Q: Today, I'd like to begin by making an analysis, as it were, of the transfer from the Lister Institute to the Rockefeller Institute. I have a letter before me, which is dated July 31, 1934 which is sent to Tom Rivers and is sent from the Lister Institute. I would like to read the letter. "My dear Dr. Rivers--" (interruption)

"My Dear Dr. Rivers: The main purpose of this letter is to inquire whether there is any way in which I might work with you in the coming year. My present National Research Council Fellowship ends in December, 1934. As you undoubtedly know, the medical fellowship board does not meet this autumn, and no applications can be considered by them until the spring of 1935. I am, however, most anxious to carry on work in virus diseases, and particularly desirous of spending some time with you if it is at all possible. My experience in England has been most interesting, and my own endeavors have not been entirely fruitless. In addition to following the work of Professor Ledingham on the various aspects of elementary bodies and Hurst' works on neurotropic viruses at the Lister, I've had occasion to observe closely the work of Bedson and his group at the London Hospital and that of the workers at the National Institute. Indeed, I am hoping to be able to spend the last few months there. Much of my time at the Lister has been spent on the in vitro cultivation of vaccinia with particular reference to lifeless media. This work has been entirely unsuccessful. However, I--(interruption) However, I continued the work on the
"B" Brebner virus and have obtained rather interesting results, some of which will appear in the August number of the British Journal of Experimental Pathology. In short I found that it is serologically specific virus. And have sera which are capable of distinguishing it from any other known virus. Quantitative serological investigations revealed a partial immunological relationship between the "B" virus with pseudo rabies and herpes simplex. Work now being completed also points to a similar relationship between pseudo rabies and herpes simplex. While none was found to exist between the latter and virus III. I found no difficulty in transmitting the "B" virus to McKacka's rhesus monkeys in England. Many data taken together suggest that most of the rhesus monkeys in New York were immune, probably due to widespread infection of this virus. The "B" virus produces a very interesting disease in the monkeys. Clinically, very similar to that produced by vaccinia given by the same roots. Histopathologically, however, there are definite acidophelic intra nuclear inclusions and there is no immunological relationship whatever with vaccinia. At the present, I am engaged in investigating the mode of action of some anti virule sera, analyzing the in vitro reaction with the help of the ultra centrifuge, and some rather unexpected results have already been obtained."

May I have the last page? "It won't be long now before my time here will be up. And I am most anxious to carry on."
Dr. Sabin

Having gone as far as I have, I know of no one with whom I would rather work now than with you. If it is at all possible, I should be extremely grateful to you. Sincerely yours, Albert Sabin."

Dr. Sabin, did you always consider going back to the Rockefeller? Did you have any considerations of staying on at the Lister? or the Pasteur?

A: In the first place, my National Research Council Fellowship was for just a year. The possibility of working anywhere outside of the United States, if it had occurred at all, as a desirable approach to explore, would have been the National Institute for Medical Research in Hampstead.

The Pasteur Institute, which I visited on a holiday, subsequent to writing this letter, did not impress me as a center of serious virus research. The Lister Institute, was, to be quite frank, very strong in other areas, rather than in virus research. So, if I were to enrich myself with the methodologies, with approaches and the thinking of others, I would have chosen but one place outside of the United States, and that is the National Institute for Medical Research, where at the time, Christopher Andrewes, Smith, Laidlaw, Elford, others, were really working in the forefront of medical virology, certainly.

Whether--there was no question--that the man I knew best--and I did want to get back to the United States--was Tom Rivers at the Rockefeller Institute. Perhaps there were other sub-conscious factors that motivated me to wish to work at the Rockefeller
Institute, aside from my sincere conviction that it was one of the best places where modern, certainly medical virology was in progress.

I didn't know anyone else there in virology. Dr. Rivers had helped me before I went to England. We had many discussions. So, I naturally addressed myself to him. Besides, the only way I possibly would have been able to work at the National Institute for Medical Research, would be to get an American—another medical fellowship. A fellowship from the National Research Council, that is.

As I indicated the Board wasn't meeting until later. I was really stuck. So, that the only possibility for me would be to get a regular appointment in an institution. In England I couldn't do that. There were very strict laws against foreigners working in England at the time.

Q: It was really the result of the depression—
A: I didn't have any resources of my own. I had to earn some money, so I had to get a job. Really, I chose it, not because there was nothing else, but because it was a choice between two places: the National Institute for Medical Research at Hampstead in London—and there I couldn't get a job; and the Rockefeller Institute, where I could certainly try, and I did, and I got.

Q: It's clear that your letter to Rivers sent a flurry of correspondence, that you were unaware of. For example, Rivers
Dr. Sabin

immediately brought this to the attention of Dr. Flexner, because he himself did not have room in his laboratory. And Flexner brought it to the attention of Dr. Oletsky. And did you hear from Flexner or Oletsky? Do you remember who wrote to you subsequently?

A: Well, I do not remember and I don't know where all the correspondence is. They'd be in the files you have in Cincinnati. But I think I actually heard first from Rivers. In which, he explained why he cannot himself offer me an immediate job to begin January 1st, 1935, in which he told me about Oletsky. There was a vacancy there and possibly there might be--I suppose that when the appointment was confirmed, I heard from Dr. Oletsky. At which time, I think I wrote him a letter.

Yes, may I read this?

Q: Oh, please.

A: I find that Dr. Oletsky wrote me a very formal letter because this letter that I see here, through your courtesy, is in fact, probably the--subsequent to his writing to me first and offering me the job and my accepting. Nope. Apparently it was not.

At any rate, he said "I'm looking forward to congenial association with you and hope it will be mutually advantageous. Would you be so kind as to let me know as soon as your plans are made when to expect you here. I hope you are finding your present position enjoyable and profitable. Best wishes for your success." Now that was November 1st. Now, I must say
that I did not know Dr. Oletsky. To be very frank, as a novice in the field of microbiology, even though I had read the literature, I was not aware of all the things that he had done. But again, I am, in a sense, delighted. By the letter you brought to my attention and my reply, because it was long hand. I didn't have a copy, that I wrote him a reply to that letter on November 13.

There are not too many things of which I am especially proud in my life, that I may have written or said. But reading this letter, which I would like to read, I feel proud of myself.

Q: Good.

A: For several reasons. I say, "Dear Dr. Oletsky." This is on November 13, 1934. "Many thanks for your kind note. It brightened, to a considerable extent, an already cheerful prospect. Since philosophy has been divorced from science, scientific literature had ceased to reveal the men behind it, and I have known you only from your writings. Your letter, therefore, has raised in me great hopes for an enjoyable as well as profitable future.

"Most of the work which I had planned is practically completed, and I am sailing for New York on December 15. I intend to visit you to talk things over on December 26th or 27th and to start work as soon as possible."

Now the part that I like is the description how "current scientific writing belies little or nothing about the man behind the writing."

Q: That's absolutely true. Now there's one word in there that strikes my fancy. That is "profitable". Dr. Sabin, what
was your salary upon coming to the Institute and the rank that you had when you came.

A: I hope you didn't—that you're not interpreting the word profitable in financial terms.

Q: No, I'm using this really to make the point that it's not in financial terms.

A: Naturally, my appointment was—not the lowest rank—but the lowest rank on the scientific staff, namely an assistant. The salary was $2000, which was standard for an assistant. It did represent an increase over the $1800 which I received as a National Research Council Fellow. So I was moving upward. But, if I may comment at this point on my use of the word "profitable". I, naturally, as you anticipated, had in mind, the possibility of being able to fulfill my desires along the lines of a scientific career.

Q: Do you remember going to take up your post at the Institute? Did you first have to go to see the director, or did you immediately report to Oletsky's laboratory.

A: I frankly don't remember. Somehow, vaguely, it seems to me that I went directly to Dr. Oletsky. Actually, I also saw Dr. Rivers. Because there was a very close tie between him and me. To a considerable extent, he was my consultant and mentor prior to going to England. Because my excursion into virology occurred just a little over a year before my going to England. He was obviously the leading virologist in the United States at the time.
Q: Dr. Sabin, but Rivers in the Institute was not in the laboratories. He was in the hospital. Oletsky's laboratory was in the division of laboratories. How sharp a bifurcation was that in Institute life?

A: It seemed to me very soon after my arrival at the Rockefeller Institute, that there was a very sharp division both social and scientific.

Q: Could you tell me about that?

A: Between the hospital, which was as deeply involved in what you might call fundamental medical and scientific research and the division of laboratories. I would say that the division, if I would use something that would be comparable, at least comes to my mind by association now, succession of events, was almost like the Berlin Wall.

It seemed to me, now I may be wrong, that the hospital had put up a wall between itself and its activities and those of the so-called Division of Laboratories of the Rockefeller Institute. One had nothing to do with the other. For example, the hospital was a very closely knit community, and it was predominantly made up of physicians who were also scientists—soloed or occasional Ph.D.'s. A Ph.D. like Van Slyke, or a Ph.D. like René Dubos, were tolerated. But, as it should be, it was a place for training the clinical investigator. They lived there, many of them, particularly the doctors who were in the early stages, equivalent to my, let's say, being an assistant. They performed medical duties. They'd take care
of patients. The laboratories were right close together. The wards—-it was the breeding ground for the great clinical investigators of America. It was wonderful, really.

But they were a closely knit group, and their activities after hours, like journal clubs, or something like that, were just their own. They had their own very intimate activities and social activities, and the only time one would somehow get together would be at lunch in the main wonderful dining room near the library in the building, and in the division of laboratories, not the hospital. At Institute seminars, usually on Fridays, and as occasion might arise, on the basis of developing personal relationships. That was to me a very striking phenomenon, which this subdivision was, incidentally, if I may develop this point--

Q: Oh, please--

A: The decisive factor in my leaving the Institute in 1939. Because I had my serendipity become involved in research. It had to do with arthritis and rheumatic fever, in animal models that is. The division between the institute laboratories where I worked and the hospital were such that even though Dr. Swift and his young associates quickly looking over my shoulder also became involved in this and made some terrible errors in an early publication, because they didn't consult or exchange data with me.

My desire to pursue this to some sort of definitive end point when I say pursue this particular experimental approach of the possible role of the so-called pleuropneumonia like organisms in human rheumatoid arthritis and in human rheumatic
fever was blocked because I didn't have access to the hospital. I couldn't work in the hospital. Although I was offered a wonderful promotion to association membership, my own laboratory, and everything—in the Division of Laboratories—not in the hospital. I nevertheless, decided to leave because I wanted to be—I was also a physician. In studying rheumatic fever and rheumatoid arthritis I wanted to have access to the patients. That was denied to me, and that's why I left.

Q: Now, this is very—you had the M.D. Now, was there a pecking order in the division of laboratories. Were certain laboratories more favored than others?

A: Well, when you use the word "favored" I obviously have to ask the question, "By whom"?

Q: Favored by the director.

A: I would look upon—of course I have no way of objecting that. But, by osmosis, when you live in a place, as I did for five years. It didn't take long to get it. You get a feeling of pecking order. I think it's not an unreasonable expression to use. I'll say that generally speaking, there was a certain—the pecking order was first of all, the people working in the hospital at the Institute. Then, within the Institute itself, there was no question of there being a pecking order in the way—by pecking order means the way you submit somebody's dominance.

Q: Yes.

A: There were, not only within. Or, not only, let's say, among different disciplines, like chemistry, and biophysics
and various other activities, but within the Department of Pathology and Bacteriology, as I think it was called then. The Division of--not Department. The Division of Pathology and Bacteriology. There was also a pecking order. Some were lower and some were higher. I'd say to some extent, it was based on the value of the--it was a justified pecking order--on the value of the scientific contributions. Because it was quite obvious that the contributions of some were significantly greater than the contributions of others. As in every great Institution, there are some who are very pedestrian and some who are much more outstanding.

It is perhaps to the credit of the Rockefeller Institute in that ear, 1935, and that's exactly 40 years ago. As we speak of now, it's exactly 40 years since I--one month--at this time 40 years ago, I was already one month at the Rockefeller Institute.

But at that time, I would say the number of mediocre and pedestrian workers at the Rockefeller Institute was small--perhaps much smaller than it is now, probably. Because they were choosing and picking very very carefully and they just allowed those that they really didn't want to keep, to leave, because at that time, there was a tremendous growth of medical schools in the United States, and they needed scientifically trained faculty very badly. So, the Rockefeller Institute was being raided all the time. If the director didn't particularly think too much--the director and the head of the department--of the person,
however, were fair, he might be—they would encourage him to accept invitations. If they asked you to stay, then you knew that they wanted you. That is why it was my gratification, that when I was—had several offers in 1939—and I went to see the director.

Q: This was Dr. Gasser?

A: Yes, it was Dr. Gasser then. He tried to get me to "Now, look," he says "We'll promote you to an associate member, and that's practical insurance that you will become a member, and we'll give you everything you want. Of course, the salary we are offering you is less than the others, but you have to decide now and to stay on here, and all the things that are on here, we'll see that you get."

And I told him, "The one thing I want, I know I cannot get, and I am making my decision on that basis. I must have open access to clinical material." And the place I was offered—at the Children's Hospital Research Foundation at the University of Cincinnati—"I have my own wards, I can see my own patients, I can carry out clinical investigation, investigation in the laboratory. That I cannot do here. And about this you apparently cannot do very much." And that is why I am choosing to go to Cincinnati."

Q: Now, taking this point of division, what instrumentalities were there within the Rockefeller for cooperation? What brought people together?
A: Okay. There is a difference, if I may--

Q: Yes. Instead of cooperation--it is a poor word.

A: Between the real meaning of the word cooperation and cross fertilization. I'm not. If I interpret your thoughts correctly, you are speaking more of cross fertilization.

Q: Cross fertilization.

A: I would say the most important opportunity for cross fertilization in the Division of Laboratories was the lunch room. It was a tradition. You didn't bring your lunch in a bag. You went down at a certain time, and you had lunch. Of course it was Dr. Flexner's original intention to use that lunch room, which was, I understand, endowed in a certain way. You didn't pay the actual cost. To use that lunch room as a means for cross fertilization. But he was defeated in one special respect. Very soon, there developed as part of this pecking order, at certain tables. Certain people representing certain departments whether the hospital or the other part of the Institute, would have certain tables. They were almost invariably the same people would come and sit at that table every day.

Now, as a stranger and a newcomer, I wanted to live up to Dr. Flexner's concept of the lunch room. I remember going to sit at different tables. You know, there were times when nobody talked to me at that table. There were times when I want somehow or other, to enter into the discussion but I didn't because--it was quite a number of years.
But this I felt immediately was a bad thing. Nevertheless, I did cross. I did not choose to sit at the same table every day, although that was a terrible tradition. So, I'd go and sit at Landsteiner's table. I'd sit at Avery's table. I'd sit at Rivers' table. I did move around. To me, ultimately, first they didn't talk to me. I was a "good new boy" and I didn't talk out of turn, but little by little, you see I became involved. And those were some of my most interesting experiences. Because there was unquestionably an extraordinary group of men at the Rockefeller Institute at the time.

To this young boy that I was at the time, it was an extraordinarily stimulating experience. It was a great influence, and there also developed interesting, let's say, influences on my work, on the things that I did. That was one.

Q: Can you illustrate any of those influences as you remember them now?

A: Certainly--(sentence incomplete). I cannot say exactly which thing that I did. The discussion with Rivers and his group, the work that they were at the time concentrating very profound study of the nature of vaccinia virus. The nature of the virus. The individual virus particles. The immune responses to it. They influenced my thinking, and the experiments that I designed. Although that was their property. I never invaded anybody else's field.

Q: Even though you had worked on vaccinia--
A: Yeah, I know. I had worked on vaccinia. I used it as a tool. But I had other things to pursue. Certainly the contact with Wendell Stanley, who worked miles away in Princeton. But he used to come down for lunch, and we developed an early association at that time which was maintained until his death a few years ago. We remained friends, and during World War II, I came down to work in the Princeton laboratories because they were so close to Trenton State Prison where I worked.

Wendell Stanley and I immediately became involved as co-workers. On dengue and some of the things that we saw with electromicrostrophy (?) which Wendell Stanley had developed already at that time, very sophisticated fashion. I realize now that we saw forms of hepatitis B antigen there, and not the dengue virus. But at any rate, it became the basis for friendships.

Shope also was a wonderful person who worked at Princeton, but he used to come down occasionally to the seminars and otherwise. He was also a remarkable person who I am sure—whose specific approaches to problems influenced my thinking.

Q: Now were there other instrumentalities, other than the lunchroom where you could--

A: Incidentally, before I leave the lunchroom I want to say that there was an extraordinary good group at the time that was working out of the Rockefeller Foundation Laboratories. Another separate unit. They were working in part on yellow fever. There was Max Thieler. And then, of course, Tommy Francis began to work on influenza. And a very intimate relationship developed
for example, between Max Thieler and myself, and the group that
was working on membranes, the so-called Elford membranes, which
then I used considerably. Also, my very good association with
Max Thieler influenced my later work on _______ and so on.

Q: I don't want you to go into that now.

A: Yes, but I want to say that all of these associations
actually, originally, were all in the lunch room at the Institute.
Although I don't want to leave the impression that I was frozen
out constantly, I merely want to leave the impression of the
newcomer. He comes; he is frozen out. He sits down at somebody's
table and instead of being greeted like a new member of the
family, nobody talks to him. But if you behave, like a good boy,
as you do in a fraternity, when you are being raised, and you
don't make a nuisance of yourself, gradually you become accepted.

Aside from the lunch room, the other cross fertilization
opportunity is really in the laboratory division, was mostly at
seminars that occurred on Friday afternoons for everybody in the
Institute used to go to. Then of course, if you needed advice,
or if you needed certain things, I remember going up to see P. A.
Levine, you see, about something. Or my namesake, Florence Sabin,
I went to consult her in connection with certain things. Or, I
would go to, I remember, now, what's the name of the surgeon who
got the Nobel Prize, who was working--

Q: The what?

A: The surgeon.

Q: Carrel?
A: Alexis Carrel. Now why did I forget his name? Alexis Carrel had some very fancy instruments that I learned about in my studies of antigen-antibody reactions. I went to use those instruments. And Lindberg was working at that time with the machine from which the heart-lung machine came later, but it kept thyroid alive.

I mean, there were these wonderful personal relationships. What was lacking, perhaps, that the hospital had, were these journal buffs. The laboratory division was much larger than the hospital. The numbers were so large, perhaps, that the intimacy that could develop over in the hospital was not as readily developed. But on a departmental basis, there were no departmental journal clubs like in the division of pathology and bacteriology. As a matter of fact, I had a feeling that there was a certain jealousy--you didn't want the other division heads--not department heads--to know what you were doing. There were things of that sort.

Q: Did the animal room play any function?

A: The animal room played a function to the extent that--rather nice traditions at the Rockefeller Institute that I always admired--was that first thing in the morning, that you did was to make rounds in the animal quarters, on your particular experimental animals. Everybody kept their animals in the same place, rather in the same building. So, very often, you'd encounter people when you'd make your rounds. The hospital kept its animals there, too.
You'd be called—"Come in, you've got to see this."

"Look at this really extraordinary thing, or you come in," you see. So, you might call the animal quarters the hospital in quotes. In which, people would meet. Well, I think, basically, I've covered the various opportunities for cross fertilization and the possibility of calling on other disciplines when you visualize the need.

Q: This is fine. Now, what was Oletsky's laboratory like when you first came? What kinds of problems --?

A: When I arrived in Dr. Oletsky's laboratory, in January 2nd, 1935, there was only one other person working with him, and that was Dr. Harold Cox. Dr. Syverton, Jerome Syverton, had just left and the work in progress at the time was concerned with the attempts to immunize mice, guinea pigs, with inactivated equine encephalomyelitis virus, there was also work on, if I remember correctly--can we hold off for a moment.

Dr. Oletsky and Dr. Cox were engaged in work on active immunization. The basic interest, of course, was to find principles that might be applicable to immunization against poliomyelitis. And certain model systems in mice and guinea pigs, were used with viruses like equine encephalomyelitis in order to find something that would be relevant to poliomyelitis.

Another very interesting approach that was being pursued was one that is reported by Oletsky and Cox at the end of 1934 in which preliminary treatment of the nasal neuocosa with tannic
acid and with alum was able to protect mice against intra nasal inoculation of equine encephalomyelitis virus. Now, this was also regarded as an important approach because it should be recalled now that that was the period, when, in keeping with Dr. Simon Flexner's conviction—and that, I must say, of practically all of us, not all of us working in the field of poliomyelitis at the time—that the nasal root was the root of invasion in human beings and therefore, if you could in any way prevent the virus from entering the nervous system from the portal of entry, that would be one approach for control.

When I arrived, I was made aware of some of the studies that also were being made, that had been made, on the particularly neurotropic activities of the viruses causing vesicular stomatitis in cattle. Dr. Oletsky and his associations previously had published reports on neurovirulence for mice, on the behavior the the vesicular stomatitis viruses, especially, which were never known to produce encephalitis in cattle, the natural host for these viruses; nevertheless, showing how they produced encephalitis regularly in mice and guinea pigs and then also some observations of the different behavior of young mice and older mice when innoculated with these viruses by peripheral routes.

The observation, that while after innoculation by intra-cerebral route, the young and old mice were reacted with regularity in developing encephalitis, that when the virus, if I recall correctly, was given by intraabdominally or what is correctly called
intra peritoneal route then, that there was great irregularity in the way younger and older mice came down. I had my thoughts on polio from the time that I began to work on in 1931. One of the mysteries always was, why was polio predominantly a disease of the very young, was called infantile paralysis. At that time, still, why there were relatively so few adults who were getting it.

That intrigued me very much, and I wondered whether there might be some basic law there that would be of interest in explaining why a disease like polio was predominantly a disease of the young. So, I picked up where Dr. Oletsky and his former associate Syverton, Cox, had left off because they merely recorded the observation that there was a difference in peripheral and intra cerebral inoculations, as regards to the results that followed. I began to study it systematically. This became a very interesting field of studies because it revealed remarkable new phenomena, about the spread of viruses from the muscle, from the nose, into the nervous system, about the spread of the viruses in the nervous system itself.

I was interested always in working out very precisely the parameters of what was involved. Not only precise age groups, when resistance would appear, the quantitative aspects, and what were the mechanisms, so that I devised all sorts of experiments to elucidate this. Well, the results of that are published in a number of publications now. Actually, I think, my first prize in science, the Theobald Smith Award of the American Association for the Advancement of Science, was based on these studies which
I came to call "constitutional barriers" to the spread of certain neurotropic viruses. Which, I was able to show that different viruses behave differently in the same host, and that it is not a question at all of the host but it also was a very close—it depended on host virus inter relationships. Of considerable interest, of course, is that ultimately, the motive that led me to carry out these studies, mainly to determine whether this had within it an explanation of _____________ of poliomyelitis turned out to be—that it had nothing to do with it. These factors didn't play a part in the _____________ of polio at all.

Q: But here is the first piece of work that you do; it relates to the question—

A: The first piece of work—let me modify this—that was not an extension of what I brought back with me from the Lister Institute. Because is where I carried on—. But the first piece of work which was directly influenced, let me say, by what had gone on in Dr. Oletsky's laboratory before I came.

Q: And this was the age of the susceptible hosts and the neuroinvasiveness of _____________ of different viruses. And it's fair to say that one of your conclusions was that the physiological process affecting certain—was a physiological process affecting certain tissues and resistance can develop in animals without relationship to previous exposure or antibodies. In other words, it was the physiological process—
A: Without previous exposure of existing antibodies, as a result of cross immunity. Of course, ultimately, I showed what that process was.

Q: Yes, we'll get into that later. But in a sense, this is the new work you begin.

A: Resistance without relationship to previous infection or specific immunity. That's what it was. That's basical principles.

Q: Yes. And in a sense, you really now devote yourself to immunological problems.

A: This was only one because, also, under the influence of Dr. Flexner's continuing interest and of course, the interest of Dr. Oletsky and polio, and also because what was going on outside the Institute at the time—you may remember, that Brodie vaccine, the Kolmer vaccine—it was of interest to study the relative role of antibody in relation to resistance—antibody and previous infection.

I might say that almost this is one of the steps in developing it. Rightly or wrongly, that led to the concept that infection did something more than produce antibody. That resistance, specific resistance, coming let's say, from vaccination or only antibody might result, or something different from what was left behind as a result of infection. And you might say, that this is the devious path to the development of a live vaccine against polio.
Q: This is interesting because, in a sense, Oletsky's laboratory is asked, or directed, by Dr. Flexner to test out the validity of two vaccines which were presented at that time: Park &. Brodie, and the Kolmer vaccine. Now, you knew both Park and Brodie. Did you--how did you feel about making this kind of--

A: Of course, I owe everything to Dr. William H. Park. He gave me the opportunity in 1926 to get started. And I am quite sure that Park never himself called it the Park and Brodie vaccine. Dr. Brodie, who was a Canadian, a very active worker, came to work in New York City Public Health laboratory and he then, using monkey nervous system and in four months, developed a vaccine. Undoubtedly because of his great respect and admiration for Dr. Park, who was a lovable person--he was interested--I am sure he greatly encouraged him to work on active immunization.

It was probably Brodie who called it the Park and Brodie vaccine. Because Park, when I knew him in 1926 didn't do any work himself. He was the director, the professor--but he wasn't in a lab. He didn't claim it to be. So we'll call it the Brodie vaccine to separate what happened.

The record speaks for itself. It was a premature attempt to immunize with material that wasn't and perhaps at the time, couldn't be properly evaluated. Undoubtedly, some of the consequences of premature testing in humans, were due to incomplete inactivation of very virulent virus and so, some persons, in the judgement of those who studied it at the time, developed paralytic polio as a result of getting the vaccine.
The development process that was involved in this obviously hinged partly on measuring the appearance of antibodies in monkeys that received such a vaccine. If I remember correctly, I think that Dr. Oletsky was asked by Dr. Flexner to determine whether a vaccine inactivated this way produced antibodies in monkeys.

In trying to refresh my memory on this, because it's so long ago—I find in my collected reprints a discussion at a meeting of the American Association of Pathologists in 1936. This was a discussion of a paper that was presented by Dr. Kolmer. I think that some of this also has some bearing on the work of Dr. Brodie. So maybe I can cover the work of the two at the same time.

I said that "Dr. Kolmer's conclusions, if I understand them correctly, are 1) that the resistance of monkeys to poliomyelitis bears a definite to the content of anti virule body in their serum. 2) that the antibody content is greater in the serum of convalescent monkeys than in that of vaccinated monkeys and 3) that the failure of Oletsky and Cox to induce an active resistance to infection with poliomyelitis in most monkeys, treated with rincinoleated vaccine" This was a method of inactivation of attenuation that Kolmer had said that he had used. "may be attributed to the fact that insufficient virus was administered. I regret to say that further studies on the same subject by Dr. Oletsky and myself have led us to adopt an essentially opposite conclusion."
Now I can see that one piece of work that I did in the laboratory was a direct outcome of the controversies at the time. That I had to check something and it was in line with my studies in the mechanisms of immunity that I had done before while I was at the Lister. So, I said then that Oletsky and Cox are not alone in maintaining that vaccinated monkeys may frequently exhibit appreciable antibodies in the blood without being resistant to infection by way of the nose. This is the point apparently here. In digressing from what my discussion was. That Oletsky and Cox, because of what was in the air at the time, were undoubtedly interested and Simon Flexner was interested, to determine whether you could vaccinate a monkey the way Brodie let's say, inactivated his virus and then tried different procedures and find out whether there was any resistance—not by innoculating small amounts in the brain, but by giving the virus in the nose. Because, again, it was the conception at the time that that's the way the virus goes in, and if you vaccinate a monkey that resists it, well, you've got something. And if it doesn't resist it, then where are you?

Oletsky and Cox found no resistance after vaccinating monkeys by the way Brodie did it, and by the way Kolmer did it. I think they also tried some tannic acid treated material. Following through on their models of work with equine encephalomyelitis in guinea pigs. I continued my discussion to say that Schultz and Gebhart, Aycock and Hudson, and co-workers have all obtained similar results, using varying amounts of virus—both larger and
smaller than that used by Dr. Kolmer. See, that was very much in the air at the time. In a recent investigation, Dr. Oletsky and I undertook to determine in what way vaccinated and convalescent monkeys differed in resistance to infection by the nasal route and in the content of antibodies in their blood. Convalescent monkeys were found to be resistant to re-infection with the same strain of virus when tested at intervals of three weeks to three months after onset of paralysis. Antibody formation in them was very much slower, however.

In none of the convalescent monkeys, one month after onset of the paralysis was there demonstrable antibody in the blood. Some of them showed antibodies for the first time in two months, while others not until three months. I say at this point, and I am digressing from my discussion, with the knowledge that I gained subsequently—that this was unquestionably due to the fact that the so-called MV virus was used—which strictly was a neurotropic virus. And this delayed response was due to the fact that the virus multiplied only in nervous tissue.

With other strains that I studied subsequently, others have studied subsequently, we find an antibody response very rapid. So, you see, this was the role of strain that was not appreciated at the time. Nevertheless, it was, and I still am with my discussion—it was a wonderful opportunity to study, separately, antibodies and resistance to infection.
I'll continue now with my discussion at the time. We thus had convalescent monkeys, without demonstrable antibody, which were resistant to infection and vaccinated monkeys, with antibody, which were not resistant. Obviously here also, I am speaking on the side, because again we are dealing with a strictly neurotropic virus. The virus was given by way of the nose. It could very quickly escape the antibody in the nose, because it probably wasn't even there, directly invade the olfactory bulbs and this is another example of how certain model systems, if you don't know really what occurs in nature, are useless and misleading.

At any rate, the issue here is antibody base—the basic mechanism. Titrations on the serums of convalescent monkeys, which finally developed antibody indicated that quantitatively there was no appreciable difference between them and serums of non-resistant vaccinated monkeys. So, that again is a digression. You had vaccinated monkeys that had a certain level of antibody and they didn't resist intra nasal innoculation. You had convalescent monkeys that either had the same amount of antibody or none demonstrable, and they did resist. Our confusion for the present, therefore, is that the relation between antibodies in the blood and the resistance in monkeys to infection with poliomyelitis is obscure. If one actually exists. And that convalescence, and here is a twofold thing. I say this as an aside. That convalescents possess some mechanism of resistance, which many vaccinated monkeys apparently lacked. This is really
the prologue to findings that I made later on with live attenuated virus and finding that certain persons, and Mrs. Sabin was one of them, incidentally, in whom the attenuated virus multiplied perfectly in the intestinal tract, produced an excellent resistance to infection, to re-infection, but no antibodies.

So, this was very much a point at issue, and then in replying, Dr. Kolmer brought out the point that he never was able to get an effective vaccine unless there was residual infectivity in his vaccine. He said, for example, "We never use a vaccine unless this preliminary test--the preliminary test is to innoculate .3 of a cc. intra cerebrally, as has been our custom." He says, "We never use a vaccine unless this preliminary test shows the presence of sufficient active virus to produce paralysis within 40 days after innoculation. His vaccines without this amount of virus failed to innoculate monkeys." And then, just to show the framework just scares me to death--within which people operated at that time. There was nothing to stop Dr. Kolmer from going out and giving such a vaccine to many people in whom--who then developed paralytic polio.

You see, this was the concept that there was something to restrain this man from going out and innoculating people with a vaccine on this basis was just absolutely incredible. He was operating on the Pasteurian principle, that if rabies virus is depressed in its infectivity to a certain point, as measured by putting in the brain, of course, if you gave it by peripheral route, it wouldn't do anything. Assumptions without tests--
Assumptions based on inadequate comparisons, and this is really an example of what ultimately led to the greater control of human experimentation by medical scientists.

Q: You look at these human experiments. You know there is only one person who really suffers in all of this, and this is Brodie. Kolmer is promoted to full professor. Park is retired, and Brodie is driven to the Provident Hospital in Detroit.

A: What conclusion do you draw from this, and then I'd like to comment.

Q: The conclusion that I draw from this, is that the younger worker who has absolutely no clout or position, and who is moreover Jewish, bares the burden of this terrible mistake.

A: I hope you will excuse if I will disagree on every account.

Q: Number one, Brodie behaved as an irresponsible investigator. The fact that Park and Kolmer did not suffer has nothing to do with the rightful fate of Brodie. His fate had nothing to do with his being Jewish. He did bad experiments. He went out into the field. Sure, he should have been restrained by Park. Park is guilty for not having restrained him. This is again where the desire to be first on the scene, and particularly when there are two competing vaccines in the field: Brodie and Kolmer, with nobody sitting over them. I think the fact that Kolmer and Park, who already had a great deal of prestige in the bank, were not made to suffer, although I am sure they must have suffered by
themselves, has nothing to do with the justifiable fate of Brodie. The fact that he did get a job in a hospital in Detroit later on I think was perhaps an opportunity for him to carry on, but he couldn't do it. I don't want to go into the details now. It appears to me, he committed suicide. He ended his life. I surely don't think that the fact of his being Jewish had anything whatsoever to do with it.

The fact that I am Jewish has never interfered with my activity in medical science.

Q: That may be so, but I regard Brodie as a scapegoat in this--
A: No. I do not.
Q: My own feeling is that the one person who should have been punished in all this was Paul de Kreuf.
A: Why?
Q: Because Paul de Kreuf was pushing both Park and Brodie—not Kolmer—but both Park and Brodie for results.
A: That I was never aware of.
Q: I have--
A: If I may interject here. Then Paul de Kreuf was guilty. Park was guilty and Brodie was guilty. Now, you say Park didn't suffer. I knew Park for many years—a very sensitive person. He stands behind his people. And I am sure that he—when he became aware that his lack of proper restraint of Brodie was responsible for the unfortunate paralytic consequences in a certain number of persons who received the vaccine, he suffered
so much that firing him or anything like that would have meant nothing. He was advanced in age, and had made so many great contributions that that was not a factor. I didn't know personally Kolmer well enough to know whether he himself suffered or not, although he seemed to have held out to the last that the vaccine was not responsible.

Q: I have to change the tape
Q: ... suffered, and the point I am trying to make is only one was stripped of position, and this was the weakest one. I don't know how one guides responsibility. Certainly, everyone in the area of virus research bore some responsibility. Flexner bore some responsibility for not stopping this. The Public Health Service bore a responsibility, only speaking after the fact.

A: May I--

Q: Please argue with me.

A: No, may I recall to you that we are talking forty years later about a situation that does not obtain now—in other words, a public responsibility for individual medical investigators. The era, that period, was an era in which no one considered himself his brother's keeper. That was a bad era. I think the consequences of both the Brodie and Kolmer's vaccines—although it was much worse with Kolmer, because Kolmer really gave stuff that he knew was infectious. He only hoped that it wouldn't be so infectious. It was much worse with Kolmer than with Brodie, that they did not have sufficient concern themselves not to proceed as they did. Furthermore, there was no mechanism by which they could be stopped. These experiences, I think led to subsequent mechanisms.

That their motives were the highest is just another example for the hackneyed expression "The road to hell is paved with good intentions". That is why I am one of those people in the present era who holds out very strongly for the need of very specific codes for the conduct of human tests or human experiments. For the need to have other people and not just your buddies in your
institutions pass on the need for certain experiments. Because the problem we're discussing of forty years ago, still continues. Individual investigators despite their--the best motivations aren't always the best judges. Furthermore, tests like laws are made, for the one percent or one-tenth percent of those who cannot be trusted to use good judgement or be decent and honest. So, in science also, it's not a question of making regulations for the 99.9 percent of scientists who would on their own proceed with great caution. It is rather for those who would not. That is why I am in complete agreement with all the work which people are calling "barriers to the progress of medical science" I do not regard them as barriers. I regard them as absolute necessities in a society in which the scientist is not above the law and is not above critique by his--not only by his own colleagues--but by the other members of society.

Q: Did you ever have occasion to speak with Brodie afterward?
A: Yes. We were very good friends. We were very close friends until the end. He felt the pain very very deeply. It was too late. Now, I'm rather shocked to hear that you think both Dr. Park and Dr. Brodie were pushed by Paul de Kreuf. Certainly, a man of the highest integrity. It is merely another reflection of the times when people lose a certain balance of judgement.

Q: I think it was more than a loss of balance of judgement. I really do not believe that Paul de Kreuf understood virus research, or understood viruses or the special problems of viruses. He'd grown up in an age of bacteriology. He'd grown up in an age of heroes--the heroes that he celebrated--Pasteur, Koch. He
literally waited for a eureka in the laboratory.

A: I think, let's also look back to the times. 1935. Three years after the election of Franklin Delano Roosevelt, a very close friend of Paul de Kreuf. Paul de Kreuf, as I understand, working very closely with FDR, trying to utilize the fact that he has risen to the top of his country. The top position of his country could help promote scientific research in poliomyelitis. He was already, if I am not mistaken correctly, in 1935, in 1936, getting ready for the establishment for the National Foundation for Infantile Paralysis.

He was a promoter. He was looking for results. We've gotta be very careful of promoters, particularly when they have the personal bias. That is why it is important to submit them to judgement of others. Mistakes can be made that way sometimes, but the benefits far outweigh control stakes.

Q: But it's clear--is there anything in Brodie's work that merits remembrance now?

A: It's been so long ago. He started out making fairly pedestrian observations. Then, he jumped into the field with an attempt to get a quick vaccine. I don't know many motivations. But I would say that the negative results were perhaps an important stepping stone. But even so, they didn't serve as well. We still had the Cutter incident. But when the Salk vaccine first went, and certainly Salk had done much more preliminary work on monkeys than Brodie had ever done. Nevertheless, what one observes on
a hundred monkeys is no more, no less than just that. When you go out to inject something into 50,000 human beings, things can occur that you do not foresee. The need for going slowly, instead of rushing, then, led to the unfortunate Cutter incident, in which I don't remember the exact number, but it was well over 100 or so, who were paralyzed as a result of incomplete inactivation of the vaccine. It was not the principle of the thing, nor the procedure. It was more improper safeguards in the procedure, and too quick a rush to do it.

Only a week ago, when I was in Palm Beach, I was being interviewed by somebody from TV, and he reminded me and showed me this atrophied leg, that he was told--he was a baby then--that he was one of the victims of the Cutter incident.

Q: Two years later, you were asked to reinvestigate another claim that had been made. This was a claim of Jungeblut that somehow or other vitamin C—that gorgeous vitamin that we celebrate today—somehow had an effect in preventing polio. Could you tell me how you got into that work and what were the results of that investigation?

A: Just to shed light on the way I was working at the time on polio, I was using model systems to lead me towards polio. The study of how vesicular stomatitis and other viruses invaded the nervous system was merely a way to lead me to find out how polio virus could be shown to invade a nervous system and ultimately led to the studies on man. So that I did not choose to try to repeat every claim that was made.
In 1937, I believe, there was the first publication by Professor Jungeblut of Columbia University, that when he inoculated monkeys with a strain of virus that he had that was not the mixed virus used by us at the Rockefeller Institute. We only used the Flexner famous MV strain that had been passed for twenty years in monkeys. That he found, and then when he gave vitamin C to these monkeys in certain doses, and incidentally, he insisted that it had to be naturally occurring, natural vitamin C and not synthetic vitamin C, that approximately 31% of vitamin C treated monkeys resisted infection as compared to only 5%--this is out of large numbers--there were 243 monkeys that he treated with vitamin C and 136 served as control, so there was a difference of 31 versus 5%, which appeared significant enough.

For some reason, when he gave synthetic vitamin C, he had no significant effect--this is also in large numbers. Only 11 out 101 monkeys that got synthetic vitamin C, the same dose, failed to develop paralysis. Well, when Simon Flexner, who obviously must have seen that paper before it was published in the *Journal of Experimental Medicine*, the organ for the Rockefeller Institute, saw that, he came rushing into the lab, I remember, and at that time I was already working on polio very carefully. I was finding out the best way to infect by way of the nose, and the passage of the virus by way of the olfactory bulbs to find the pathway--and he said "This is a wonderful thing if this is really true!" Alright, only 31% survived, but this is after all, he put it directly in the brain. How about if he would put it in the nose, then maybe he could get all of them protected.
Why don't I do that? I said "I've had previous experience with Professor Jungeblut when Dr. Park urged me to do something. I prefer not to get involved. "No," he says, "It's so important." So, if I recall things correctly now, the way it really was, was that I got in touch with Dr. Jungeblut because we were very good friends after that episode in 1931, we published a retraction together, of his original claims, and I asked him about intra nasal. He said, "Well, the trouble is, my strain, the Aycock strain, doesn't produce very much polio by way of the nose." So I said, "why don't I give you the specially passaged and standardized material that I use now which produces paralytic polio in practically every monkey that I give it to by way of the nose by the technique that I know of.

So, he said, "Give me some of that virus," and apparently, I sent him this special preparation of the MV virus, and he gave it intra nasally, and he had no results. Only one of 25 monkeys that was treated with the same amounts of vitamin C, natural vitamin C, that were affected by intra cerebral, had absolutely no effects against the intra nasally administered. But, he said "When I used a larger amount of vitamin C, instead of 5 to 25 milligrams daily, that among ten monkeys, which received 50 to 100 milligrams" see, that's ten times to 44 times to ten times as much, that "nine out of ten were protected as compared to 15 out of 50 that developed" just as I told him, and in his hands, it also produced paralysis the same way. Then he published
then. So here again, we have the situation which 90%, not 31% as before. If you only increase the dose of vitamin C. I'm telling you in this detail, because it has an interesting historic aspect. That 90%, when Simon Flexner saw that, this second publication appeared, let me see, I think it appeared the same year, and also the Journal of Experimental Medicine, the very next volume, and the same year.

Flexner, I remember, coming into the lab, and not speaking to me, not following channels through Peter Oletsky, but of course Peter Oletsky was there, because I was doing all the work on intra nasal, carefully following it through, and getting these regular results, which could be reproduced. He said, "You've got to do it. Either the man is right, then you can give huge doses of vitamin C like that, you can protect 90% of the monkeys, or 100%, get it with controls." "Or else you've got to find out what's the matter." Well, this was a direction from the boss.

Besides, I couldn't resist it. I remember going down to Dr. Jungeblut's laboratory, finding out just what he was doing, etc. By that time, he also informed me, by personal communication, that subsequently, he discovered that synthetic vitamin C would also work, that it wasn't some by-product, that if you used larger amounts of synthetic, you could get the same effect as with the natural.

I went to work and did a series of 36 monkeys in the first test. These monkeys were on the usual, normal, vitamin C adequate diet, just the same as he was doing. I gave them the large doses
of vitamin C, in different groups; and there was absolutely no effect at all. Instead of finding 90% protected as he found, I found nothing. So, I went back to the laboratory. I went back to him. Let's see. I'm checking the reproduction. I don't trust my memory.

I find that I wrote this in my report. "With these data at hand, Dr. Jungeblut's advice was sought. And a similar experiment was carried out jointly in his laboratory. In a group of 40 monkeys" Mind you, we weren't using the small numbers that Flexner was accustomed to using. "In a group of 40 monkeys, of which 10 were controls, and 30 were treated with varying amounts of vitamin C, only one monkey, a treated one, escaped paralysis.

The way this was done--was, that I brought the virus to Dr. Jungeblut's laboratory. The 40 monkeys were lined up. I gave them all--I myself gave them the virus intra nasally. I said, "You can treat them now with anything you like. I'll leave. I'll just come around to examine the monkeys." And nothing happened.

I didn't stop there. I said, "Why? Is it possible that the preliminary diet of the monkeys if there--maybe, let's say--maybe the vitamin C works only when the tissues are, have been changed by an inadequate amount of previous vitamin C and then when they're changing over, when you give them a big dose, they (sentence incomplete). So then I did a very careful experiment and ordered some monkeys, from one dealer, and let's me see. Let me refer back again. Ah, here we are.
On December 28, 1937, you see, this was done very fast. Things were published very fast, particularly things that Simon Flexner liked. The two publications appeared in 1937 and we had already done those tests. But then in December 28, 1937, I got a new shipment of monkeys. I made sure. I got 46 monkeys—a single shipment. They were selected for their excellent physical condition and nutrition. Special arrangements were made that during their voyage as well as in the establishment of the dealer, their diet would include oranges and other fresh fruits. So, they would be perfectly—

Q: They wouldn't have scurvy.

A: They wouldn't have low vitamin C. Then, I divided these monkeys up into two groups. One I put on a vitamin C deficient diet. The other one on the normal diet, and tested it again. And again, there was no effect from vitamin C at all. Whether the tissues had previously been depleted or not. The only thing that I learned from this is that monkeys, like guinea pigs, that are deprived of vitamin C, really deprived, have a tremendously high susceptibility to respiratory and enteric infections. I lost 70% of the monkeys during the period when I was holding them during deprivation of—

Q: Did any of them develop scurvy?

A: Subsequently. During the three weeks when I lost, I don't know, 70% of 21 monkeys—"70% of 25 monkeys on the scobutic diet I lost between the 9th and the 15th day." And then, when I was losing no more—and at that time, they had no signs of scurvy.
they were just coming down with respiratory infections. In the control group 21 monkeys on just a normal, adequate diet, there weren't any during that period. They were kept in the same place. They had the same experience. But, when I then inoculated the monkeys at three weeks with polio virus and continued to keep the controls on the scurbutic diet, untreated. Those developed real, typical, outright scurvy. Remarkable classical scurvy with the bones coming apart, and the hemorrhages and so on.

Well, what was the explanation. I offered Dr. Jungeblut to suggest any other experiments. We couldn't use his strain for intra nasal inoculation because it didn't produce enough polio. So there was nothing else to do. We did the experiment with the one strain with which we could infect regularly intra nasally. I offered Dr. Jungeblut to be a co-author on this. But he felt so upset that this time, he did not accept the invitation. He never published anything else on it. I think he tried. He couldn't confirm his own data. I'd rather not say here what he unfortunately discovered to be the explanation. But at any rate, this was an unhappy experience of being put on somebody's tail. But it had to be done. And Simon Flexner was right. One of the problems that we have in medical research is that somebody makes a wonderful, if true, report. You see, it's something that's happened, and nobody wants to waste the time and energy for repeating it, because they say, alright, suppose I repeat it and find that he's right. The credit will all go to Jungeblut or to Mr. Smith or Jones. But supposing that I find that he's wrong. What have I done? I've
shown that so and so--why should I bother? And yet, you see, controversies of this sort--and other things, and I could document them very well, there's a number of other things--can go on, waste a lot of energy, and sometimes lead to infinite polemics and unnecessary work. Somebody has to do it. Dr. Flexner was right in telling me "You go ahead and do that. Hear?"

Q: Was he pleased with the outcome?

A: I wouldn't say he was pleased. He was unhappy. But it was clean cut. I mean, he watched my experiments. I kept temperatures and the records of each monkey. I published in full in the Journal of Experimental Medicine. As far as he was concerned, that was an end to a chapter, and an end to Mr. Jungeblut's claim.

Q: What I find of interest is that the papers of Jungeblut were originally accepted by the Journal of Experimental Medicine.

A: On their face, they looked perfectly alright.

Q: Since we're talking about controversy, I thought at this point we might tie off the "B" virus because in a sense, it ends in a kind of brief controversy and it shows a kind of misconception that some people have of the nature of the work that you did and the work that Gay and Holden did. On May 9, 1935, and I'll read this into the record, you received a letter from Josephine B. Neal. Now, could you tell me who she was, and what her importance was.

A: Well, my recollection was that she was a very excellent neurologist. And that she was the executive secretary of a commission that was in existence at the time at Columbia University Medical Center, called the William J. Matheson Commission for
encephalitis Research, which incidentally, my own boss and benefactor, William H. Park was a member of the Commission.

Q: And so was Frederick Gay.

A: And so was Frederick Gay. It was predominantly a Columbia University group. The chairman of this commission was the dean. I don't know if he was dean at the time—William Rappalye.

Q: I'll read her letter and if you want, you can read your reply and then make a comment.

A: Did I write her a reply?

Q: Oh, yes, You have it right—

A: Because I find in my collected reprints also a discussion after a presentation by—Alright I'll bet to that later.

Q: Well, you write her a very interesting reply. But let me read the letter first. This is dated May 9, 1935.

A: Let's set the record straight. This is about less than five months after I return from the Lister Institute where I had done my work on "B" virus, both on its behavior and its immunological properties as I reported in my letter to Dr. Rivers. And also, I think it may have been before the material was actually published in the British Journal of Experimental Pathology. I don't remember.

Q: Yes. She says "Dear Dr. Sabin:

Will you please sent me two copies of your article 'Mechanism of Immunity to Filterable Viruses' and any other reprints of articles that you may have written but which you have not already sent me reprints. Since your return to New York, I have been hoping to see you personally. I was not a little suprised at your footnote
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in your first article on the Brebner virus to the effect that Dr. Gay and Dr. Holden had received from me a specimen of the material which I had received from you.

If you recall the circumstances, you will remember that Dr. Applebaum was present at Dr. Brebner's necropsy at Dr. Park's request and obtained several specimens of tissue which he brought to me and which I later divided with you. The footnote of your article carried with it an implication that the facts of the case do not bear out. I discussed the matter with Dr. Park and he was not a little surprised at your statement. I also wrote to Dr. Wright that I did not receive a reply.

As you know, Dr. Holden isolated the--"

A: Excuse me. Wrote to whom and did not receive a reply?
Q: Dr. Wright.
A: Oh, yes.
Q: "As you know, Dr. Holden isolated the virus first, and I believe spent considerable time in talking over various problems concerning it with you. For none of this did you give her any credit in your article.

I should be very glad to know your attitude in this whole matter."

And then you replied a week later.

A: You want me to read the letter and refresh my memory while I am looking through the discussion--

Q: Yes. Why don't you read the letter because the letter, I think, is a very interesting one.
A: The date of Dr. Neal's letter was May 9th and I replied on May 16th. At least I took a couple of days to think it over.

"Dear Dr. Neal:

"I was very glad to have your letter and hope you will forgive me this delay in answering it. I have been devilishly busy and wanted to give the matter more thought than I had time when your letter came.

"To begin with, I am sincerely sorry that the footnote to which you refer in my paper with Dr. Wright" in parenthesis, Dr. Wright was professor of surgery and I made him co-author on the paper, although he didn't have anything to do with the work. "is capable of the interpretation which you have apparently, and not unjustly given it. The wording is undoubtedly ambiguous, but I want to state that the 'us' referred to the Department of Bacteriology and Surgery and the hospital division rather than to Dr. Wright and me.

"The work came from the department headed by Drs. Park and Wright and I worked in both of them. The case was intensively studied during life and after his tragic death, we had every intention, to say nothing of the extraordinary personal interest, to complete the investigation with the post mortem material.

"That we had every reason as well as right, if not sole right, to investigate this case, can hardly be denied. As you may know, the material which I received from Dr. Park was not the only material available to us. You say in your letter that 'Dr. Holden isolated the virus first, etc.' and that you would like to know
my attitude in this whole matter.

"It is perhaps incorrect to say that Dr. Holden isolated the virus first, for several reasons. Actually, we each independently and simultaneously isolated a virus from the tissue from the same case. But if one were to read Drs. Gay and Holden's reports and my reports, one would gather that not only was two different clinical and pathological entities being described, but it would also have to be proven that the two viruses are the same. I think they are the same, and there is now sufficient evidence that it was not herpes as claimed by Gay and Holden. Perdrau has been making a comparative study of the "B" and "W" viruses." This refers to Perdrau at the National Institute then, Medical Research in London.

"In my talks with Dr. Holden, I told her of the visceral lesions in the patient and experimental animals and of the conception that the case represented a unique clinical and pathological entity caused by a virus which most likely was not herpes and even suggested some form of joint or simultaneous communication. The only answer to this suggestion was an early, preliminary report in the Proceedings of the Society for Experimental Biology and Medicine.

"In their reports, the case was incompletely described, clinically as an encephalitis, pathologically as acute disseminated encephalomyelitis, and eugologically as herpes. And none of these assertions, as you know, is correct."

"The pathological diagnosis was incorrect, even on the basis of the description which was given, since it was distinctly stated that no perivascular demylenization was present. And that feature
is the outstanding and most important characteristic of acute disseminated encephalomyelitis. That the case represents a unique hitherto undescribed clinical and pathological entity in which the skin lesions, the necrosis of regional lymph nodes and visera as well as the central nervous system involvement, form a part of a single picture and are caused by a virus which hitherto, with hitherto undescribed biologic and immunologic properties is a conclusion which one is led to after a study of all the data.

"You'll find that I have always referred to the publications of Gay and Holden and wherever possible, attempted to integrate their data with my own. Please believe me, Dr. Neal, that I am far more interested in uncovering a little more of the truth of certain diseases and their causes than in claiming credit for what I do. I have no illusions about my meager accomplishments. I regret very much that I did not have the opportunity of discussing this matter with you personally in the past and hope that we may do some time in the future."

Now, I think, as an addendum to this I would like to read a discussion of a pathological data on this "W" virus that was subsequently reported and that I discussed so. Can you shut it off a moment so I can find it.

Q: Sure.

A: In the spring of 1935, this was about the time these letters were exchanged. Dr. Abner Wolf and Margaret Holden presented a paper at the meeting of the American Association of Pathologists in New York, entitled "Studies on Inclusion Bodies of a Neurotropic
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Virus" Of a neurotropic virus "and Various Organs"

Abner Wolf is, was then, and became even more so a very eminent neuropathologist. He worked at Columbia. In that report they speak of the "W" virus, originally isolated by Drs. Gay and Holden from a case of fatal ascending myelitis. And they described the studies in a series of rabbits after inoculation of this virus. Those descriptions are very excellent. At the end of that abstract, as it was published in the American Journal of Pathology, they said

"The so-called "B" virus, isolated from the same human material by Dr. A. B. Sabin is probably identical with the one here described." But they still say nothing about what its identity is. Is it "W" virus, what? Something out in outer space? Is it herpes virus? Or is it a distinct virus as I had claimed?

Permit me to read here my discussion immediately after this presentation.

Q: I'd love to hear the discussion.

A: "This virus, the pathology of which Dr. Wolf has just described is very interesting. It was isolated simultaneously from the same case by Drs. Gay and Holden and myself, by myself. They called it "W" virus. I called it "B" virus. I see by this work that the two viruses which we have studied individually are apparently identical.

But at one time, they expressed the opinion that their "W" virus was herpes. I should like to know whether further studies since then have caused them to change their minds, and whether the description just given for herpes, was for herpes or for a distinct
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virus. I've since then published the results of an extensive biological and immunological investigation which showed that while the "B" virus is related to herpes, and to pseudo rabies, it can be distinguished clearly from them by serological and biological methods.

"One of the important biological distinctions from herpes is that the "B" virus is pathogenic from a macacus rhesus monkey. It may be recalled that the human disease followed a bite by a rhesus monkey. It has been possible to show that apparently the "B" virus causes a mild natural disease in the rhesus monkey. While when transmitted to a more susceptible hosts, it produces a disease and lesions of the same type seen in man and the rabbit.

"It belongs to a group of viruses which might be termed pantropic because they're pleura cellure (?) and because of their pleura cellure affinities."

Dr. Wolf in answer to me:

"In answer to Dr. Sabin, to my knowledge there has been no further work by Gay and Holden on the question of the relation of this virus to the herpes virus. My own interest in the problem is in the pathology. And from that point of view, I consider that this 'W' virus is identical with the 'B' virus. In the majority of the organs inflammatory changes were present but these were not marked and did not form an important part of the picture."

Well, this is in response to something else. In other words "My own interest in the problem is in pathology and from that point of view it is identical with 'B' virus and they never did anything more." And this is the last word.
Q: But they didn't at this time recognize, even after your papers that "B" virus was part of an evolutionary tree?

A: As a matter of fact, I may have said it before, but I think if I had been as sophisticated as Dr. Gay was, I probably would have missed the boat too. Maybe I was too eager, you see, to pursue the differences that I observed and less eager to make a case for something that Gay and Holden had already previously made up their minds on. Because as members of this Commission, they had put forth the hypothesis that human encephalitis was caused by herpes virus.

And so, while this is just another proof, you see, that this was caused by herpes, it just so happens that herpes virus, human herpes virus does cause encephalitis in some persons, but it also so happens that they were too eager merely to get additional confirmation for their hypothesis and in their eagerness, overlooked important differences, which to me as a novice, I was interested in pursuing further. And I must confess, it was during the pursuit and these unusual properties of this virus that I got to know Tom Rivers fairly well. Because I was a novice, and I took the sections. I took my results before going to London and I showed it to Tom Rivers. Tom Rivers was in on all of this, and he kept saying, "Of course, yes, these are differences, but you have to have more evidence and data to show that it's different, and if you claim that this is a virus that came from a monkey, or if you maintain" I don't think he said claim "that it came from a monkey and that it may be different from human herpes, you'll have to prove it." And that is why, when I went to London, I really did
two things that he asked me to prove. One was to find out whether or not vaccinia can really grow in the absence of living intact cells and the other was to really find out whether this virus is a totally different virus from the human herpes simplex virus.

Q: But the interesting thing to me is Dr. Neal and Dr. Gay are literally "the establishment" (sentence incomplete)
A: Well, they are.

Q: Well, look, it's the Matheson Commission on Encephalitis, and when you speak for the Matheson Commission on Encephalitis, you speak with thunder. The fact that, although Rivers was part of the establishment, he didn't mind, as a scientist, he didn't mind fighting with them.

A: Well, let me see how much of it was fighting. As a matter of fact, he was more open minded than they were. I think Dr. Park my old friend, was a member of the Commission. When the data were in, and I had the data. I pursued it further when I went to London and when I came back, I had the data which was quite definitive. They didn't. I think that Dr. Park ultimately, because this was--I had no more communications on this after '35. There were no more publications after this exchange in May, 1935. I think both Dr. Park and Dr. Rivers prevailed and said "Well, you just went out on a limb."

Q: I'm interested to see that this kind of debate can occur.
A: What language Tom Rivers may have used privately, I don't know.

Q: One only hesitates--
A: I had a feeling that he sort of relished it, because Tom Rivers was not above that sort of thing. "Here, look at the big professor at Columbia. Then this kid comes along, you see, wet behind the ears, just learning about viruses, and he finds something you and Gay misses the boat." I think there was a little of that at least that he conveyed--

Q: There's one other thing that I'd like to talk about today, and then with thing, I think we've done enough for today. And this is the work that you do on the in vitro cultivation of polio virus using two three to four month old embryos which are obtained aseptically by an intern named Lance Monroe at the Rockefeller--I mean at Bellevue. And could you tell me about this because in a sense, you know, there is absolutely fantastic debate now in the Edelin case with fetal research at--in Boston. You want me to stop here?

A: Now, Dr. Lance Monroe and I came to know each other when we interned together at Bellevue Hospital. When I left at the end of 1933 to go to England, he became resident on obstetrics and gynecology. As I indicated in the second paragraph of the publication on this "The problem of cultivating the virus of poliomyelitis was being pursued not only for the further elucidation of the nature of the virus, but also in the hope that successful in vitro propagation may facilitate attempts at adaptation to new hosts in tissues and provide new material for further experimentation on active immunizations." Always the business of active immunization.
Now, the concern for active immunization which Dr. Flexner had, which led him to suggest certain experiments to do by Oletsky and others in the department obviously there, and this was tied in. Now it was also despite many publications the conviction on the basis of the available data, the workers at the Rockefeller institute and elsewhere were in accord that no propagation of polio virus had been demonstrated by methods which had proved successful for cultivation in most other viruses up until that time.

I then asked Dr. Monroe if he could provide me with some human embryos which become available in many big hospitals during the course of therapeutic abortion. And I want to call attention to the fact that as stated, in the publication, they were three to four months old, human embryos. These are embryos which are incapable of independent life. I don't know why these particular ones were removed, abortion was certainly not practiced and certainly not in a public hospital of Bellevue, but there are therapeutic abortions—for the welfare of the mother etc, etc.

I think that there is no connection between this and the situation which you mentioned, which is apparently under discussion. Dr. Edelin's experiment—with much experiments with much older embryos. I would like to take this opportunity, as an aside, to indicate my own conviction that what I hope will come out of the present time, will be a definition of life. My own definition of life is the following: Life begins for something that is developing inside the womb of a woman only when, if the mother
dies, or if that product of conception is taken out of the womb, it then has the possibility of independence. Without the potential for independent existence, it is not a living thing, and it is not even a human being, because if you carry it further back, you might say that life begins when the egg is fertilized by the sperm of its owner.

Of if you carry it still further back, you say the sperm of its owner's the alive, the ovum is alive, because when the two come together, they have the potential for giving life. Now, instead of going backward, let's go forward a little. In the earliest stages of development, after the egg is fertilized by the sperm, the embryo goes through developments which pass through the development of the species. Antogeny recapitulates filologically (?). In other words, every stage from the earliest period of evolution that the race has gone through, the human species has gone through, the embryo goes through. At certain stages, it doesn't resemble a human being any more than an embryo in a mouse resembles a human being. So that, in effect, in my judgement, the definition of life is when something has the potential for independent existence. Therefore, this doesn't concern this controversy at all.

When I look at this publication and see the naive and unsophisticated way in which we worked with living tissue, I am surprised that any positive results were obtained at all. Yet, this work was possible only because two embryos were gotten very close to one another. And it was possible to make serial passage on tissue which survived in the refrigerator. The results
for the first time, showed that with the MV virus which I must say
now, unfortunately, we used at the time--this Rockefeller Institute
virus--passed for twenty years in the brains of monkeys--that were
their virus--multiplication could be demonstrated only in the
nervous tissue of the embryo. And the other tissues of the same
embryo that I carefully tested, the spleen, the liver, the
kidneys, the lungs, nothing.

Well, this obviously, was very pleasing to Dr. Flexner.
He said "Ah hah, this goes to show," and to others, Dr. Faber.
There were a number of people, who at the time, regarded polio
virus as being a strictly neurotropic virus, meaning that it
multiplied only in the nervous tissue. That it came in through
the nose, invaded the nervous system, and then was all in the
nervous system. Well, as you know, now, it was about fifteen
years later or so, fourteen years later the John Enders decided
to repeat these experiments, as he has indicated in his Nobel
lecture.

He--in order to make it easier to test--I don't think John
Enders ever inoculated a monkey. I'm not sure. But because
Armstrong's mouse-adapted virus was available at that time, and
it would be easier to test the multiplication in mice, whether or
not it multiplied--he didn't use the MV strain. Or any other strain.
He used this so-called Lansing strain which didn't have too many
passages away from the human, and it was passed in mice.

Well, as we were able to show subsequently, that strain of
virus is not strictly neurotropic. It hadn't lost its original
human qualities. And that's strain multiplied in the non-nervous tissue as well as in the nervous tissues. Enders had confirmed and his collaborators had confirmed that multiplication in the nervous tissue of the mouse, of human, but because he used a different strain. And when subsequently, I went back and repeated the tests exactly, but again with the MV strain, I found that what the Lansing strain--the mouse-adapted strain did--the MV strain did not. The MV strain was strictly neurotropic. The Lansing virus was not.

Q: I think Enders was served by two other things that people sometimes overlook. And the two other things were one, circumcisions were done routinely in the Boston ____________ Hospital or the Children's Hospital. And secondly, he had antibiotics--

A: Let me tell you why that is not crucial. It's interesting to the subsequent work that was done. The original work was done on human embryonic tissue.

Q: Was the original work done on that?

A: On human embryonic tissue as he states, he started out by where I left off. And it was only subsequently through the work which the contributions of Weller and Robbins that the material that had multiplied in the non-nervous tissue as well as in the nervous tissue of a human embryo was carried over to cells grown out from bits of human skin, or then monkey tested and so on, that it was found that another important discovery was made.

It was the important discovery that the virus can destroy those non-nervous cells outside the body. This was a discovery
particularly due to the efforts of Robbins. Now, the primary thing--it didn't--I had no problems with working very carefully. I had no problems with infection. It was not that infection. He was not served. It was possible to carry it further because antibiotics make tissue culture work better and easier fifteen years later than when I was working, but that wasn't crucial. I would say that fifteen years later, the main contribution was another strain of virus. If I had done the same work outside of the Rockefeller Institute, if I had used a freshly isolated strain of human virus, I would have found the same thing in 1936. That was the crucial issue.

Q: The one thing --
A: It's a different strain of virus. It's an important lesson.

Q: The one thing that is also clear is that everyone knows about the neurotropic qualities of the MV virus. It's stable, that it's neurotropic. It has neurotropic--

A: Every other polio virus is neurotropic. The difference is that the MV virus will not multiply in non-nervous tissue, whereas freshly isolated strains, strains as they are found in nature, before they have been changed by many passages in the laboratory under conditions which the MV strain had been submitted to, it has additional properties. The property of multiplying in other tissues. And that is why virologists at the time, and I among them, spoke of pantropic viruses, meaning viruses with an affinity for the nervous system, as well as for other tissues. Strictly neurotropic viruses, meaning those that under certain
conditions would multiply only in the nervous tissue, you see, so it's a matter of strict neurotropism and it's also very important that carries with it, in my judgement, a very important lesson for the present and the future.

And that is, that if you are working with a virus that has been out of the original host, and in this case, let's say the human body, and it's been submitted to a lot of changes in the laboratory, don't for heaven's sake, draw conclusions about the human disease or the original virus from a laboratory product. We're still facing this situation. I think much of the work with animal tumor viruses, cancer viruses, belong precisely in the same category. Because it's easier--there are hundreds of people doing work with artifacts that have been passed in the laboratory and have changed in such a way that you can publish ten papers a year if you work with them and not with the viruses as they occur in the original animal--in the original tumor of the animal.

This has many other, let's say, relevancies to medical virology.

Q: I think at this point, we can stop. We've gone for two hours.

A: No, we've gone for three hours and three quarters.