Hayflick, Leonard 1994

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Interviewer: Dr. Robert Stevenson, formerly of the National Cancer Institute

Stevenson: (Begins mid-sentence)--and we're freely associating about some of the history of tissue culture, currently talking about Dr. Wilton Earle's laboratory.

Hayflick: I don't recall who told me this story, so it could be apocryphal, but Wilton Earle allegedly had been persuaded that the only soap in the world capable of cleaning his glassware sufficiently for cell culture was a soap called "The Gold Dust Twins," which my parents remember, but I have to confess I didn't.

Stevenson: We used it at home.

Hayflick: You used it at home?

Stevenson: Yes.

Hayflick: Possibly I've seen it, but I don't recollect. In any case, you will recall the religious fervor with which people washed their glassware in order to be sure that it worked correctly and that their religion was completely different from other peoples' religion. Well, that was the degree of interest that Wilton Earle had in this soap product when he learned that it wasn't going to be manufactured any longer. And my understanding is that he took what remained in the rest of his annual budget and bought some horrendous volume of this material. I'm recalling, probably with exaggeration, one or two boxcar loads, and had it delivered so it would last him the rest of his professional life. Those are, you know, one of the Earle stories, and there are many other interesting Pomerat stories.

Stevenson: Well, Earle told me one time that he had problems with sodium chloride and how he realized that he had to get batches of this stuff in and pretest it because one batch of chemically pure sodium chloride that he had turned out to have been produced in some kind of a silver evaporating dish and he had silver chloride mixed in with the sodium chloride, and you can't get anything much more toxic than that for cells. And so this batch of sodium chloride, when made up into saline, was killing everything off. And he tracked that down. So there were these stories, one after another, with various and sundry things which were found to be, at one point or another, in his culture history, causing a number of very severe problems and, one by one, he rooted them out until he developed a procedure which was almost a religion in terms of the various and sundry products that were used. Different kinds of glass, for example, weren't satisfactory because they etched when exposed to strong cleaning solutions, and one thing or another.

Hayflick: Yes. I've gone through a lot of that, and I'm sure you have too. As a matter of fact, I have, now that I've finished my book, *How and Why We Age,* some ambition to do another one on that era and anecdotes and stuff that I know about and probably will return the favor someday and come back to you and ask to interview you about some of those things.

Stevenson: Well, there are plenty of those anecdotes.

Hayflick: The reason I am persuaded to do this is largely because of the people with whom I interact at Genentech. I've been with them 12 or 13 years now, and they have lots of young people there in a huge Cell Culture department. When I was first there we were culturing cells in T flasks and I raised them from T flasks to 12,000 liter fermenters, which is what we're using today. That gives you some idea of how big this thing got.

Anyhow, I regale them frequently with stories because it makes a point very effectively in some of the things that they're doing. And some of the things would really be, I think, not only entertaining, but of scientific interest to a lot of people.I remember--I don't want to go through all of them now, but--

Stevenson: Let me ask you another question here.

Hayflick: Oh, one of the things I wanted to mention is I probably have something in the neighborhood of hundreds of pounds of paper for you. Is this something that you would like to get, to receive?

Stevenson: When you do an appraisal of what you have, let me know and I will contact the Library. My tentative answer is I think they would love to have it all. Seriously.

Hayflick: Well, I would prefer--It's sitting there and I have no specific plans for its use, and I'd just as soon give it to an archival unit, rather than throw it out.

Stevenson: Well, I'll tell you, the whole--Your career and your development of the WI-38s, has impacted on so many different activities in microbiology that inter-relate and connect to that, that your collection would serve as a nucleus to tie in with a tremendous variety of very interesting activities on the parts of other people and other institutions, and I think it could be a very critical resource for someone who is going to tease out these relationships in future years. So, I really think it is valuable material.

Hayflick: You know, I never threw out any letters that were written to me. I probably have a file with your name on it, with many letters in there. I know I have two files that thick of correspondence with Frank Perkins that covers a whole period, of Rod Murray and before Rod Murray and after Rod Murray, and Frank's--now that he's dead it probably doesn't matter but, you know--sarcastic remarks about Murray and all that stuff. You know, I have all that from lots of different people. So, I want to be clear on what you want. If you want all that stuff—

Stevenson: Oh, very definitely. And I think it would be a marvelous thing to have that all preserved.

Hayflick: Okay. I'm going to keep you busy for a long time.

Stevenson: Well, that's great. I think that would be helpful.

Hayflick: Yes. I think I have-- I recall seeing a file well before the Cancer Virus era in which you invited me to NIH--I think it was one of my first trips to NIH--it must have been in '59 or '60, or '61, where you had a committee, and I remember distinctly Tom McCoy was there and you were there, and I was there, and I can't remember, but there were a couple of other people who were there. Do you know what this was?

Stevenson: It was the early C4 Committee.

Hayflick: Oh, that's what you're calling the C4 Committee?

Stevenson: Yes. It was at that time that we tried to get a whole spectrum of what kinds of materials were out there that would be interesting and useful.

Hayflick: There was another guy who--damn, what was his name, from Michigan or Minnesota--

Stevenson: Syverton?

Hayflick: That's right. Jerry Syverton.

Stevenson: Jerry Syverton. He was chairman of that committee eventually. And he only presided at two meetings and dropped dead up at--

Hayflick: In New York in a cab.

Stevenson: In a cab. He had just come from Sloan-Kettering and was going to the airport. And then Bill Scherer, who was on his faculty, took over the committee as chairman and it was mainly Scherer's activities that started that whole cell banking collaborative project that got to be known as the C4 Committee.

Hayflick: That's right.

Stevenson: But you see, those people, Bill is dead now and has been for some years.

Hayflick: Bill?

Stevenson: Scherer. And so a lot of these people who were in on those early developments are no longer around to talk about them.

Hayflick: Is Charlie Boone still around?

Stevenson: Yes. He came to my retirement party.

Hayflick: Did he really? Where is he?

Stevenson: Yes. He's still at the NIH.

Hayflick: He is?

Stevenson: Yes. He'd gone off from NIH and went over to Saudi Arabia and was a pathologist for one of those hospital complexes over there and made some money, or whatever, then came back. I think he's married for the second or third time, and so forth. He's as crazy as he ever was.

Hayflick: He was always an off the wall guy. But he did some good work. I keep citing his papers and people have never heard of him. Some of the shocking things--You know, people accept the fact that the continuously-I'm trying to think of the names of the lines now. I know he did L929. I don't know whether he did 323, but I think he did. Boone grew those cells on beads.

Stevenson: Right.

Hayflick: And, first of all, when they're not grown on beads and introduced into appropriate laboratory animals, they rarely produce tumors. But, when he put them on beads and put them in animals, he got tumors 100 percent of the time.

Stevenson: Right. He also coated pieces of plastic and things like that, which were being used for prosthetic stuff.

Hayflick: Nobody, of course, ever reads those papers. That's another thing I'm sure you've realized too, that nobody reads the literature that's even 10 years old, to say nothing of 20 or 30. People are totally unfamiliar with that, and that was extremely important in the early days of the biotech industry when, as you know, the leap from diploid cells to heteroploid cell populations occurred literally in a matter of weeks, whereas the leap from primary cells to diploid took a decade or more. And I've written about that too. But, in any case, that paper has never been given the attention that it should have been given. It was very important at that time.

Stevenson: A lot of people don't recall that when I had Boone we had no laboratories in my program on the campus at NCI, and we had Boone and his fellows out at Melpar, which was a contract facility, and some of the people that I hired and put in his laboratory were George Todaro and Stu Aaronson, and one fellow, who is professor of pathology at Western Reserve, who is fairly quiet but does extremely good work, is Thomas Pretlow and he has continued the work he started, because we furnished them with the early prototypes of the Anderson cell centrifuges--not the virus centrifuges, but the cell centrifuges--and he started in and got interested in cell separations in studying pure populations of different kinds of cells using technology that was made possible initially by that cell centrifuge. And then others have used different kinds of ficoll and other gradients and salt solutions, et cetera. But his work, I think, is superb.

Hayflick: It is. Yes. And the other--The thing that I failed to recall earlier, I just remembered, and that is this. My affiliation with Genentech began at a time when they could only produce biologically active tissue plasminogen activator by recombinant DNA technology, in CHO cells. So here was a product that was being sent to FDA for approval and the first[was] produced in a heteroploid cell population that produces a low-level of tumors in animals. Well, of course, that was a huge worry. I remember talking to people about it and, in a cavalier fashion, mentioning something in that context about studies done a few years back wherein these cell populations were inoculated into humans. And they said, "What?" And I said, "There's a whole bloody literature on that." "HeLa cells?" they said.

Stevenson: Chester Southam and then Alice Moore.

Hayflick: Of course.

Stevenson: Yes. Nobody remembers those.

Hayflick: They said, "Give us the references." Well, hell, we did that in my first paper on the diploid cells. We put diploid cells in one arm of terminal human cancer patients and HeLa cells in the other.

Stevenson: Well, that was what, you know, I felt was the reason why Rod Murray would never have anything to do with it because Wilton Earle and George Hyatt from the Navy Tissue Bank had developed that so-called "human skin epithelium," and they buttered that onto a surgical defect in a volunteer and it turned angry looking, and they sampled it, and it was full of all kinds of crazy mitotic figures and so forth, and Wilton almost had a heart attack. They cut the stuff out and repaired the defect, and all went to church and said their novenas and stuff, and hoped that the guy wouldn't come down with cancer. But Wilton then called me aside, because George had hired me to head up this tissue culture lab in the Tissue Bank. Up to that time they didn't have one. And this was in 1958. And one of the first jobs I had was to try to put this skin line onto collagen, bovine collagen that we had gotten both in sheets and in gel form to see if we couldn't use this as a substrate for growing skin epithelium. And so, when we were getting some little degree of success, and I went to Wilton and talked to him about it, he sat me down and told me this whole story very confidentially, swearing, you know, I'd never say anything about it because he was worried about it. and he said, "Don't ever trust these human cell lines because they'll transform and turn malignant and the goose will be cooked."

Hayflick: Well, I don't know whether you remember it, but I'm sure you were in Wilton's lab at the time. The year must have been '61, or '62. Were you in his lab in '61 and '62?

Stevenson: I was at the Cancer Institute then in the Virology Research Resources Branch.

Hayflick: You may remember that--I don't want to say it was a letter, because it wasn't--it was a committee report published right in the beginning of *Science* magazine signed by Wilton Earle, Karl Habel, Joe Smadel, and two or three other leading lights at NIH. Only NIH people had a monopoly on the truth. This was a committee that was set up intramurally at NIH to address the issue--why they chose to do it at that time I don't recall--to address the issue of substrates to be used for virus vaccine production. And it was a one page, at least one full page in *Science* with a declaration by these people stating their opinion that human cells could never be used for manufacture of human biologicals. And, what you're telling me came out clearly in that report, Bob.

Stevenson: Well, you have to remember too, at that time, that the whole problem with SV-40 and the monkey kidney stuff had come out, and that--

Hayflick: Well, not in '61 or--

Stevenson: Yes, it did.

Hayflick: Oh, wait. You're right. '61 and '62. It would have been out by then.

Stevenson: They were scared to death of all this stuff, of latent viruses and all of this stuff. And so they thought that continuous passage materials--

Hayflick: Well, it was the continuous passage that I recall that they emphasized, not so much the "human," although I think they did mention "human," it was continuous passage of normal human cells that they said would be dangerous because they always transform. Of course I had evidence that this never happened, and it hasn't happened to this day.

Stevenson: But, of course, in terms of the majority of the cell lines that they had that were non-fibroblastic, all the epithelial cells--

Hayflick: They were rodent cell populations for the most part. Well, the thing that upset me about that was not only the opinion that they had reached, but that they had set themselves up as "the" authorities in the area without consultation or bringing in people, non-NIH people.

Stevenson: Yes. Well, that was typical.

Hayflick: And that really was a tough struggle. We wrote a letter. In fact, were you a signatory to that letter?

Stevenson: I may have been.

Hayflick: You may have been. There was a letter that I think I drafted and it was massaged later by the people who signed it. T.C. Hsu, Charlie Pomerat, myself, and maybe you, were on that first letter. I don't remember. Although you were on some later letters. Maybe you weren't on that one. I think it was T.C., Charlie Pomerat and myself, when we responded to that. And they printed it in *Science*. It was a long letter in opposition.

Stevenson: But you must recall too that there was a plethora of ignorance at that time.

Hayflick: Of course.

Stevenson:I remember the last papers that I presented while I was still an NIH employee were at Prague, at an IABS meeting, and at that one we presented work done under contract with Pollard at Notre Dame. He had discovered that all of the germ-free mice strains that they had at Notre Dame did, in fact, have murine leukemia virus associated with the germ line and, up until that time, people had assumed that if they had germ-free mice, that they had no latent viruses or anything else in them, and here were these viruses fully integrated into the genome.

Hayflick: Did you know Morris Pollard well?

Stevenson: Not very well. Fritz and Gene Deinhart, friends of mine, knew him quite well.

Hayflick: I knew him very well. I don't know whether you knew--He was at the University of Texas in Galveston when I was there.

Stevenson: That's right. He was there.

Hayflick: And that's where I learned the little bit of virology that I've picked up. We became very close friends, not only scientifically but also socially, to the extent that our children were raised with his. When one of his sons came to Stanford, and Morris and Molly (his wife) called and said, you know, like any concerned parents would, "We have a son at your university. He's a freshman. Will you look in on him?" And I said, "Sure. We'll have him over for dinner a couple of times." And we did. You know who it was, don't you?

Stevenson: Yes. Johnathan's in jail.

Hayflick: You know he was a snotty kid even then. He just thought that he was smarter than everybody.

Stevenson: That must be a terrible cross to bear for that family.

Hayflick: I don't know how they survived. Morris I can see surviving, but Molly, I can't see how she survived. I really can't.

Stevenson: He wrote a book called *The Physics of Viruses*. He was very good in--

Hayflick: Yes. Well, he was the sponsor of the "Perspectives in Virology" conferences.

Stevenson: Yes.

Hayflick: He got it started. That's another interesting story, how that got started. All these little stories. It was started by a virologist here at the University of California at San Francisco. I think he's dead now, but during that period he was in his late 80s and 90s. He was head of the Hooper Foundation at UCSF where the current NIH Director was serving.

Stevenson: Shannon?

Hayflick: No, no, the current head.

Stevenson: Varmus.

Hayflick: Varmus. Harold Varmus was head of the Hooper. And the Hooper Foundation was founded by K.F. Meyer. Yes. He was a legend at UCSF. Anyhow, he was approached by a fellow who recently immigrated to this country.

Stevenson: Well, it was the Hartz Mountain seed people.

Hayflick: Yes. That's right. You know the story then. Yes, it's the Hartz Mountain. The guy who--

Stevenson: Canaries and pet foods and--

Hayflick: Well, they were importing birds into this country and they came down with psittacosis.

Stevenson: Psittacosis.

Hayflick: Well, you do know the story then?

Stevenson: Not all of it. No.

Hayflick: Well, the birds came down with psittacosis and the Feds said, "No more importation of birds." This guy thought to himself, "No more importation of birds and I'm financially dead." So he went to K.F. Meyer at the Hooper and he said, "I have this problem. You guys are virologists, rickettsiologists, whatever, can you help me?" They said, "Well, we'll give it a shot." He said, "I'll give you my last few bucks. Do what you can." And they did, and they discovered what the organism was. They said they couldn't cure it, they didn't know how to cure it, but the suggestion they had was to quarantine these birds. From their studies it indicated that if they didn't show disease after a certain period, then they remained healthy. And, they wrote papers on this. And the head of Hartz Mountain, Gustav Stern, got legislation passed and the birds poured into the country and he made zillions. By this time Morris Pollard was a student there. Gustav Stern said, "What do you want for helping me?" And they said, "We want you to support science." "How can I support science?" "Fund the Perspectives in Virology meetings." And that's what he did and that's how these great meetings got started.

Stevenson: Okay. So that's one for the record. Well, tell me a little about how you got to Texas and then how you got to the Wistar?

Hayflick: Just a second. Okay. How I got to Texas.

Stevenson: You were born?

Hayflick: I was born in Philly and went to the University of Pennsylvania for my Bachelor's degree, but I took a leave of absence to join the Army in 1946, mostly for financial reasons. My sister, who is a year and a half younger than I, was graduating high school, and I decided to join the Army for 18 months in order to get the G.I. Bill which would provide me with a four year college scholarship. I did do that. Actually, it was a stroke of genius, because I would have escaped the draft in 1946 by 5 days. I was 18 on May 20th of '46 and the draft ended May 15th, so I could have escaped by 5 days. But, had I not joined, I would have been drafted into the front lines of the Korean War. That's when they began the draft, just at that date. Well, anyhow, I got my Bachelor's degree, and then I left and went to work at Sharp & Dohme which, when I was hired, I knew would be linking up with Merck. And Sharp & Dohme was in Glen Olden, Pennsylvania, and then I moved to West Point, Pennsylvania, where Merck was, and worked there, and overcame what I had previously believed to be true, and that was that I didn't have the tickets to get a Ph.D. I just was awestruck by my professors at Penn and thought I would never reach that far. But, working at Sharp & Dohme with all these Ph.Ds., some of whom I thought were dumb, I decided, well, I'm just as dumb as they are, so I'll go back to school, and I did, and I got my Master's at Wistar. I wanted to work with what were then called PPLOs, but the chief honcho in PPLO at Penn wouldn't have me. His name was Harry Morton. I don't know why. I think he had too many students. And so I became a student of Warren Stinebring. Do you remember Warren?

Stevenson: Vaguely.

Hayflick: Well, Warren Stinebring was a young assistant professor, and he said, "I'll take you as my student." I said, "Fine." I said, "But I'd like to work on PPLO." "Fine," he said, "That's great". In fact, he was so junior that technically I couldn't be a student of his. On paper I was a student of Stuart Mudd, the Chairman of the Department, who could sign all the official papers. I was technically Stuart's student, but practically Warren's student. And, I worked on the *Mycoplasmas* present in the Wistar rat colony which, at that time, was being used at the Wistar Institute in the world's first artificial insemination institution. And the Wistar rats were used for the Aschheim-Zondek test to determine ovulation time in the women who paraded through the Wistar Institute. Edmund Farris was in charge of the institute.

Stevenson: Hilary hadn't come?

Hayflick: Oh, no. This was years before Hilary came, years before. And I sat in my little lab in the Wistar rat colony. That's where I first met the Lewises. I have all kinds of funny stories about them, but I won't go into that in this hopefully brief sketch. And, then I got my Master's and went on to get my Ph.D. But, during that period of time Warren Stinebring, for reasons that I don't know--I probably did know at one time--heard about a tissue culture course being given in Cooperstown, New York, at Mary Imogene Bassett Hospital. "I'm going to the tissue culture course," he said. And, the other student and I waved to him and said, "Good-bye." And he left and, at the end of the summer he came back all full of piss and vinegar about this tissue culture stuff. "You must go into tissue culture," he said. And, Frank Capral, the other guy, who is now at Ohio State, and I, said, "Oh, forget it. We want to do--" Frank was interested in *Streptococcus* and I was interested in *Mycoplasma*. "No, we won't do it," we said. Well, he kept beating on us, and beating on us, and finally, being students, we had to come to some compromise and I said, "Okay, I'll do my Ph.D. thesis on the growth of *Mycoplasma* in tissue culture." He said, "Fine." And we set up the tissue culture lab.

Frank and I, during one summer, when Warren was gone for another course, I think it was, we built the first tissue culture lab at Penn with a little glass hood where you put your hands in, and Bunsen burners and this kind of stuff, and we did our work. And so I did my dissertation on the growth of *Mycoplasma* in tissue culture.

By that time Warren had left, about a year before I was to graduate. He continued to be my advisor and returned for the dissertation and stuff. But, about a year before I was to graduate he said, "Hey, Len, I'm down here in Galveston, Texas," which is where he went. I think by that time he was Associate Professor. They had this great program there called the James W. McLaughlin Research Fellowships. It's money that comes to the University of Texas from Oklahoma oil fields owned by James W. McLaughlin. They give out stipends and Warren said, "Maybe you'd like to work for Charlie Pomerat here?" Well, that sounded great. Pomerat was a leading cell culturist, and the salary was phenomenal. It was $3,600 dollars a year, but it was tax-free because at that time, you know, fellowships were tax-free, so it was really equivalent to more like $5,300 or $5,400, which was out of sight. So I said, "Yep, I will." And Ruth and I--by that time we were married--went down to Texas, and that's how I got involved with Charlie Pomerat and Morris Pollard. And, after being there for a couple of years-- Well, it's interesting the things that happened there, funny stories too, especially about how T. C. Hsu discovered the hypotonic treatment of cells to explode them and see individual chromosomes. It's a fantastic story. I don't know whether you know that one. It was an accident having to do with Galveston tap water. We'll talk about that one some other time. Anyhow, one day I get a call from a call from Werner Henle, who was one of my teachers at Penn, and a nifty guy. I don't know whether you ever knew him, but he was an amazing guy.

Stevenson: Oh, yes.

Hayflick: Everybody loved Werner. "Leonard," he said, "We've got this new guy coming to Philadelphia, coming to the Wistar Institute, by the name of Hilary Koprowski, and he's looking for somebody to run a tissue culture lab, and you're one of the few people I know who is into tissue culture. Would you be interested?" I said, "Yes. It sounds interesting. Tell me more." "Well, I'll have Koprowski get in touch with you."

He did, and the rest is history. He made me a nice offer, I went there, and that's how it all started.

Stevenson: Okay.

Hayflick: That was when I went back to Wistar. Okay, Robert, you don't want to hear my entire life.

Stevenson: Oh, I do. I do.

Hayflick: We'll save it for some other time.

Stevenson: No. It's interesting to get these things. A lot of this stuff--

Hayflick: Who else are you going to be interviewing?

Stevenson: Well, I've interviewed Harvey. I don't know that I'm going to have too many other people.

Hayflick :From that era?

Stevenson: From that era.

Hayflick: Gee, there are lots. Lots.

Stevenson: Very, very many. But the point is, I don't have either the time or the inclination to sit down and do an awful lot of this stuff. I'm going to do some of it and see how it goes and whatever and, as time permits, I'll take this machine when I sit down with people and talk to them. But, you know, it's not easy.

Hayflick: You know, a lot of the stuff that I have that you want were pulled out of my general files and put into about 25 linear feet of files for my lawsuit against NIH, so I have it mostly collected in those files. I've only written one book. I may do a second book on that incident plus lots of other little things that I have been trying to do, and might want that stuff as resource material. But I assume that if it goes to the archival place that you mentioned, it will be available to me.

Stevenson: It will always be there. It would be preserved against perils, both in deterioration and robbery, fire, theft, inadvertent loss or whatever, so I think it's an important thing to do. You have so much--you know--just in this little bit we've been talking and everything, there is so much there. It's a gold mine of information.

Hayflick: I agree, of course, which is one of the reasons I've been thinking about doing a book on this whole era. It was fantastic-- It was the golden era of cell culture and virology that we both experienced.

Stevenson: Yes. One of the things I tried to do was interest a young fellow named Steve Dickman, who is a *Nature* science writer for Europe and had been at Fritz and Jean's house one night I was there at dinner, in Munich, and I tried to interest him in writing some of this stuff up, but he felt that at the moment that there was no market for this kind of an historical thing and so forth. And then also, when I got to talking with him and relating some of the early history of this stuff, I found out that he didn't know who Jim Shannon, nor Hill, nor Fogarty, nor any of the people were from this period of rapid explosive growth of NIH. And I realized, you know, this is a generational gap here and that we've got to find someone who at least has some either knowledge, appreciation, and/or interest, in that era because it's damned hard to get the younger generation to think in terms of anything that's more than two years old.

Hayflick: Absolutely. There are so many people alive today who have played key roles. Joe Melnick for example. His name came to mind only because I was asked recently to write something for his--I don't know--what birthday would it be?

Stevenson: He'll be 80.

Hayflick: 80 probably. Yes. Were you asked to do something?

Stevenson: No.

Hayflick: Yes. I sent him a copy of my book. But Joe Melnick is really a gold mine.

Stevenson: Yes. He was involved with all this stuff.

Hayflick: So was Hilary.

Stevenson: So was Hilary.

Hayflick: And lots of people. Another person who has done this, Klein, George Klein. Do you know him?

Stevenson: Yes.

Hayflick: He's written several books. There is another long story about how this all happened. I've been corresponding with him regularly in the past few months and he's sent me several of his books and other things that he's published. For example, it's very interesting, and I just happened to think of this, he even mentioned in one of his books on the history of that era that tissue culture began with Enders, Weller and Robbins, and I had to gently point out to him that--

Stevenson: Applied tissue culture.

Hayflick: Well, you know, I should take that back. Virus growth in cell culture, which began in the '20s, not in the '50s but in the '20s. I don't know whether you remember but, in fact, there are a whole series of papers. People don't even know about these papers. I know the woman--she worked at Penn; she was a pain in the ass, but she worked at Penn--who grew poliovirus routinely in neural cultures in her lab, nervous tissue cultures, from humans and monkeys constantly.

Stevenson: How did she prove it?

Hayflick: Because she grew the virus, diluted it and put it back into monkeys and got the polio.

Stevenson: Really?

Hayflick: Oh, yes. This is well recorded. Now, the reason it's important is this. Not only was she one of the first, but there were Maitland cultures in which they grew viruses in the late '20s, but it was important because at that time Albert Sabin--

Stevenson: Tried the neural cultures and it didn't work for him.

Hayflick: Well, I'm not so sure about that because--

Stevenson: Well, I am.

Hayflick: Well, what he said was, Albert's claim was, that polio only grows in nerve cells and to try anything else in culture is a waste of time. And that's the way it stood with that *ex cathedra* statement until Enders, Weller and Robbins came along.

Stevenson: Well, it would interest you to know that Albert was present at the commemorative meeting, or whatever you call it, several years ago when they had this retrospective on the Virus Cancer Program. And Albert got up and opined--and it's on tape--that he didn't think much of any importance ever came out of that program, that entire program.

Hayflick: That nothing of importance ever came out of it?

Stevenson: No. So, Albert--

Hayflick: I heard him say, and I'll never forget what he said, at the Clinical Center meeting in, when was it--it must have been in '82 or '83, somewhere around there. This was the meeting--I don't know whether you ever heard of that meeting--but this was the meeting that was set up by NIH, but specifically by FDA--as you know, that's how they usually do things; when they need to make a critical decision they get all of the leading people at a big meeting at NIH and talk about the issue--and the issue was do we sanction the use of heteroploid continuously propagable cell lines.

Stevenson: I was at that meeting.

Hayflick: Were you at that meeting?

Stevenson: Yes. And I gave invited comments.

Hayflick: Well, do you remember what Albert said when he got up? It was unbelievable. In fact, I wrote this in a comment on the news when published. I was one of the few people that nearly fell off their seat when he got up and said the following. He looked up and said, "I don't know what the problem is here," said he, "but people have been using diploid cells which are continuously propagable for years to grow vaccines with no trouble at all," and sat down. Well, I don't know whether you know it, or not, but he was the chief opponent to their use. He was the one who guided Roderick Murray. Roderick Murray didn't take a piss without calling Albert Sabin first.

Stevenson: Yes. I know. Albert was, at times--

Hayflick: And Albert hated Hilary's guts, therefore as Hilary's employee he hated me, although I spent a very lovely evening at his home in Washington once. He invited me for dinner.

Stevenson: Well, when he--I knew Albert from Cincinnati days because I started in my career with the Public Health Service. And I was interested, in that time, and assigned the job of looking at the removal of viruses from water and sewage. And Albert was developing his vaccine. So I used to go over a couple times every week to the university and pick up samples of feces in plastic bags that he was getting from his volunteers inoculated with these strains of polio, and I'd bring these back to the lab and process them and then we'd see about the removal of this stuff from water and sewage using different kinds of treatment practices. So, I got to know him then. And then, you know, there was a hiatus. He went off to Israel and so forth, and then he came back, and I had him at Frederick and had to create a lab for him in Frederick when he was testing the Guilio Taro findings with herpes virus as causes of cancer. And we got to be close buddies because I used to drive him back and forth to Washington. So we'd have several hours a day in the car talking about various and sundry things. But he was a real character and absolute--I mean, he was like an Old Testament prophet; *ex cathedra*, when he made a pronouncement, that was it.

Hayflick: There are all kinds of Sabin stories. We could keep each other here for hours and hours with stories of these personalities. But there was something else that I was going to mention in that context that I don't remember.

Stevenson: Well, we were talking about the neural cells and the growth of the poliovirus but, I mean, his authority in pooh-poohing the idea that anything else could grow it put a real damper on--

Hayflick: It held the field back for years.

Stevenson: That's right.

Hayflick: But you say he was unable to grow it in neural cells?

Stevenson: My understanding was--

Hayflick: Maybe it was mouse or rodent?

Stevenson: --was that he himself had had difficulty growing it in neural cells.

Hayflick: It could not have been in rodents, because polio only grows in primates.

Stevenson: And so he'd given up the idea of trying to do anything in tissue culture for a number of years. And it wasn't until much later, with the monkey kidney stuff, that he tried to do things then.

Hayflick: Yes. What we ought to do is sit down for-- Oh, I know what I wanted to tell you.

Stevenson: Hang on until we get a new tape.

Stevenson: There was a group of people who were in Albert's lab at the time and they used to have a--

Hayflick: Chanock was one of them?

Stevenson: Yes. And they used to have a monthly meeting and commiserate with each other about how poor their lot was. It was at that time that I met Ben Sweet and Hennesen, who was there.

Hayflick: Walter?

Stevenson: Yes. He was there in the lab at that time, and Chanock. I forget who some of the rest of them were. But my colleague, Norm Clark, who was a virologist and had been trained at Yale, and I used to get together and meet with these guys and drink beer and so forth. So, my days with Albert go back quite a bit. That's a whole different story.

Then when I moved to the Tissue Bank from Cincinnati, we had been getting viruses from Bob Huebner's lab to investigate *Adenov irus* Type 3, which had been implicated in outbreaks of conjunctivitis traced to swimming pools. And so we were testing the inactivation of those viruses by chlorine and so I knew Chanock and people in Huebner's lab. So, when I got to the Tissue Bank and I was doing autopsies on stillborns, I would harvest the kidneys and send them over to Huebner's lab--Chanock, and Wally Rowe and Carl Johnson--to use for their virus studies.

Hayflick: Are you going to interview Rauscher and Moloney?

Stevenson: Rauscher is dead.

Hayflick: Dick Rauscher is dead?

Stevenson: He died two years ago on New Year's Eve, going across the Tappan Zee Bridge. He had a heart attack right in the middle of the bridge.

Hayflick: Oh, maybe I did read about that. Was that what happened to him?

Stevenson: Yes. Yes, I think it was two years ago.

Hayflick: Actually, I think, now that you mention it, I believe I did know. And John Moloney?

Stevenson: John is alive and retired. He lives in the Bethesda area. I haven't seen him in a long time.

Hayflick: It's too bad you can't interview Bob Huebner.

Stevenson: Pardon?

Hayflick: It's too bad you can't interview Bob Huebner.

Stevenson: Yes. He's just completely out of it.

Hayflick: Have you seen him at all recently?

Stevenson: No. It's been several years and, when I did, he didn't know who I was. Harriet, said, "Oh, Bob, you remember Bob Stevenson, don't you?" and so forth, and he said, "Yes, of course," but it was obvious that he really didn't. His Alzheimer's is pretty bad. It started really about the time he retired because his farewell speech at the luncheon that Jim Duff cranked up for him made it obvious that he was not hitting on all cylinders.

Hayflick: And where is Jim Duff?

Stevenson: Jim is retired and lives in the Washington area. I'm going to get him on tape and try to get some of this stuff down too, because Jim goes back and spans that. I hired him in the last year of the time I was at NIH. He then continued and took over the Human Cancer Virus Task Force Executive Secretary job, and Huebner became Chairman of that after I left.

Hayflick: Well, okay, Robert. I think I'll head back across the Bay and start going through some of that stuff.

Stevenson: All right. If you feel like sitting down with a device like this and just sort of free associating and telling anecdotes, that would be great.

Hayflick: It'd cost me a thousand dollars in tapes.

Stevenson: Well--

Hayflick: When I sit down and start to talk it won't be like you. Where do you--

Stevenson: Where do you have to--

Hayflick: If I told you to do the same thing about the last 40 years, or whatever, you could keep that tape machine going for a couple weeks I think, unless you were given some constraints.

Stevenson: I think that, you know, a lot of these things--

Hayflick: I mean, what I would have to say, most of it, or a lot of it, will be what I'll be giving you.

Stevenson: But, you know, a lot of what is--

Hayflick: Believe it or not, I have "Bob Stevenson" down here, "301-231-5511." That's that. Was that at G.W.?

Stevenson: That was ATCC.

Hayflick: ATCC.

Stevenson: Right. So that's no longer current. And I'm now in New Hampshire, and that's soon to be no longer current. I got an offer today to sell the house.

Hayflick: Oh, good. I guess it's good.

Stevenson: Yes. It's on the market.

Hayflick: Where do you want to go?

Stevenson: Probably back to the Washington area. I made plans to go to Europe, go to Paris, for two months in January and February, but it's too quiet in New Hampshire and Santa Fe was too much out of the way, and I'm not yet comfortable in retirement as to where I feel good. It's like a dog, you know, that circles around a number of times before they finally lay down and rest.

Hayflick: So, give me a telephone number.

Stevenson: Right now it's 603-427-2417. That's Newcastle, New Hampshire.

End of Interview