Q Dr. Sabin, I would like to begin today by getting some reminiscences from you about Jonas Salk and the development of Salk vaccine as you remember it now. Do you remember when you first met Salk?

A Well, I think it goes back probably to 1938. I don't know when he got out of school now but when I was at the Rockefeller Institute I think he would come to visit or—well it is difficult to recapture dates—but I think he went to—I don't remember when he went to work with Tommy Francis, whether it is '38 or not and I think before I left the Rockefeller Institute I knew him and I remember particularly either 1938 or '39 or both when he and his wife were at Wood's Hole and I was at Wood's Hole and we spent quite a lot of time together then.

Q And I guess during the war your careers diverged a great deal.

A They didn't diverge because as you recall, there was a commission for influenza and other epidemic diseases in the army which had different commissions. Dr. Francis was chairman of the influenza commission. Dr. Paul was chairman of the Neurotropic Virus Commission. I was a member of the Neurotropic Virus Commission and we used to have meetings, and Jonas Salk worked with Tommy Francis and reports of the work on flu vaccine would come at meetings
of this board and the various commissions. Well as a matter of fact we had quite a bit of contact during the war years although he worked on influenza and I worked on neurotropic viruses, you see.

Q And so it is really after World War II that he began first to work on polio virus.

A It was quite a bit after I think. '48 or something like that.

Q Did you have much contact with him while he was typing various polio viruses?

A I think the record shows that this collaborative typing program was set up by the National Foundation by a committee of which I was a member. And we actually, several of us working had already accumulated evidence that there were different types. Of course it was already known from 1931 or 1932 work that Fawn and Burnett had done that there was an indication that there were different types. But what one didn't know was how many different ones. So basically this was a very good collaborative program that was set up, and he was only one of a number of collaborators and those of us who had worked longer at it--many isolations from human beings to contribute. In my laboratory we had been isolating viruses from human beings for many years. We were particularly involved and we had strains that we could submit that we knew--didn't cross-
immunize against each other. But that was a collaborative effort during which many of us worked very closely together and the publication of this shows the kind of inter-relationship that existed.

Q Now, there are of course, at the beginning of the 1950s, '51 and '52 especially, marks really the beginning of interest in vaccines. Now, were there--was there division among virologists as to live virus attenuated vaccines and inactivated vaccines?

A Well, you say among virologists. You have the general philosophical discussions and concepts which are relevant to experience with others, experience that Rivers himself had where he could never immunize with an inactivated small pox vaccine, or vaccinia. Had to use 31. Or the experience that others had had with yellow fever. It isn't that the Rockefeller was all set on having live virus yellow fever vaccine. It is that the killed yellow fever vaccine just didn't do very much. So there was this accumulated experience that with viruses, the ones that really worked were the attenuated viruses that would produce an infection and the resulting immunity that simulated that which followed natural infection. The experience with killed virus vaccine in polio was a matter that had a certain unhappy background. It goes to the '30s which is well known. And then as you know, Isabell Morgan and the Bodein and Howard Howe had started over again with formulaized vaccine but using spinal cords. And it could be shown that when it
was inactivated by test certainly that now wouldn't mean anything for the exclusion of residual live virus. That it was possible to produce some antibody in monkeys and actually this is well known now. The probability of having something practical really didn't come forth until the development of the methods for growing cells on a large scale. It wasn't just a demonstration by Anders and Fred Robbins and Tom Weller that the scitopathologic effect could be observed in cells and tissue culture. There had to be developed techniques for growing cells on a large scale so you could get a large quantity of the vaccine. This took several years and it really didn't come into being until about 1952, '53. That work was not done by Enders' group. Other people who entered the field and the fact that antibiotics were available at the time made possible progress in cultivation on a large scale. Of course the first results were with lines of human cells—malignant-like neural cells for example. A lot of work was done in helo cells (?) but nobody would use a cancerous cell, a human cell, as a substrate for making vaccine for human beings. So this actually was the stimulus that led—and if you ask me now who some of the people were involved in it, I don't know off hand. I know Melnick was one, and others who were really concentrated on propagation of monkey kidney. At first a great deal of work was done with testicles and monkeys. I remember my early experiments in tissue culture are also testicular but gradually, you know, a lot of people get into
the field, techniques for large scale cultivation of monkey kidney. And this sets the stage for producing large enough quantities. Incidentally that wasn't done by Salk either, you see. Set the stage for producing virus on a large enough scale to be able to inactivate it, to test for antigenicity and at least to be able to centrifuge or at least have the virus without too much cellular tissue.

Q  Dr. Sabin were you aware of Salk's work on inactivated vaccine?

A  Well he wasn't the only one working on it. Let me say that one of the good things that came from the National Foundation was that they had very frequent meetings of its grantees. And that everybody's work was presented and criticized. So that I would say that anybody who was a grantee of the National Foundation--there was hardly anybody working on polio who wasn't--so that meant that everybody working on this in the field at least in the United States knew what was going on. And there were other people working on inactivated vaccine too. There were things like ultra violet tried and some other ways of inactivating other than formulet. But as I said on the BBC broadcast when they asked me a question about that period, that while there were many other people who were doing experiments on inactivated polio viruses grown in tissue culture, Jonas Salk really took it on and did a very energetic job. And he very quickly got out in front in his field because of the well organized effort on a large scale, on a scale that
didn't compare to the piddling small scale operations of some other investigators. And he forged ahead.

Q Now in--he forged ahead very quickly so that early in 1953 there was a meeting of the Immunization Committee of the Foundation which you were a member where the possibilities of working with the vaccine, developing the vaccine for trial, came up--do you remember?

A There were so many meetings. I don't. There were many meetings in which I participated and I remember we used to meet in special places where we would all spend the night together. I remember some nights when Jonas Salk and I were up till after midnight discussing things. Of course, he knew and everybody else knew that I was working. We were not competitive countries at all. And we had many conversations in which I tried to suggest things that ought to be clarified in order to--

Q Can you remember some of the things that troubled you at that time about--

A Well, I remember things that troubled me but I can't in remember detail. One of the things that apparently I was opposed to from the very beginning is the use in 1953 when we already had various attenuated type 1 viruses of a kind that we could put in a million or ten million into the brain of a monkey and it wouldn't produce any paralysis whereas the Mahoney strain which he was using, one tissue culture dose would regularly produce paralysis and I used to plead--I would say for heaven's sake, don't use a virus that is so
virulent when there are all sorts of other things. I said I have the Mahoney strain from which I selected a variant that is one at least ten millionth less virulent than the Mahoney and it multiplies to very high titers in monkey kidney tissue culture. Why don't you use that?

Q What was the objection?

A I don't know. It was one of those difficult things. It was a matter of obstinacy. I had already started with this and besides I am going to kill it. It is not going to make any difference. You see it follows. This is the old story we had such discussions at the meetings again and again. The inactivation follows a straight curve and I can predict that there won't be a single virus particle left. There were others who said look sure it drops off but then there is always a period--and there can be all sorts of things that can be--aggregates of virus particles. There can be all sorts of other things that may just prevent one from being inactivated that you won't pick up. There may be one in a liter or ten liters which you won't pick up by a test that when that goes into a thousand or ten thousand children you are going to pick it up. Therefore, don't use a virulent virus. Well, we all know. Everybody knows what happened. Because by not paying attention to this came the unfortunate so-called Cutter accident that some people have called the Salk accident because the procedure really wasn't good enough.

Q But that isn't--
A  Which caused paralysis in so many children.

Q  You know, the use of the Mahoney strain ultimately was decided on by committee.

A  You know, this is a very interesting phenomenon when scientists meet, particularly when committees are made up of people who are not supposed to have any axe to grind and don't work in the fields themselves because they say if he works in the field, he has a bias. So you pick very good, highly respected people to serve on a committee. And the things, the gut feelings that an investigator in the field has is very difficult to graft onto a colleague however experienced he may be. And then there are certain--oh, I have attended so many committee meetings--there is a certain emotional situation that is created. There was pressure. There was no question that the pressure on the committees were--and that moreover, anybody who disagreed was sort of waived out of the committee sooner or later. I think Smardell was another person who used to raise hell in the committees. But it was a situation as you realize from the record now that even after this horrible accident occurred, and then when the question was well, it was probably due to the fact that this Salk didn't work out--that Schope played an important part in this. That the fact that the vaccine wasn't even filtered, see. To get rid of other things. That it remained in aggregate so it wasn't properly inactivated. And then theoretically, it was assumed well
if the vaccine will now be filtered before formulent is added to it, then all these things should be overcome. Well, much of it would be overcome but again, based on the assumption that there might never be an occasional uninactivated particle which won't make any difference if the virus was not virulent, but it would make all of the difference if the virus was virulent, as this one was. This was the most virulent polio virus, in the whole firmament of polio viruses that had been tested. There was none more virulent. And as a matter of fact as you know, subsequently, when others got into the fields outside of the United States, of making killed polio vaccine, this virus was not used, despite the insistence of Tommy Francis and of those of his friends who helped carry the day because to begin working with another strain would have delayed things a little bit and the assumption that there was no more problem in the United States after that despite the use of the Mahoney strain is not--is not valid. Because as I have indicated in a number of publications, the reports that came in from the Public Health Service during the years up to 1960 when millions of doses were administered and there were paralyzed cases occurring within thirty days after administration of the vaccine in summer and winter and when the first paralysis was at the site, in the extremity that was inoculated--in the leg if it was in the leg--in the arm-- if it was in the arm, right or left and those were called correlated cases. But, under the pressure of the moment and still the Public
Health Service said there is no evidence. They don't say that it isn't incon--they say there is no good evidence. Well to my mind there is evidence.

Q So, from the beginning you are against the use of the Mahoney strain.

A Virulent strain. And I also remember during those meetings of insisting on other things that needed to be tested without assuming that there will be no problems. I was concerned obviously about the fact that the virus had to be grown in tissue culture which had sera in its medium--bovine serum--ten percent. And the way the vaccine was prepared, the whole tissue culture fluid was used. And I was very much concerned about subsequent doses producing hypersensitivity to bovine serum, not so much the small amount of monkey kidney that was there which also you couldn't eliminate and I remember insisting that guinea pig tests be carried out. Well some things were done, but it was a big hurry. I don't want to be completely--seem unfair as to how the hurry was justified. If I remember it went something like this. If you can save ten or a hundred or so from getting paralysis by going faster than by going slower, well some of these things you will find out as you go alone. You see. It was this messianic fleeing which caused a lot of trouble. Because if there hadn't been that political pressure after the horrible accident that happened with the first mass use
public would have been turned off. If the political pressure on the Congress, on the Surgeon General, hadn't been what it had been the public would have been turned off and there would actually have been a delay.

Q I don't want you to jump ahead. In April of 1954 before the field trials began, you became very vocal in your opposition to the holding of a field trial.

A No. It wasn't to the holding of the field trial. It was to going into the field trial where hundreds of thousands would be inoculated with a vaccine made out of virulent polio virus because not only I because among those who maintained that having demonstrated that a certain concentration of polio virus after inactivation can give rise to an antibody response not only in monkeys but also in children. The next step is to go ahead and use the least virulent strains. Again on the assumption that the escape of virulent virus particle from inactivation by whatever mechanism could be enough to paralyze, to kill a child, and that you didn't have to. Yet it was impossible, and the great pressure that was created, to have the process slow down just a little in order to change the strain of virus.

Q Dr. Sabin, do you have--there is some correspondence between yourself and the medical director of the Foundation at that time I think which characterizes part of this debate and I wonder if you would read it into the record.

A Well, I can read it into the record only because you have found it. As a matter of fact, I had forgotten.
Well, this letter from--

Q Hartfinder (?)

A Hardland. Hartland and Riper says in part "it has been reliably reported to me that at the meeting of the American Pediatrics Society held in Buck Hill Falls last week you were quoted as having told several individuals that you had determined from officials of the Eli Lilley company that when sufficient vaccine was tested, these tests consistently demonstrated the presence of live virus. You are further reported to have called this the 5 cc test."

The letter continues. "I cannot believe that you would maliciously spread such rumor. In any event, because of the authenticity of this information reaching me, I will appreciate a reply at your earliest convenience." And here is my reply. "This is in reply to your letter of May 11, 1954, I was not present at the meetings of the American Pediatric Society and I do not know what statements were attributed to me. However, it is true that Dr. Powell of the Eli Lilley Company has informed me that when he tested larger amounts of formulized tissue culture in bottles instead of in tubes he was able to detect living virus when the ordinary prescribed techniques failed to reveal such virus. This is an important although not a surprising observation because it indicates that at certain stages of inactivation by formulent the amount of residual virus may be so small that tests of large amounts would reveal it while tests of smaller amounts would not. I don't know who
the individuals are who quoted me. But in discussions of the problem of the formulized vaccine, this is a fact of scientific importance and not 'a malicious rumor.'" Well, I had practically forgotten this, but I think the use of the word 'malicious' by the medical director of the National Foundation for Infantile Paralysis brings back a little bit of the flavor of the atmosphere that attained at the time and the tremendous emotional pressure under which the whole operation was going on. So that when any scientist would dare to question the control tests for safety that were being used, that was regarded as a malicious attitude.

Q Were there others who questioned?
A There were others.
Q There were others.
A But, apparently I carried more clout.
Q You know, I have searched the literature and outside of immunization committee meetings, John Enders says very, very little, especially in public.

As a matter of fact, he didn't attend these meetings. And when he finally was asked at a congressional hearing, he came forth. As a matter of fact, John Enders quite generally behaved like a well brought up boy. He would speak when he was spoken to, had specifically asked, and when he was specifically asked at a congressional hearing after the event, not before, I rather think that this is unfortunate. I have a great admiration for John Enders. We just had a
recent reunion. He is ten years older than I am. I think if John Enders had spoken out before the critical, tragic event, and that if he had come out with me it may have been possible to change the production procedure first by eliminating the very virulent virus and secondly by insisting on more careful control tests. Because what—what was I talking about? This was a fact. It was a fact that was only discovered when large scale production was being done, that the 5 cc test, putting in half cc in ten tissue cultures tubes and if that didn't detect virus, the people—the production people quickly were able to show that if they used instead of small tissue culture tubes, that if they would use a liter bottle with a big surface of cells, and if they would add to it ten or twenty mill of vaccine they would pick up virus with great regularity. Of course, I mean, the tests were subsequently changed, but at the time when I raised the issue, the very thought of it led the medical director of the National Foundation to call it malicious. This is a reflection.

Q There are—this brings back to the community—I know that there were many who were opposed to the use of Mahoney virus. As a matter of fact, there was one meeting which was devoted to this question of the use of Mahoney virus, and yet—a person like John Paul.
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A Excuse me. Are you referring to the meeting during the congressional hearing after the tragic event or before?

Q No, no. Before.

A Well I don't remember now. It is possible.

Q The interesting thing is people like John Paul had doubts. Dave Bodein had doubts of the use of Mahoney and yet no one...

A Is it on record?

Q It is on record. It is on record, yet nothing was ever said publicly. You are almost the only one who ever says anything.

A Well I didn't say anything publicly. I might--I said publicly later, but I think initially I didn't say these things publicly. I said these things where it would count, where it should count.

Q Yes. When I say publicly, I mean at meetings let us say of the Cincinnati Academy of Medicine, or at other meetings. But outside of the immunization committee meetings these people say nothing. John Paul--

A It is unfortunate because I will tell you very frankly there are many of my most esteemed colleagues during the course of my five decades of work in a scientific career whom I find not very critical. Or, if they hold certain concepts, they don't hold them strongly enough to come out against the strong kind, and then I think it has its virtues when let's say human life is not at stake. But when you--

Q Now, Dr. Sabin, one of the things that you are talking...
about now is about the production of the vaccine which is a
different matter than the theoretical aspect.

A No. What I am talking about is not theoretical. It
is the transition from small scale to large scale. And when
you--let me give you some specific examples, so it's not a
generalization. Small scale. Let us say you harvest a liter
of fluid that has a great deal of polio virus in it, you
harvest it in tissue culture, using the jargon, and you
centrifuge it to get rid of the cells that would be floating.
And you have a clear fluid and you go ahead and you
immediately add the formulent to the clear fluid. And you
get then a certain activation curve of the virus depending
on the temperature and the concentration of formulent.
Now let's see what happens on a large scale, when instead
of one liter you make a hundred liters. And you harvest the
different fluids, from different culture bottles and you
can't do it right away. So this is set aside to sediment
in the cold rooms. The let us say it is centrifuged. From
the centrifuge right away, and then it is set aside in the
cold rooms and perhaps two, or three or four days later when
it is all accumulated, you go ahead and add the formulent to
it. Well, as it was subsequently discovered after a good
deal of detective work, it turned out that when those
centrifuged fluids were kept in the cold room there was
some sediment of formulent. Some sediment at the bottom.
Since all the cells were already centrifuged out, what the
sediment was wasn't exactly clear, but it wasn't re-centrifuged
again. It was never filtered originally. And so the inactivation of virus when you have some sediment in material that entraps virus particles is quite different from which you get when you have a perfectly clear fluid right after you centrifuge it, and this turned out to be one of the basic problems subsequently in the tragic Cutter incident.

Q  Why didn't it happen during the tests?

A  It didn't happen as I just got through describing so I will repeat. I just got through describing that when you make a hundred milliliters or a liter and you centrifuge it clear and you have it clear, you have no sediment, you have no entrapment of virus particles. You do it on a small scale, it doesn't happen. But when you do large quantities you have set the centrifuged fluid aside and then certain aggregation happens in the cold and it changes the picture. I was trying to illustrate in answer to your question what are some of the differences in the transition from small scale to large scale.

Let me give you another specific example, of things that happen in small scale and large scale. When you do things on a small scale. When you take out one percent of the culture fluid, one percent of the thousand let's say it's ten mls, and you test for virus in that particularly under conditions in which it was inactivated and you don't find any. But now you use larger quantities, and you go
and you test larger quantities, and you will find that under conditions in which a test on five or ten cubic centimeters of culture fluid will not reveal any residual virus, that if you do a test on ten times as much, you will find it. And it is only—it makes sense if you do not doggedly insist on a hypothetical formula that Salk insisted on that the inactivation process just goes along down a straight line, and that you can predict that beyond a certain point that no matter how much you test it, there won't be any live virus left. Well this turned out to be wrong. It still is wrong.

Q Okay. Now, what about Francis has his test of the vaccine, and the vaccine is proclaimed safe and potent—

A Well this was the impression that he gave—safe and potent.

Q Yes. Now, it is—

A Potent meant that about 70 percent.

Q Now, what about that potency.

A Well, the potency meant that out of I think every hundred who received two doses at a certain interval—two or three—I forget now. It is so long ago. That 70 percent who had no demonstrable antibodies now had demonstrable antibodies. Then of course it was also known that the response to the vaccine was influenced by previous experience with natural infection with any one type of polio virus. So that if you had children with no previous exposure to polio virus infection naturally, it took much more to
produce a positive response than in somebody who already had
naturally acquired antibody Type 2, or 3 or 1, because the
smaller amount of antigen in killed vaccine was able to act
like a booster in a person who was already partly sensitized
by previous exposure. So again it was a very general way,
about 70 percent effective. But you know later on, the problem
about potency arose because after the tragic Cutter incident
when it became absolutely essential to filter the culture
fluid in order to get rid of aggregates, then the potency
was not as great as the vaccine that was used during the
field trial. Now here again it shows the aberrations that
critical thinking can lead you into. One of the arguments
that Dr. Francis used during the congressional hearing right
after the event was that how can you be sure that if you use
the same strain of virus but attenuate it, it will have the
same antigenic potency. Well, in science you don't have to
ask questions like that. You test it. You do it. You--
if you are not sure you test it and you do it. And you do
dit side by side. There is a way of measuring it. There was
a test for potency in the monkeys. It is a qualitative test.
Why did something that takes exactly one month or so to get
an answer--why should one operate something unless one is in
a terrible hurry. And nevertheless, he never thought that
the very act of filtration that was now necessary would
sufficiently alter. So that the same dosage that was used
in the field trial would not actually produce the same effect.
Well there are all kinds of problems.
Q Were there different tests for potency beside that monkey test that you--

A Well, basically. I mean there are some guinea pig tests but the standard test that was required for potency was one in the monkey, in which certain amounts were inoculated. Antibody was tested for, and you had to have a minimum, instead of a diem cit sufficiently high. It was all--there was an awful lot of collusion there which was unfortunate. Because the political pressure exerted to be perfectly frank by Mr. O'Connor the committees that he was able to set up until finally the Public Health Service rebelled. But for a time, there was tremendous pressure to push things forward.

Q It is interesting that you can get tremendous pressure on scientists not to ask scientific questions.

A Not to ask. It is to counteract. And it is not just on scientists whereas we are talking now with the Public Health Service controlled agency. It was the pressure also on the Bureau of Biologics to set the potency requirements, to set the initially before the Cutter accident the kind of safety requirements. It is in a way scandalous and I think the argument that you are serving the greater good by going in a hurry, that you will protect, that you might save the lives of some children--that doesn't hold. It was the same argument that was used in 1935, 1937 because if it turns out bad you turn off a public response which is absolutely essential for achieving optimum results.

Q Now, didn't the application of methiolate
to take care of bacterial contaminations--

A  There were all kinds of minor incidents--

Q  But that effected the potency.

A  The filtration was probably responsible more than anything else for diminishing the concentration of virus.

Q  But there can be no doubt for example, that Salk vaccine did prevent paralytic polio.

A  Well as I said in print on other occasions. There is no question at all that a large number were protected from paralytic disease. But it is also true that a large number of them had three or four doses and still developed the paralytic disease. While it is a consolation as I showed in a recent analysis, there is no question that after five years of use of Salk vaccine, the average annual incidence, and you cannot judge by a single year. The average annual incidence of paralytic polio was reduced by 70, 75 percent. There is no question about that. 25 percent is still a hell of a lot of polio. An outbreak of polio—I mean our over six thousand cases of paralytic polio in 1959, four or five years after the use of the vaccine. That is a hell of a lot of polio. Still, there is no question that many were prevented.

Q  Now, could that 25 percent have been due to the fact that people didn't get vaccinated at all?

A  Well, some didn't get vaccinated and as is known from what happened in epidemics, that about 20 to 30 percent of the paralytic cases occurred in those who had had more than
three doses of Salk vaccine. It wasn't potent enough.

Q So, part of the problem was the potency of the vaccine.

A Part of the problem was the potency, and as always, the other part of the problem is the percentage of coverage. And that even if you get 70 percent coverage, and if the vaccine, unlike live virus vaccine does not produce a break in the chain of transmission, it continues to circulate in the community, and then when it builds up, what I mean, when it builds up, I mean that when a sufficiently large number without immunity are build up in a community, and there is a virulent polio virus beginning to spread, and it spreads, and who gets it. Those that have been unvaccinated, those that have had insufficient of doses of vaccine and those who have had a sufficient number of doses of the vaccine but didn't--the vaccine wasn't potent enough to produce a response because as I pointed out before, the dose required to produce a response in the number of injections is much greater in those who are so-called triple negative, who have had no previous experience with any polio type of natural infection than it is in those who may have had the infection, natural infection with one or two types of polio virus.

Q Now, Salk made a claim of immunologic hyperactivity. Somehow or other if the vaccine was given, it would--I don't know whether it would stop or heighten the--
A Shall I explain what he said?
Q Yes.
A He said that even if the number of doses of vaccine that were given had failed to produce a demonstrable antibody response the immune mechanism would be sensitized in such a way that in the course of natural infection it would lead to a much more rapid antibody response so that the whole process would be aborted. He never gave this up. Never. It was a hypothetical thing and I was actually able to study this in volunteers to whom we had given Salk vaccine and to children in our studies later and who did not develop the antibody response or some that had some antibody response—and then feeding the attenuated strains to see how rapidly the antibody response would come, in those with previous doses of Salk vaccine and those who didn't have any. And there was no difference because the antibody response during the course of natural infection or after feeding the attenuated strains was so fast that it isn't the rapidity of the antibody response that makes the difference between a paralytic infection and an inapparent infection. But he never gave that up.
Q He still maintains it?
A Even two years in an article in Science he still parades this idea, but it isn't true.
Q Now, do you remember going before the congressional committee?
A I remember it. I remember details, but it was not
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a very pleasant thing.

Q The thing I recently reread the hearings and I found one thing absolutely intriguing. First, you were the first one asked to speak by John Paul who in a sense acted as the chairman for the group of scientists, and you were asked to do a very specific thing. You were to instruct the congressmen as to the problems of polio. This was your first job before—

A I don't remember that at all.

Q You don't remember that at all. The other thing is really the reluctance of the scientists to give a yes or no answer initially as to whether to continue the trials or to continue the vaccination program or not. And finally, Wolverton broke the question before the scientists into two groups. The first question was would you recommend substitution for Mahoney strain and most everyone said this with some qualification or not with one exception and that was Rivers. Rivers could not be budged on this. His argument was Mahoney strain was the best antigenic strain that he had and that was it.

A May I interrupt this?

Q Yes.

A Already at that time I had an attenuated variant of the Mahoney strain that as I pointed out at an earlier reply there was one ten millionth less virulent than the Mahoney virus they used to make the killed virus vaccine because I had segregated out by that time the data were
already published in full in the *Journal of Experimental Medicine*. It wasn't anything that was not available.

Mind you this is 1955. It was already published in 1954, long before, long before the field trials was even planned in 19--

ABRUPT END OF TAPE.