Remember I spoke with you earlier as a leader of an orchestra and these are just names but they are also people. Now we have spoken somewhat of Channick but there was a very interesting man who worked with you for a time in '55 and in '56 named Anton Schwartz and I wonder if you would tell me something about Anton Schwartz.

A I think I would not like to concentrate on any one person but to perhaps try to recall--

Q Alright. Why don't you--
A What the situation was beginning in 1953. The names of many of the coworkers appear on some of the publications of the period. So if I go back to 1953 when the first attempts at attenuation began the two people who worked with me I didn't have any experienced person who himself contributed anything but I must confess hands because they were people untrained. I didn't have with me during this entire period of extensive work anyone who could have taken over if I would drop dead. Now, this is the way it was. So I had the respons--and this is one of the reasons why I didn't have time for writing. I had to have first of all a large team of people who came, who incidentally came for training, technical help. But let us state. Let's start in 1953.

In 1953 Dr. Walter Hennison a very fine young man from Germany after he had spent some time working with Christopher Andrews on influenza asked to come to work with me on polio. He had no experience in polio or even very much in tissue cultures because when he worked on influenza he worked in eggs and so on. He turned out to be a very wonderful person and a tremendous help. Another person I had in the laboratory at that time who had been with me longer was a man called Johann Vinceller who was veterinary trained. I met him in Leiden. He came from Professor Verlinda's laboratory. He worked with me during the period of studying properties of polio viruses in mice and monkeys and so on. He was a person without any initiative whatsoever. He had reliability however and was very helpful in the
division of labor. He subsequently left to work in the bureau of laboratories in New York State Health Department so that when the time came to look for a place in which to do some human tests he was already working in the bureau laboratories at that time.

Then Dr. Channick had been in my laboratory from 1949 on but working entirely on hollow (?) viruses. The equine encephalitis viruses, St. Louis encephalitis virus, working entirely on hemic glutination. Then he was off in Japan and when he came back he joined the effort in the attempt to determine what would happen as I have already mentioned if one could possibly select variants in eggs and in the skin of monkeys. That was his, his effort.

In 1955, early in 1955, '56 when it became evident that this was not it I realized that Dr. Channick was very, exceptionally competent person in our work on hemo glutinants, hollow viruses he showed a great deal of initiative. It was certainly quite different from the others. And I thought it would not be fair for Dr. Channick to continue working in a field in which I had all the initiative. I had all the plan that my deep personal affection for him which I felt called for him becoming involved in something, in a field in which I wasn't doing anything so that it would be apparent that he did. He was responsible. He would write it up. He would publish it and he would develop his own identity. And in discussing the various fields. I am very much interested in this now because with events of the future years and the
great stature that Dr. Channick achieved is the fortunate decision in which I think I was partly involved of my telling him that although there were many people engaged in studies on respiratory disease. There were certainly lots of people engaged in it, that that was a field that offered great opportunity for a person who had his clinical training because he had very good clinical training. He finished excellent training in Chicago and pediatrics before he came to me. And for one who had acquired many techniques in virology. There we were in the Children's Hospital Research Foundation. We had a wonderful clinic. I thought I said to him at the time that your career combining both clinical and virological training utilizing the respiratory disease problems that present themselves--

X I knew he'd do this.

Q Hi.

A I am sorry. Utilizing the many respiratory and disease problems in children coming to our hospital and clinic would present a special opportunity because you have at least two good disciplines. Well he was enthusiastic about that and he went off entirely on his own and within a year isolated a totally new virus, parainfluenza that subsequently came to be called parainfluenza and showed its important ideologic role in croup in children. And his subsequent growth as an investigator in the field of respiratory disease has been appreciated by everyone and a source of constant gratification and joy to me. So he was quickly moved out of the group.
Now other people were with me. Dr. Romulus Alvarez was with me and he had. We had a division of labor because of the many problems that were presented by the non polio viruses in the intestinal tract. That became his special activity. So that in effect while Romulus Alvarez was in my laboratory before he left us at the end of 1956 to go back to Mexico he worked entirely on echo viruses and so called orphan viruses. And was responsible really for making a very excellent contribution from our laboratory to the total field. So he was not very much involved in polio work. Now, another person who then came along and let me just look up because I list some of the people. But let me, in order, was Anton Schwartz.

Anton Schwartz was a very remarkable person. He incidentally became in subsequent years director, vice president for research of Dow Chemical. He is now a director of Dow Chemical in Europe for biologics and so on. Anton Schwartz was a remarkable person in the light of his previous history because he had practically no training in virology or in the laboratory. He was the son of a Prussian general when World War II broke out. Some people think that everybody who is called Schwartz is Jewish, is not. And when he was 16 he joined the Nazi navy voluntarily you see and as soon as he went out his ship—he was sunk and he was saved. He returned to Germany. He was then put into the army, sent to the Russian front. He was captured right away, recaptured right away, recaptured and finally from deep in Russia somewhere he wandered night on all fours back to the German
lines almost towards the end of the war and then when the war was over he studied medicine in Germany and he came to the United States to do an internship. He met an American girl whom he married and then was doing an internship across the river in Kentucky. He was just about completed and he must have been reading in the newspapers the work that I was doing and so on. Because really I don't think he knew the literature very much. And he came in one day to me and he said I would like to learn the techniques of tissue culture how you are working with polio. I have no experience in the lab I only have an M.D. degree and my experience in a hospital and he was a very likeable person and he came in. I trained him. He began to work with us and he worked with me I think up until oh I guess during the period especially when the active tests in Chillicothe were going on. He was doing a lot of the laboratory work, but sometime I think in the end of '55 or early '56 his wife developed, his wife developed multiple sclerosis. He needed more money. He was offered a job by Pittman Long and he left because I notice that in the report for the period of July 1, 1956 he is no longer with the group. And the techniques that he learned in our laboratory he began to apply in a very practical way to see if he couldn't make a much more highly attenuated measles strain that had come out of Enders laboratory and was used by Merk Sharpe and Dome and he did succeed. He made a lot of money for Pittman Long and it cost the people a lot more because it was patented. I don't know. It is over a dollar
a dose and all sorts of things. Well at any rate he quickly moved up the ranks because he had organizational ability. It was an important contribution, scientifically, practically as well as commercially. It forced Merk, Sharpe and Dome finally to develop. They didn't want to buy and pay the fees to Pittman More or Dow Chemical later so they had to go to work themselves and make a more attenuated strain and do all the tests but Schwartz's work forced them to do that. But after that he never did very much because he became a good organizer and the vaccine production, biological production field.

Then other people came to my laboratory. Dr. Steven Wong who had worked with another former associate of mine in Syracuse, Dr. Feldman. He came from Feldman's laboratories. So that in 1956 he was already with me and stayed with me for a number of years. And he was extremely helpful. He was Chinese, not born in the United States, but he emigrated here, had immigrated here for good and a very remarkable, meticulous person. He was especially helpful I remember in all the tremendous multiplicity of work with plaque picking plaques and growing things from plaque. I mean it was a tremendous amount of work. He was very very good at that. But also, he had no initiative. He did very well in things that I would outline and do. And we worked together. But he was a Ph.D. but he was very helpful as an associate. Another person who came to work with me during that period, some would be with me let's say for six months. I have a note here that in July of 1956, Dr. Hans Gerth from Germany came to work with
me and worked with me for six months. He again came for training. Well he was very helpful because I taught him how to do tests for polio antibody and he finally ended up by doing hundreds of tests for polio antibody. He learned the technique which he later applied when he went back to Germany.

An American girl who had had a Ph.D. I think from Cincinnati came to work with me in August, 1956, and she also was not originality but she was very helpful again in doing a variety of techniques that were involved. Again, she needed training in this field of work.

Q Well who was she?
A Pardon.
Q We didn't mention her name.
A I said. Oh, Gertrude Lum.
Q Gertrude Lum. You said an American girl.
A Lum, Ph.D. Right.

Another person who came to work with me at that time and worked for perhaps a year or so was Luigi Dardinoni from Palermo in Italy. I had the pleasure of having a reunion with him only just recently twenty years after he came to work with me. He is a professor at the University of Palermo now. And he again was helpful I forget in which particular field but he had to learn.

Another German who came to work with me was Dr. Reinhart Weigand—Wigand—and he had stayed for some years but he became again predominantly involved in work on echo viruses and coccaccki viruses which was another aspect of the work going on in the laboratory.
I had good technical help. I had one technician who had been with me from the very beginning who had a very green thumb in tissue cultures. But we were a busy hive. I mean there was a young lady who, a doctor who came to work with me. It slips my mind now. Rouhet. She died about a year ago. She worked for about six months but again learning techniques. And studying peripheral things. So that this was the load and I myself was not only pushing people around telling them to do this, do that. I pushed myself around. Because I was the man to commute back and forth several times a week doing 200 miles a day from Cincinnati to Chillicothe. I was the one to inoculate thousands of monkeys. Although I had trained my technicians to do autopsies I was the one to cut the stuff for fixation, to read all the thousands and thousands of slides and how I survived that period I don't know. In addition to traveling to the Soviet Union, with the program of collaboration that began in 1956 and went on. It was a very usefully I would say for many years and traveled to Mexico and traveled to other countries. And trying to write some things in between. And being a father to children late in life who, the first one having been born in 1950 and the second on in January 1952. It was a very, very hectic period. It was a situation in which a division of labor by a relatively large staff made it possible for me to do all this. But it was not a kind of division of labor where responsibility for initiative was involved at all. I mean even. It was my responsibility not only to ask that certain things be done, but I found it necessary because I was dealing with people in training to
write the protocol in detail. I am not talking about the echo virus work or some of the other things which Romulus Alvarez took off very well on—I am not talking about the work which Channick did in eggs and in monkeys although we would discuss the protocol together and he was able to take off. But in almost all the other things I would have to write the protocol. And frequently I even wrote the notes and the observations because I was very jealously concerned about the reliability of the data that came out.

Q Now when you know, people you say, Anton Schwartz showed up—did you just take him because he showed up or did you—

A Oh no. There were other people. He made an impression on me.

Q Yes. Now—

A And I needed help.

Q Oh, you needed help. Now there is one kind of help we haven't discussed and it is really important given the thousands of monkeys and chimpanzees that you worked with. Namely, who was your animal caretaker?

A Well I was very fortunate in having at least one head man. I forget his last name. His first name was Scott. A very remarkable black gentleman who had been with me from the very beginning when I came to Cincinnati in '39, he became an extraordinarily wonderful monkey handler. I mean he could handle everything—mice, guinea pigs, rabbits, whatever animals I was working with, hamsters. And, as we had more and more animals and used floors of animal space we took on more help,
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whom he trained. He was the foreman. He was in charge. And certainly without their hard labor that work also would have been impossible. Also the facilities provided by the Children's Hospital Research Foundation, although they were not very large by comparison of what some people demand, there is no question that in addition to a whole floor of laboratories having two floors of animal quarters in which at any time we could have 350 monkeys. Anywhere from twenty to forty chimpanzees at a time, thousands of mice and other animals that we would have to use as well as the facilities for growing tremendous amounts of tissue cultures and the preparations of materials from plaques and all this. It was really a beehive of activity. But it was done on a scale, and the cost, which, in the light of present activities in other places just had no comparison.

Q I think with just this we ought to stop here. I really don't--

A I would say this. Please do not--just turn it off.

This chapter is a chapter that has not only scientific relevance but is one of personal traumatic experience. It was obvious that the time had come at one stage of this work where it would be necessary to go from using the young adult volunteers to children. And while the results on about 200 volunteers or so as I indicated showed that there was no reason to suspect that the material was dangerous. How could I go out and ask for children--not in institutions but in families, living in families the way they would have to be immunized. How could I go out and ask any family for, to volunteer in these kinds of experiments if I who at that time had two daughters aged
five and seven and my own wife whom, incidentally I had tested for antibody before and found that they had no antibodies against any one of the three types of polio virus. My wife spent her early life on a farm, isolated in Illinois. My children living in Cincinnati had never acquired, and they had not had Salk vaccine because I had a certain feeling about the vaccine, so that I wouldn't give it to them. So I said unless I was prepared to make a decision, to study this vaccine in my own children who could not decide. I mean if ever there was a question of ethics, a parent deciding for children that was not only giving them something, but bleeding them and swabbing and collecting stuff and all that. And my own wife, if I wasn't prepared to do that, I really felt that I couldn't go out and ask anybody else. And furthermore, after I had done that, if I would decide yes then I could say I have done it on mine. This is how I feel about it. This is what happened. But if I could not decide yes in my own family, I felt that I couldn't go and ask anybody else. I would have to quit and leave it up to others.

So, Sylvia and I--we discussed it. Sylvia my wife and I, we had repeated discussions and I spent sleepless nights. Sylvia of course, said whatever you decide. You are the one to make the decision. I spent sleepless nights and then I decided that all the years of work that have led up to this point where it had to go or else stop was depending on this and I decided yes I should.

Q  My God.
A Now hold everything.

Q Okay.

A It just so happened that my two children were playing always in the most intimate associations with three other children, other neighbors, the Fishers and it was only just that I should discuss this with my neighbors, the Fishers and tell them the decision that I had reached about my children that their children would either have to stop playing with my children or they could make a decision themselves what to do. And so they said if you think that your children should be involved in this, then we want our children to be involved the same way. Not as contacts but as people who would participate. And moreover, we want to participate too. And when I did tests for antibody it turned out that all three children were also triple negative. And if I recall correctly one of the parents was also triple negative. It was one of the Fishers and the other one had antibody for one. So here was the nucleus. Five triple negative children ranging in age from four I think to eleven, twelve. Two triple negative parents and another parent with some. And this study was then begun. And what can you say to a five and seven year olds, particularly when you have to stab them, and the needles weren't as sharp in those days, you didn't have needles which were disposable because I had to bleed them before, bleed them at intervals because I wanted to study, not just on effects. I said we are going to collect your feces every day or every other day. You are going to have to do it in a little container so I can take the stuff out.
I am going to have to bleed you. I am going to have to do this. I did innumerable things on them. My poor suffering children stood up very well because this was quite a period. Because I gave them first type 1 and then I gave them I think type 3 was next and finally type 2 and my neighbors children and my wife and the parents and these kids, they were just wonderful. It doesn't mean that sometimes they didn't cry but by God, when they went to the toilet, they would do it exactly as I wanted them to. It was the most wonderful group I ever had. But I shall never forget the sleepless nights before I made the decision. And it was after this study was completed that my friend Professor Verlinda at the University of Leiden in Holland, he had a very big family. I think he had nine children and there were a total of about 150. I don't know how many families, faculty members and children, he did a very excellent study which showed under the hygienic conditions that obtained, and then of course, this open appeal for studies on other children. Children in institutions in the United States, children in families by Fox in the United States and it opened up the field. So that was one thing that was part of an important decision of this aspect of the work.

Q Well, I don't want to leave you off the hook here so easily.

A Alright. Go ahead.

Q Debbie had been infected with a chimpanzee cold virus that made her awful sick before you had gotten into this test.
A I don't remember the details and I think this is a red herring which is a separate thing.

Q It is not a red herring because a psychological question. The psychological question is this. The children had come to you late in life. Here they are, five and three--

A No. Four and--well there was eighteen months difference.

Q Eighteen months difference. It is more than taking you know, a child. Here is a joy that has come to you late in life and you don't know what it is going to do. It is more than a sleepless night that is involved.

A The point is that obviously the thing that kept me awake and the thoughts--this is not the place for it, but I do want to comment on something. You are quite right that this was not the first time that I had studied them because as all other children they had become sick with all sorts of things at various times and one time before this thing some cousins came to visit them from Illinois. They all became sick, and poor Amy had a fever 104, 105 and her tonsils were with white spots and they were in the middle I was concerned that she may have had a strep infection and she was going to get rheumatic fever and all that, but before I would do anything I would make cultures. Now I was looking really for hemolytic strep. But I had all those tissue cultures going on and I inoculated some monkey kidney tissue culture. There was no hemolytic strep. It was a cocci sacki. It was an echo virus. Or was it a cocci sacki B3. I think it was cocci sacki B3 as it turned out. That the cousins brought from Illinois, spread throughout the kids and that was responsible for a chain of infection in the
family. On another occasion an illness broke out Amy, Debbie, I had it. There was some stee ateric (?) I was going to cover that later and in more detail. But this is in relation, to the children. And again I obtained cultures because it was something very unusual and it turned out to be identical with a virus that was going around in the chimps, that I isolated from chimps which I probably brought into the house, etc., etc. So they were not unexposed to having a throat swab made. And having a blood specimen taken because I, in order to identify the cause, they had already been exposed. Little children, you see. I mean this is one of the situations where the decision was whether or not it was justified to do it. And I would say, why did I finally believe that it was justified to do it. Because there was a great principle involved here. The principle was the elimination of poliomyelitis as a crippling disease once and for all. Not only--it was a big thing because if it weren't important I wouldn't have done it. I felt if the importance was so great that it had to be done that if I did not think I should do it on my children about whom I felt as you described, as I did because of the circumstances when they came. Then I couldn't ask anybody else to do it. It is a basic ethical question which I think comes up in many other situations.

Q  Now I will let you go and you can go on to the other one, about what I wanted to--

A  How much time do we have?

Q  We have about ten minutes.
A One of the interesting relationships that developed as a result of this work was with Soviet colleagues that began in 1956 and played a very important role in the subsequent years at various stages of the search for the optimum strains of vaccine and also during the period of extensive field trials when we had reached that point. And also subsequently in widespread use of the vaccine. Early in 1956 a group of Soviet virologists made up of Professor Chimokov of the Institute of Poliomyelitis in Moscoe, Dr. Boris Shilivah, his wife and co worker of the same institute, Professor Anitole Alexanderovich Smoradinsov from Leningrad whom I first met in 1953. I had not met the Chimokov's before. Arrived in the United States on a special mission. This was early '56 to learn how to make Salk vaccine because up until 1954 there were practically no significant outbreaks of poliomyelitis in the Soviet Union. In 1954 the assumption of many delegates to International Polio Congresses which was that the socialist way of preventive medicine in the Soviet Union protected them against poliomyelitis was shattered because the first big epidemic of poliomyelitis involving about 18, 20,000 paralytic cases occurred, and then because the Soviet Union is a big country, another one occurred, involving other places in 1955. The Salk vaccine was out in the United States in 1955 with the report of the committee. And production had begun, so early in '56, they are in the United States to learn how to make Salk vaccine. They knew of the work that I was doing in search of a live virus vaccine, live virus strains. And they came to visit me in Cincinnati. I think it was February and March, 1956.
I showed them everything I was doing and Dr. Smoladinsev especially who would not have the direct responsibility of making and being in charge of making killed virus vaccine such as Chumokov and Dr. Boris Shilivah had that specific assignment from the government was especially interested in what I was doing and asked whether he couldn't collaborate in some of these studies. He said that there were many opportunities for carrying out such tests in the Soviet Union because mothers worked. There were special centers to which children are brought. They are there and various kinds of studies of potential benefit to them are carried out. And he said he had access to thousands of children in institutions which I didn't have and he said they haven't any Salk vaccine to use yet. There was no problem in the Soviet Union of what shall we do, shall we withhold Salk vaccine and new tests. No, he says. So they invited me immediately to come to the Soviet Union. There was--they had their first big national congress or what they called Infectionists, Epidemiologists, Microbiologists that was to take place in Leningrad in June 1956. They decided to invite representatives from different countries as observers. Not to present anything but as observers, as guests. And this was the first time. This was just two years after Stalin died. Things sort of opened up. And I was invited from the United States. Christopher Andrews from England. Sven Dard from Sweden. I mention all these names because ultimately it was significant. From France they invited a manufacturer. Maurier, Charles Maurier, the president of a big biological--. From Italy they invited a man from
Milano again a manufacturer. I am trying to think whether there was anybody from any other Western country. At any rate we were the observers. I spent a whole month. They took me around from the north to the south to show me the Soviet Union. I gave a number. I was asked to give a number of lectures which I gave and then the last one was especially important. This is not at the Congress but during my subsequent stay when it was all over. And at that time really I gave lectures the one, the most memorable one was one on the genetics of resistance to infections with certain viruses. The chairman of that meeting was a virologist Victor Ishdonov who was deputy minister of health. He was a very important person. And because of the title, genetic resistance, the turn out was in thousands. And the hall wasn't big enough, and he said to me, he says, you know I am surprised to observe here that there are many more geneticists than there are virologists. And it was an obvious reaction to the fact that Lisinko isn't was no longer the part of the dogma and geneticists if wanted to hear any discussion involving genetics and as it turned out this was part of my genetics work I discussed before in which I showed that resistance to certain albo (?) viruses was determined by a single gene in the Mendelian laws applied and it was a terrific experience. At any rate during that period we then decided on a system of collaboration in which certain strains that I was testing on volunteers in Chillicothe, that aliquots of the same material would be sent to Smoradinsov. Smoradinsov began with this work.
Chimokov continued his responsibility which was to set up the best possible production of the Salk vaccine. Smoradinsov in Leningrad would do these experiments. This was a very excellent opportunity to learn by techniques that he was to use exactly the same that we used, what was happening under conditions in children because he tested them for antibody and so then in effect I would say. I am trying to think now whether the first tests on children were done before I tested my own or not. Now, obviously some children had already received the mouse vaccine that Koprovski had used so that I was not the first one to use children. But I think I was the first one to use children under these particular conditions in my family. I cannot now remember to be quite frank whether Smoradinsov had used the previous strains of vaccine. I think he did in children's homes in Leningrad before I did.

At any rate this relationship continued and I would go back every year to again go over the data and the results and to have lectures in the Soviet Union on the status of my work. I remember one particularly in 1957.

Q  You are going to have to stop here.

A  Alright.

END OF TAPE