This is an interview of Mrs. Harriet Huebner, wife of Dr. Robert Huebner, and Dr. Jim Duff, previously of the NCI, taken on July 18, 1995. The interviewer is Dr. Robert Stevenson, formerly of the National Cancer Institute.

Stevenson: Okay. We had a list of questions that Carl Baker had prepared and we can use those as kind of a frame work for discussion, to make sure we cover all those points.

Huebner: Do you have the list with you?

Stevenson: No, I don't.

Huebner: I have a shortened version of the list, if that will do; the five important results and the scientists involved, that was the first.

Stevenson: Before we get started on that, maybe we could go back in time and kind of pick up where Bob Huebner got started with virus cancer type investigations. He was at the National Institute of Allergy and Infectious Diseases and had a Branch, or a Laboratory, consisting of a number of well known investigators, amongst which was Wally Rowe and, in his lab, Janet Hartley, who were pretty well known for work in some of the murine viruses. And up until I knew Bob from the Cancer Institute, I had been at the Public Health Service in Cincinnati and knew him more in terms of *Adenoviruses* and a variety of enteric viruses which his laboratory had been very instrumental in discovering, developing and, in fact, he supplied us with *Adenovirus* Type 3, which we used in disinfection studies and things like that. So, my interaction with his lab goes back well into the mid-'50s when we were getting material from them.

Huebner: This was where? In Cincinnati?

Stevenson: At the Taft Center in Cincinnati, in the Public Health Service. We were doing work on enteric viruses in water and sewage and, at that time, I was also collecting fecal specimens from Albert Sabin's polio vaccine experiments to determine what quantities of *Poliovirus* were in feces, and we were using his vaccine strains in his volunteers as some indication of what you could find in raw feces. So, that goes back a long way. I first met Bob Chanock in Cincinnati.

Huebner: Yes. He was with Sabin's lab.

Stevenson: He was with Sabin at the time in Cincinnati.

Huebner: Right. And that was back in 1953-55, somewhere around there. Right?

Stevenson: Right. Do you recall when Bob got interested in tumor viruses for the first time?

Huebner: I think, as I remember it, the big push. I remember when Trentin talked about *Adenovirus*-12, and he was very interested in that. He was becoming interested. That was the time, I think, that it suddenly hit him that the *Adenoviruses* would be good candidates. They were ubiquitous and they hung around for a long time and they had qualities that some of the other DNA viruses he was aware of did not. So he felt that that would be a logical situation. And then Trentin's observation of the Adeno-12 induced cancer was of great interest. And that was my first-- But I was very new at that point. I came into the lab in about 1957, so I was very new to that. I don't know that he was much involved. He was still writing papers on infectious diseases at that time.

Stevenson: Right. Well, it was right about that time that Charlotte Friend came up with her virus.

Huebner: That's right.

Stevenson: And I remember when I first moved to Washington, in '58, the following year I went to the AACR meeting in Atlantic City and Miriam Liebermann was talking about this agent that they had found in their mice after irradiation and so forth. And I asked her, "Did you ever check to see if it was a virus?"

Huebner: Yes. Oh, is that right?

Stevenson: Yes. She went back home and they said, after that, that that radiation virus turned out to be a virus.

Huebner: Was that the Friend virus that she was talking about?

Stevenson: No. That was the Kaplan. The Kaplan-Liebermann.

Huebner: Oh, that was the Kaplan. Right. Okay. Yes.

Stevenson: RLV, the radiation leukemia virus.

Huebner: That's right. Speaking of Kaplan, I forgot about Henry Kaplan. I'd forgotten all about him. That's interesting. Well, so, at what point would you like for me to start?

Stevenson: Well, your first beginnings, or remembrances, about, you know, the lab getting interested and Bob getting interested.

Huebner: We were interested in that. Soon after that, I think, we became very involved in the *Polyomavirus*. Stewart and Eddy came out with the *Polyoma* and Habel, I think--he was working with SV-40, I guess--but I remember *Polyoma* was another big one. And Bob became interested in the natural… He was immediately interested in what these things do in nature because, in all of the cases where things were induced, they were usually induced crossing species lines, not species lines necessarily, but strain lines, and so the *Polyoma* was the next thing that I remember that really grabbed his interest. We did a lot of work on *Polyoma*. I don't know whether you remember this, but he actually had Murray Gardner looking into wild mouse colonies, looking for the natural sources of virus there, the natural cancer viruses, that they were carrying. I remember, they found there that they did have a strain of mouse that did produce virus, but it had no excess cancer, which gave them the insight that these mice probably have learned to immunologically live with cancer viruses. I think, since then, we've discovered these probably have lots of regulating genes that are very functional. So that was his next thing. He also went to Holland and trapped a whole bunch of mice there looking for *Polyoma*, and he became less interested in *Polyoma* when it seemed that it was not causing what he thought. Let me digress for a little, he was interested in the mouse *Polyoma* because he felt that mice lived very closely with man in the sense that they invaded all of our silos and left their droppings everywhere. We had studies going in which mouse droppings were checked. Do you remember that? That was a while ago. He went to Poolesville and Laytonsville and the silos all over the up county looking for the excreta of mice with the idea that mouse viruses could get into the human population. I guess they did with the *Hantaviruses* later, but we didn't know that. The *Polyoma* became less and less interesting--all of the DNA viruses--became less interesting as he began to discover that somehow there was no excess of DNA viruses in human cancers. And that culminated with the collection of the gold-plated sera, that Sabin had a big hand in, of people with long-standing--I

believe--breast cancer. I think it was long-standing cancers in which they looked for evidence of *Adenoviruses* and they simply didn't find an excess *Adenovirus* experience. And, at that point, Bob decided to drop the *Adenoviruses* as a possibility as an important cause of human cancer in the natural situation, which created a little bit of havoc, I remember, in the field. He was beginning, also, to be interested in the leukosis viruses, the avian leukosis viruses, and the murine C-type viruses. I can't remember what the precipitating factors were there. I'm sorry. I just don't remember that. But I remember that he really gave up on the DNA viruses.

Stevenson: I know he was suspicious about the whole Rous sarcoma virus complex and he dissected that whole thing, much to the consternation of Ray Bryan and people in the Cancer Institute. Because up to that point they thought they'd been dealing with a single entity, and Bob was the first to show that that was a complex of a whole bunch of helper viruses and everything else.

Huebner: Yes, because, yes, the sarcoma genome needed helper virus to be expressed. And I think Rous, as a matter of fact, did all sorts of funny machinations to get expression of his virus, and it hadn't been repeated for many, many, many years, I think, many years.

So, that was…I can't help on going back to the C types. So, we can start at that point, shall we say?

Stevenson: Sure.

Huebner: You asked what the five important results were in my view. Now, I'm talking as a layman. I'm really-- I was very, very peripheral to this whole effort. I just worked in Bob's office. I was his secretary. Then I was an administrative person. And then I was his wife. So, familiarity counted for something, but I'm not a scientist and I think we have to remember that.I think, to my mind, the five important discoveries were: First, the prevalence of animal retroviruses. I think that had not been suspected. And this came very quickly, in a very small period. There were glimmerings of this all along, but I think it was Moloney and Rauscher and Gross, and Friend. We discovered in our lab the xenotropic virus which Levy discovered xenotropic virus, and Gardner eventually discovered the RD-114 virus of cats, which was a xenotropic cat virus, and also there was a feline leukemia virus and I guess that was Essex. I don't remember. Then the vertically transmitted viruses that were in the germ plasm that were defective except when the cells were cultured for very long periods, the CAKR and various other vertically transmitted viruses. I think there it was Huebner, Rowe and

Hartley who did a lot on the vertically transmitted viruses, but I'm probably missing very important people.

Stevenson: Morris Pollard.

Huebner: Pollard. Right. That's right. I forgot about Morris Pollard. Yes.

Stevenson: Yes. He looked at the germ-free.

Huebner: At germ-free colonies. Right, at Notre Dame.

Stevenson: And found that the…

Huebner: And then there were the mouse mammary tumor viruses, the mammary tumor virus and B virus, and I think that Beltzen is the only name that comes to my mind on that one. I seem to recall that there is Type D virus, a monkey Type D virus, but I cannot remember who did that work or what the cancers…

Duff: I think they called it the "foamy agent." I just saw something about it.

Huebner: Is that right? Oh, okay. I haven't read that in months. Okay. I think, along with this, was the spontaneous release of Type C particles from aging cells. That was in the AKR cells where you didn't see any virus in the animal itself, but the cells, the defective virus, was rescued from long-term culture. And then the rescue of *Sarcomavirus* by helper virus. That was very, very integral to the whole oncogene theory. That was very critical. And the description, I don't know whether this was probably a little bit later, but the description of the first exogenous human leukemia virus, which was the HTLV by Gallo. I think that was important. I don't know what sort of happened with that situation, but it seemed to me at the time that was very important. At least it piqued peoples' interest and probably kept money coming.

Stevenson: Didn't the Japanese discover that at the same time?

Huebner: The Japanese did. Yes. I don't know whether it was the same time or--

Duff: No. I think it was in the Japanese people. Yes.

Huebner: Was it? Is that right? It was picked up by the Japanese? I don't exactly recall, I remember that vaguely. But I remember Gallo certainly publicized it and that probably kept the money flowing. Another is the role of the DNA tumor causing viruses. I think that was very important, even though it didn't happen to have a lot to do with cancer. It probably does inadvertently because they probably do trigger, or switch on, the oncogene in some cases. The *Adenoviruses* would be Trentin and Green, as I remember, and I think Ginsberg. I can't remember. It seems to me Ginsberg did some work on that too, and the SV-40. I think Gerardi was involved in the SV-40, and I think Maurice Hilleman. There were other people in SV-40. Habel. Carl

Habel.

Stevenson: Dave Axelrod.

Huebner: Who was that?

Stevenson: David Axelrod?

Huebner: Oh? Axelrod I didn't know at all. I didn't know David Axelrod.

Stevenson: And Bill Hoyer?

Huebner: Yes, right. There were a lot of people working with the SV-40 and the Adeno SV-40 eventually. That was Green also. Then there was the *Polyomavirus*, of course, which was so critical. That was Stewart and Eddy, Gross, Huebner, Rowe, Hartley, and again I think Habel was also involved in that. There was *Herpesvirus samirii*, and that was Krueger. The only person I remember was Krueger, and Ablashi eventually worked with that, but I think Krueger was one of the first to describe that. I don't know who else worked with the *Herpesviruses*. Roizman maybe worked with *Herpesviruses*.

Stevenson: I think Deinhardt did.

Huebner: Deinhardt, right. Then there was the *Papillomaviruses*, and that's Shope, who worked with that, and Beard. And I know there were others who worked on the *Papilloma* but I don't remember who they are. The Epstein-Barr, and I think that was Dennis Burkitt. So, that's the second thing I think is important.The third is the development of tissue culture assays for the presence of DNA tumor viruses, and that includes Dulbecco, Huebner's tumor-specific T antigen, Habel's transplantation antigen, and I think that's the *Polyoma* transplantation as I remember, and the tissue culture assays for the RNA type viruses, which was Rubin and Vogt, Huebner, Hartley, Rowe, Temin, Bishop, Varmus and Baltimore. I don't know whether Baltimore worked in tissue culture or it was just molecular work, but I think he may have. Anyway, he, Bishop, Varmus, and Temin all worked in the molecular side as well, but I think they did some tissue culture work also, or at least the tissue culture test gave them the tools to look for the molecular information. The fourth is the discovery of viral genes in the cell DNA of mice and chickens, and I think that was very important to Bob's viral oncogene hypothesis; the notion of things being transmitted through viral genes in the absence of virus. That and the stochastic occurrence of cancer when you didn't have big epidemics somehow eventually led to the oncogene hypothesis. The other thing associated with that is the identification of specific oncogenes like the *ras, mos, myc*--various oncogenes--associated with virus-induced and naturally occurring tumors. And there I know there were many, many people involved and the ones, of course, that I know best are

Todaro, Aaronson and Baltimore. There are probably 50 people involved in that. The last one is the postulation and confirmation of RNA-dependent DNA polymerase, the reverse transcriptase enzyme, by Temin. That, I think, was put out in his protovirus. That led to his protovirus hypothesis, or was part of it. And that probably was very, very important in subsequent molecular studies. And the rest, I don't think I have a lot. The key administrative management decisions, and who made them, I really was not privy to a lot of that. To me, the key decision was the decision to mount an integrated and comprehensive program utilizing the contract mechanism to enlist collaboration in various disciplines and areas of expertise.

Stevenson: Now, you stated earlier that you had had a function in the laboratory of looking after a number of the administrative and financial things.

Huebner: But those were just with our contracts, just with the contracts that Bob had.

Stevenson: You must have been aware of the need for resources, either in terms of money, personnel, reagents, lab space and so forth?

Huebner: Oh, yes.

Stevenson: So, can you put anything in context chronologically as to how you were able to build up and develop a program of the size that this eventually became against "administrative" decisions or whatever? At one point, Bob switched over from the Allergy Institute to the Cancer Institute because he had more resources available.

Huebner: No. These resources were available to him even before that. The Cancer Institute was supporting him long before he moved.

Stevenson: I well know because my branch was giving him eight positions.

Huebner: Right.

Duff: And about that early time it would have been the T antigens.

Huebner: That's right. That was the tumor-specific T antigens.

Duff: With Ray Gilden out at Flow. That's about the time you're talking about, when he comes over.

Huebner: Right. Bob was sort of insinuating himself into these programs as he needed them and he would need *Adenovirus* reagents, so he got this gal at G.W., Ariel Hollingshead, and a whole bunch of other people, Green and so on, but he was just sort of building like topsy. As he needed things, he would look for people who were capable of making them.

Stevenson: And where did he get the money?

Duff: Well, that was the transition. You know, in your DNA, when you started up the DNA tumor virus area, that would have been about the time when Cancer got into the funding act. And then, when

Bob came over to the Cancer Institute, they made something like maybe a $2 million dollar contribution to NIAID and brought all of his contracts that he had at NIAID over to Cancer. So he came with the Micro contracts, probably a full contract with Ray Guilden, and maybe a few others.

Huebner: Yes, I can't remember. Maybe it was Hans Meier.

Duff: That would have been in the period of like '67.

Stevenson: Well, some of the horse trading and the various things that went on behind the scenes in this whole period of time, I think, are worth at least mentioning or talking about. For example, all of this was done in the context of the Vietnam War too and, at that point, there were an awful lot of young men--physicians--who didn't want to go to Vietnam.

Huebner: That's right. Yes.

Stevenson: And I got a call from Harold Green up at MIT. He said, "I have this very good graduate student," and so forth, and "He has no interest in going to Vietnam, nor to the Indian Service. Would you hire him?"And I said, "Okay. What's his name?"And he said, "George Todaro." And he sent George down. And I got George down to Bethesda and talked to him, and I just asked him straight outright. I said, "George, why do you think you'd be of more use in this program than you would be out in the Indian

Service, and so forth?" And he says, "I know how to do research and I know what I want to do." So, we hired George Todaro.

Duff: Right. And Stu Aaronson.

Stevenson: Stu Aaronson was another. There was a whole group of them that we hired at that time. And the whole business with the Flow Laboratory, and so forth, came about because at that time there was still some thought that a human cancer virus might be isolated. And because Bob had such a tremendous laboratory that was considered to be a factory.

Huebner: It was.

Stevenson: A lot of people were very concerned about his--I don't know if you'd call it "ethics" or whatever, in terms of jumping the gun on stealing people because he could so quickly get to work on a problem and get an answer. Endicott came to me and he said, "You know, if anybody did come up with a putative human cancer virus," he said, "the last place in the world they'd want to send it to see if it matched up with any of the known viruses would be Bob Huebner's lab." I said, "Well, that would be the best lab in the country to send it to, to identify it." He said, "Well, that's true." "But," he said, "I want you to put together a laboratory that has that capability and can identify any potential unknown virus." That was the genesis of setting up the Flow contract.

Huebner: Oh, is that right? I didn't know that.

Stevenson: That's right.

Duff: That was the Flow contract that I was involved with.

Stevenson: Right, and Jerry Kern

Duff: And Jerry Kern, but that was, I think, a second contract that was involved with the T antigens. That was Ray Guilden.

Stevenson: That was later, a little later.

Duff: A little later maybe.

Stevenson: Yes, and then eventually the whole Flow thing came together in one thing and became Bob's contract.

Huebner: Yes, because Bob began to work closely with Ray.

Duff: Right.

Stevenson: But it was so amusing to me because this was one of the things. I must say, Endicott had thought a lot of these things out. One time he called us all in and he said, "What would happen if we got a hot human cancer virus and so forth? Where they hell would we work with it?" We said, "Well, possibly we'd have to go up to Fort Detrick, or someplace like that, in a contained facility." He says, "That would never do." He said, "They wouldn't let us march in up there." So that became the genesis of the planning for Building 41 and a high level bio-containment facility on the NIH Campus. But I must give Endicott credit.

Huebner: Oh, he was a terrific guy.

Stevenson: He was absolutely brilliant, and he could foresee problems and come up with them.

Huebner: And he appreciated Bob. I mean, he saw Bob's failings and he understood them.

Stevenson: Oh, very much.

Huebner: Actually, Bob really never stole anybody's work; it's just that he would get excited about it and jump on it. And I could see why he was frightening. He was frightening.

Stevenson: He was.

Huebner: Because he had a million ideas and he had a million people.

Stevenson: He also, to me, I had never known anyone who could take so much information, factual information and data, and get it on the wing. I mean, like he would have a laboratory meeting and people would come in and feed him data, and he could turn around and synthesize that data just like that. I mean it was absolutely phenomenal.

Huebner: That was his major contribution to science. He was an incredible synthesizer. He saw patterns long before anybody could see them. As a matter of fact, people fought him on that. They'd say, "It's just nonsense." He really did. It was uncanny. He didn't have so many original ideas, but he really, really did know what data meant. He could see all the possibilities. And it was interesting because there were a lot of people who were very, very brilliant and never did that. They never put two and two together the way he did.

Stevenson: One time, when Norm Anderson came up and presented a seminar on the development of ultra-centrifuges at Oak Ridge, he made a plea at the end of the presentation for financial support. And I met Bob after the meeting, and I said, "You know, we--"

Huebner: I remember that. I remember when Norm Anderson came.

Stevenson: "We've got to support this guy." He said, "I'll go to NIAID and you go to Endicott, and ask for money." I went to Endicott, and I don't know if Endicott took my word for it, or called Bob, but I told him, I said, "Huebner was there and," I said, "he's very enthusiastic about it too." And I said, "I think really we ought to support him." So between the two institutes, we got a contract and that was the origins of the ultra-centrifuges in commercial form, getting those made.

Huebner: Where is Norm Anderson these days? He's probably retired.

Stevenson: Anderson? He's still very active.

Huebner: He's in the Midwest somewhere?

Stevenson: No. He's out here in the Shady Grove Science Center. He has a small company with his son called "The Large-Scale Biology Corporation." And he's just developed a machine that will synthesize antisense compounds by centrifugal force.

Huebner: Oh, that's terrific. He's a terrific guy. He was another dynamo really. Absolutely a dynamo.

Stevenson: A real genius. But, to go on.

Huebner: That's really interesting because I'd forgotten. I didn't know I'd forgotten a lot of that. I'd forgotten about Norm Anderson until you mentioned it. I remember that meeting because I remember how excited Bob was by it. I remember that.

Stevenson: Then, at the time, we had various contracts that we were developing in the Resources Program, and a lot of those resources were ones that Bob used as well as a lot of other virologists. But one of the committee members I had was Wally Rowe, and Wally was instrumental in getting us into fluorescent antibodies.

Huebner: That's right. He was.

Stevenson: And we had a contract with BBL and Ted Carski to develop fluorescent antibodies for stuff. And there are stories about that I could tell too that are interesting. But, at any rate, no, we had a very good relationship. Harvey Scudder, who was my boss for a long time, had the highest regard for Bob.

Huebner: He was such a nice man. I remember him. He is such a nice man really.

Stevenson: He was. You know, he listened to him very carefully and he took a lot of what Bob said very seriously, and we built a lot of programs and contracts based on recommendations that Bob had made.

Huebner: I think one of the things that Bob really pointed out to me is that you really need enough money and enough leeway in science for some waste, because Bob would throw out ideas and a lot of them really just were not feasible. They were not feasible or they were not really too logical. But a lot of them were. And you almost had to follow some. He would give ideas up very quickly if they didn't work, so that he wouldn't go on, and on, and on.

Stevenson: Well, evidently he had been the bane of bureaucrats and administrators for years.

Huebner: Oh, because, yes, because he would make sudden curves. You know, he would swerve away.

Stevenson: I was told, for example, one time, by some of the old-timers at NIH, that he had gone out to California to look into Q fever. Now, this was long before your day. But he had gone out there and had covered the entire coast and done, I guess, in about a year's time, what it had taken K.F. Meyer ten years to do, or something. But, when he came back, they said cleaning up the administrative mess after him was something else. He had no receipts. He would go out and hire trucks or, you know, hire people to do scut-work, and this, that, and the other, and sort of had it scribbled on the back of envelopes, and one thing and another, and they said to try and clean it all up was a nightmare. So he was legendary around NIH for being a non-conformist to the Government way of doing things.

Huebner: He's a wonderful man to read about and think about, but he was not easy to work with, even in my situation. He really wasn't. Because he would make appointments and totally forget them. I remember once we had the Nobel Prize Committee man who was visiting with him…

Stevenson: George Klein?

Huebner: No. It wasn't George Klein. No. This was the head of that committee. I can't remember. He's not a scientist whom I knew. He's not in the life sciences as I remember. And he showed up and, of course, Bob wasn't there, so I ended up having lunch with him. I said to Bob, I said, "How could you do that?" And he said, "Well," he said, "he's a busy man too. I mean, lunch is not a big deal to him. It doesn't matter," he said, "And I had this to do." It was just crazy.

Duff: Typical.

Huebner: Anyway, okay, shall we go to the next issue, which is my contributions during this period? And there I have to pass. They were very, very peripheral. I just did whatever Bob needed--Bob asked--of me as best I could, and that was my contribution.

Stevenson: Do you have anywhere a bibliography of his?

Huebner: Yes, I do.

Stevenson: That would be helpful to have along with this, because a lot of the people who have been interviewed we've also gotten their bibliography.

Huebner: Yes, I have his bibliography. The problem is I have only one, and is there some way that someone could get a copy of that back to me, because it's the only one I have? He has 425 papers and he has three or four pages of just his awards and so on.

Stevenson: Sure.

Huebner: Because I sent it out to the family. I sent it to every one of the children and so on. Okay. The next one is main leaders who influenced the field. I don't know whether this included only during the Virus Cancer Program, or just generally. I would say, for instance, I think Rous was very influential inadvertently. Huebner, Vogt, Dulbecco, Temin, Green, Gross, Stewart, Eddy, Gardner, that's Murray Gardner, Moloney, Rauscher, Rowe, Hartley, Gallo, Levy, Todaro, Aaronson, Baltimore, Bishop, Varmus, Lwoff, Jacob, and Zinder for the original phage studies. Now, I know they were not part of the Virus Cancer Program, but I think they were very instrumental in starting this whole field. Okay. The next is membership on key NCI committees.

Stevenson: Before you read that, would you identify any foreign scientists in that group? All the ones you referred to.

Huebner: Well, Lwoff and Jacob are French, and Beltzen, I didn't mention him--I don't know how influential he was--but he worked in this program.

Duff: George Klein.

Huebner: There was Guy DeThJ [?] who was involved in the DNA tumor viruses. George Klein, for sure.

Duff: Epstein.

Huebner: George Klein, and Epstein. I don't know about Barr. Was he a scientist? I don't know. Burkitt really did the work, didn't he? I don't remember.

Duff: Yes. Let's see, the Epstein-Barr work.

Huebner: Did Epstein do any science at all?

Stevenson: It was the Epstein-Barr virus and Dennis Burkitt.

Huebner: Right. I think Dennis Burkitt was the one that did the work.

Stevenson: Who got it tied up with the Burkitt's tumor.

Huebner: Who was the man who had that marmoset colony? Is that Deinhardt? Fritz Deinhardt, I don't know how important he was, but I know he worked at Rush Presbyterian for a while, and he worked with us also. I'm just trying to think of other Europeans. I don't really know. Robin Weiss was involved in mostly carping at us. I don't think he was too helpful, but he's a bright guy.

Duff: Zur Hausen.

Huebner: Zur Hausen, and Krueger, of course. Krueger is with the *Herpesviruses*. I just can't think of any others off-hand. There were probably some more Frenchmen. I think we're missing some Frenchmen, but I can't think of any. There was a woman, an electronmicroscopist, and I don't know good a scientist she was, but she was a very good friend of John Moloney's who inspired him. I can't remember.

Duff: FranHoise?

Huebner: FranHoise Hagenau.

Duff: Right. She worked with Jack Dalton.

Huebner: Right. She worked with Jack Dalton. Yes. Another very nice man, by the way.

Duff: Ito, from Japan.

Huebner: Oh, good. Ito. Yes, Ito. And there are probably a couple of other Japanese as well, but Ito for sure.

Duff: Right. I can see them but can't think of names.

Huebner: There was one, a Kakanaga. Or was that after the fact? That was after the fact, I think. Kakanaga followed that. He was with Gallo. I don't remember the others. Okay, now we're going to the membership on the key NCI/NIH committees and their main contributions. And generally I don't remember them individually so much as I remember they incorporated much of the leading scientific talent in the areas, as well as outside statesman types like Sabin and Lennette. They really weren't contributing so much as they just had potfuls of expertise. They were all privy to the latest developments and tended not to be bound by scientific dogma. It constituted a critical mass of creative thinking. I don't remember individually who was part of it. I remember that Scolnick, at one point, was. And there were a lot of people who contributed who were not necessarily members.

Stevenson: Who were invited in as speakers?

Huebner: Even people like Ray Bryan, for instance, who was not necessarily contributing scientifically, but he had a very broad view of things. Manaker, people like that, who were interested in just pushing the field. And, of course, John Moloney and Dick Rauscher. So, I'm not very good at names. You'll find many people who can help more with that. To me, the significance of quality reagents and their role in the Virus Cancer Program, key decisions were made. I say quality reagents are always the key to scientific progress. This is particularly true in the situation where retroviruses could be endogenous, exogenous, ecotropic, amphotropic, or xenotropic. It was critical to have pathogen-specific animal strains, as well as germ-free colonies. In view of the recombinations between leukemia and sarcoma viruses and the interactions of DNA and RNA tumor viruses, the field would have been a total muddle without the appropriate reagents. Key decisions included the decision to contract well-defined viral and immunology reagents, and that's Flow and Micro and Hazelton. And animal colonies, Hans Meier with his mice, and Merrick and Digg's chickens, Gardner and Essex with cats, and Miller with bovine studies, or bovine colonies, or a bovine herd, and in tissue culture Nelson-Rees and Admore and Hackett. We had chimps. I can't remember who is that in the Southwest?

Stevenson: Kalter.

Duff: Sy (Seymour) Kalter.

Huebner: Sy Kalter's contract. And the marmosets were Deinhardt's.

Duff: Dick Heberling.

Huebner: And who?

Duff: Dick Heberling was with Kalter.

Huebner: Oh, that's right. Heberling, right. He was an immunologist.

Stevenson: Where did all these things come from, these contracts?

Huebner: These contracts all came from the Virus Cancer Program. They were all part of the Virus Cancer Program. Some were larger than others. Deinhardt's, I think, was cut off early because he went back to Europe. I know he went back to Germany. I don't know that we used marmosets that much. But we did use the chimps. One dramatic example of how the program could accelerate progress was the decision to obtain the gold plated defined antisera from cancer patients to determine whether *Adenoviruses* might be implicated in human cancer. Testing of this sera for excessive *Adenovirus* exposure in such patients quickly established that they were not involved which short-circuited further unproductive efforts. Without the extraordinary wealth of reagents we would not have progressed much beyond Rous and it's impossible to imagine the development of molecular genetics. I don't know who was responsible for the "key" decisions but, from my vantage point, it was Drs. Huebner, Moloney, Rauscher, Duff, Sabin and Lennette. Those are the people who I know were very anxious to support these things and changes that would have helped. I'm not knowledgeable enough to speak to that. With hindsight, I would suspect it would have been well to put some more emphasis on *Lentiviruses*. The only people working on *Lentiviruses* in the NCI were Howard Charman and John Doleberg, and both were relegated to the background. Outside groups working on *Lentiviruses* and other slow viruses were not really supported as far as I know. We did study them, but we didn't support them. I just remember the caprine virus and the sheep slow viruses, the caprine encephalitis and arthritis virus, CAEV. There were several of them.

Stevenson: But NIH was supporting the guy in the Neurological Institute.

Huebner: Oh, is that right? I didn't know.

Duff: It begins with "G" I think.

Stevenson: He won a Nobel Prize, didn't he?

Huebner: Oh, Gadjusek.

Duff: Gadjusek. Yes. Carleton Gadjusek.

Huebner: Yes. That's right. Gadjusek. But was the NCI supporting him?

Stevenson: No, no. But NI--whatever the acronym is--NIGMS, was supporting the *Lentivirus* stuff through that, and I think there was sort of a gentlemen's agreement among senior investigators at NIH that, particularly in areas like that which are rather difficult ones, you didn't poach on other people's territory.

Huebner: Yes. That's true. That's true. I didn't realize that Gadjusek was working on *Lentiviruses*. Kuru is a *Lentivirus*?

Stevenson: Oh, yes. And spongiform viruses.

Huebner: Yes. I didn't know that. I didn't know that. I just knew about the animal, the main virus.

Duff: His virus? Kuru.

Huebner: It's a *Lentivirus*.

Stevenson: Yes. He had a whole group of young investigators too out in the South Pacific and stuff. In fact, one of his people tested our first liquid nitrogen shipper and took it out to the South Pacific and sent back brain tissue in it.

Huebner: Oh, really?

Stevenson: Yes.

Huebner: He's a fascinating guy, really. Gadjusek is a terrific man.

Stevenson: Yes. A real character.

Huebner: And, you know, he's very shy basically. I think socially he's very shy. But he's a dynamo in the lab, a dynamo. And Gibbs, I think, is still with him. Is it Gibbs? Joe Gibbs?

Stevenson: Clarence.

Huebner: Clarence?

Stevenson: Yes, but they call him "Joe." That's right.

Huebner: Joe. Yes. Okay. "Did developments in the Virus Cancer Program have significance for molecular biology?" Absolutely, particularly molecular genetics. Most of the technology developed led directly to the molecular search for oncogenes and their subsequent identification as growth factors. Although many of the original scientists involved in the Virus Cancer Program were more biologically and immunologically oriented, they appreciated the contributions of molecular biology and supported the work of molecular people within their own programs and those of contract and collaborating groups. I think one example is Bob and George Todaro. Bob really never did understand molecular biology that well, I think, but he really appreciated George and Stu. In the larger picture, molecular biology and genetics is the foundation of most NCI programs at the present time and, indeed, the rest of the life sciences as well. At least that's my impression. Additional comments. I'm not qualified to make scientific judgments but, as an informed layperson, I would say that science is the most civilizing influence in the world. Confined to the life sciences, the quality of our lives has been improved immeasurably, making possible real fulfillment of man's immense potential. Even a minor distress, like a toothache, can keep us from anything productive. Even where no help or cure is offered by the insights developed, the knowledge keeps us from superstitions and informs us against other absurdities such as quack remedies. The Virus Cancer Program, even when it followed spurious leads and stumbled, made possible some of the most extraordinary and meaningful collaborations of its era. We can hardly overstate its impact since the benefits are continuing to expand. We should all be very proud to have been part of this far-sighted program.

Stevenson: Good.

Huebner: That's it.

Stevenson: Okay. Jim?

Duff: The Virus Leukemia Program. I thought one of the important things was when they brought Bob over to NCI, was that his interest was mainly in what I would call the solid tumor viruses and that was because after your departure there was a hole left in that program that you were initiating on the Solid Tumor Virus Programs and Rauscher decided that the best thing to do was to incorporate all of that into the Special Virus Leukemia Program and, with Bob's interest in this area, they would create a new segment, which became the Solid Tumor Virus Segment, and they changed the name then from the Special Virus Leukemia Program, to the Special Virus Cancer Program, and ultimately the Virus Cancer Program. And then this incorporated all the T antigens for the *Adenoviruses*, the incorporation of Maurice Green and the molecular studies for the *Adenoviruses*. It was a whole new approach.

Stevenson: Well, before I left, Maurice Green had done the *in situ* hybridization with the DNA viruses onto tissues and so forth, and we had a contract with him, if I recall.

Duff: Right. But the majority of the work was probably still done after that. A lot of work had to be done in the building up of his laboratory, which was done by NCI, the supplying of the reagents for him to use from Flow Laboratories, and the collection of the human tumors. This was part of the USC contract, collecting human tumors, sending them up to Walter Nelson-Rees. It was a complete shift from what you might call animal studies into human studies. A lot of Walter's cell cultures had all been involved with animals. I don't think he was doing anything with humans.

Stevenson: Hardly anything.

Duff: And so there was a big change within the Program by bringing Bob into the Program, which may not have occurred if that had not happened. And, seeing the start-up of the USC contract, was when Bob was thinking, you know, not only virologically, but environmentally, and chemically, and it was like practically a mini Cancer Institute within one contract, and it cut across a lot of territories which hadn't probably really been done up to that time. So I thought that was all very important.

Stevenson: Yes. It would have provided an opportunity for one Contract Officer, namely Huebner, then to coordinate and see that all these things were done, whereas before you sort of had to take advantage of people's individual propensities to do this, that, or the other kind of work, and somebody, you know, was capable of supporting it, such as I was, in the Virus and Cancer Program, but I lacked, in that kind of a situation, the authority to coordinate them all. The best I could do would be, for example, things like the Airlie meetings where you would bring people together and encourage them to talk together, but you couldn't force them to.

Duff: Right. And I thought that was one of the great attributes that he had in California, and that was to bring together so many different laboratories out in California--I mean not only USC, but there was Walter Eckhart at Salk, there was Lennette in Berkeley, Walter Nelson-Rees in Oakland, there was Bishop in San Francisco, there were the people up in Seattle, the name escapes me at the moment--

Stevenson: Kenney?

Duff: No. It begins with "H," I think. It'll come to me.

Stevenson: Okay.

Duff: Anyhow, all the way down to the La Jolla group at Scripps. So, that ultimately led to like a meeting probably twice a year, one in Southern California and one in Northern California, called the so-called PCTVR, the Pacific Coast Tumor Virus Group, and Bob was very instrumental in seeing that everybody knew what everybody else was doing--that was one of the things he was interested in--so that not everybody had to collect human tumors. If there was one group doing that, people could share in that. If there was somebody doing tissue culture, like Walter Nelson-Rees, you could send some of your materials to be checked out, you could ask him for cells, and it was a very good working relationship among everybody out there. And then, of course, the annual Hershey meetings brought everybody together, the East Coast and the West Coast. Also, I think Moloney encouraged Manaker to get a number of East Coast people together for similar meetings.

Stevenson: Did they do that?

Duff: And, to some degree, they did. Yes. I don't think it was as strong a group as there was out on the West Coast.

Stevenson: Did Bob have counterpart contracts on the East Coast, or were his mainly on the West Coast?

Duff: I think by that time Bob's were mainly all out on the West Coast, other than the Microbiological Contract and the Flow Contracts.-

Stevenson: Those were direct, though, service type contracts, weren't they, pretty much?

Duff: Pretty much. And then he had the strong interest in a mouse colony up there in Bar Harbor.

Stevenson: Yes. And he had his own animal group at Poolesville.

Duff: Poolesville, right.

Stevenson: So, these on the East Coast, were mainly resource-generating stuff for his own lab, whereas the ones on the West Coast were more…

Duff: Yes, except that was a difficult thing, because they were all included in the Virus Cancer Program. They were treated just like any other contract. And this, I think, ultimately led to some of the problems that the Contract Program faced, and that is that you had certain individuals like Bob benefitting from some of these contracts, and people couldn't understand that. In other words, how one person might benefit by having, you know, a contract. They saw some of the contracts, I think, at Micro, perhaps, benefitting him, whereas when they were back over at NIAID they weren't within a program.

Stevenson: Well, back in the days when, you know, these SVLP segments first got started and I had the reagents thing, Wally Rowe was on one of those committees, and that's when this fluorescent antibody stuff came up. And Wally, at that point, looked upon that committee and that program as being primarily a mechanism for him to get a resource which he could use.

Duff: Right.

Stevenson: Okay? So he brought the thing to the program as a suggestion, that this was something that he needed and could use, and so forth, and when we discussed it in the committee I said, "Well, you know, how much is this going to provide?" And it was obviously a very small amount of material. And I said, "Well, this seems awfully small." And Wally said, "Well, that's all I need." And I said, "Wally, you have to get into your head the idea that this program is for the benefit of investigators, plural, not just your private resource. And so, if we do it, you, in essence, as the price you pay for getting the reagent, are going to have to quality control and research the reagent to make sure it's proper." But I said, "Then it has to be made in sufficient quantity to be made available to other people who could use it, so we don't have to go out and repeat this process all over again." "Oh," he said. It finally came through to him then that this program was not a private resource program for NIH investigators, but basically it was a program for the entire scientific community. And once that understanding, at least, with some of the committee members that we had in those various review committees, got across then they began to see that this was something that was truly a national or international program and not something that was an isolated thing. And I think sight of that was lost, you know, in later times, when a lot of people did have the privilege to negotiate and get things which, in essence, became private resources. And that's where we got into trouble with the Zinder Committee.

Duff: And, of course, it ultimately ended up with…

Huebner: Zinder was a fine one to talk. He wouldn't send his phage to anybody. Remember that? Remember how they got it? They actually ground up one of the envelopes he'd sent a letter in?

Stevenson: Oh, his phage?

Huebner: Yes. He wouldn't send it to anybody.

Stevenson: Oh, I hadn't heard that story.

Huebner: He wouldn't send it to anybody. I'm sorry. You were saying?

Duff: Well, I was going to say that the ultimate end of the Resource Program ended up with the decision in DeVita's time that reagents would be made, if possible, and everybody would pay what they could for them, if they could somehow or other regain a compensation for making them back from both grantees and whoever was using them.

Stevenson: Yes. Those are real knotty problems.

Duff: What I was telling Bob was that when they moved all the contracts over from NIAID, at that time they were, you might say, contracts that Bob initiated, but when they got over into NCI they became incorporated into a program, and people didn't know what the backgrounds of those contracts were; they just saw them perhaps benefitting Bob more than other contracts within the Virus Cancer Program which were tailored differently. And it was rather difficult because I think that's where people felt that he was benefitting more from the Micro contracts, etc.

Huebner: Well, I remember his criticism of the Reagent Program when he got into NCI was that it was a "candy store." Remember that? He said you just sort of provided everything with no rhyme or reason, and he felt that his requests were predicated on certain need.

Duff: The other thing is when Bob came in and, at the same time, Rauscher became the Associate Director, and the Head of the Special Virus Leukemia Program. That was the first time I think that they had a cut. In other words, they had to cut the program--a financial cut--by like 20 percent. And they had to do away with a lot of the reagents. The program that collected serum, the program that collected the human tissues, a lot of that, things that could be prepared or were being prepared commercially, had to stop, other things had to stop, and I think it was clear that Rauscher felt that to support Bob some money had to be carved out of the program. And Bob was well aware of that and so he was always looking for spots where we might carve out some of the program to help support things like the USC. Then this continues. You know, I also remember Bob mentioning, you know, even the work that was going on at Fort Detrick at that time, which was, you know, still the Biological Warfare Laboratories. He used to say, "You know, we could use that money for cancer research." And so there was always the thought in his mind, you know, of where can we carve out some money to continue to support newer things that are coming along.

Huebner: Well, he was always opposed to the Biological Warfare research. He was always opposed to that. Do you remember when he discussed that with Nixon's HEW, I can't remember his name.

Duff: Right. And he was writing letters to our Senator from Frederick.

Huebner: Mathias.

Duff: Mathias. Right.

Huebner: Right.

Stevenson: Well, eventually we built a building up there for him at Frederick when I came back and took on the renovation.

Huebner: Yes. He had trailers before that.

Stevenson: Yes. And then we built a building up there, and so forth. From your standpoint in the NCI program and so forth, what was the importance of the Frederick Laboratories to your program, to Huebner's? Was that a critical element in the development of his overall program to have a lab up there and have those facilities, or was that sort of icing on the cake?

Huebner: Well, he had some germ-free mice there. He had some pathogen-specific mice there. And he used those. You know, it's hard for me to know that.

Duff: Yes. I think it was a greater boon to others within NCI who could move their own laboratory facilities up there.

Huebner: Yes. I don't know that we could not have managed without it.

Duff: Yes. It was a greater boon, let's just say, to the overall part of the biological area within, I think, the Cancer Institute.

Huebner: Because he was operating out of trailers before that, and he liked that better, because he felt that..

Duff: The Poolesville trailers.

Huebner: The Poolesville trailers, right.

Stevenson: Who did he have? He had Patman and Sarma up there. Who else was up there in that lab?

Huebner: Well, the contractor was Fish. The man who was working his program was Donald Fish.

Stevenson: He was one of Litton's employees.

Huebner: Sarma didn't stay there very long. Yes. He was a Litton employee. When was Sarma up there? He was up there mostly because he didn't get along with people so they sort of moved him around just to keep the exposure small at any given place. He just retired, by the way.

Stevenson: Did he?

Huebner: Yes, right. He was with Jack Gruber. Those are two people who really deserved each other. Actually they were both nice enough people; they just don't seem to get along with others too well. No. I really don't know what to say about that, about how much we got out of that. Probably a lot, but I don't know. He did need those defined colonies, and I think NIH was trying to provide them in-house and he felt he couldn't trust those colonies. He had to have his own. Bob would be a little bit uncompromising in ways that he didn't have to be. I remember I mentioned this, that he fought a lot of battles that really were not bones of contention at all. He'd just sort of create a-- You know, he would get a fix on something and there was no shaking it loose. You know? There was not a lot of that, but there was too much. A little bit of that goes a long way.

Stevenson: There were people who were scared to death of him, I know that.

Huebner: Well, he was full of bluster and, you know, I think you can't argue with success. I remember him. You know, he was not a political type at all because he tended to blow off, and that doesn't make a lot of friends, and he said, "Well," he said, "when things really get hot--" I remember when this man, this reporter, from *Science* started investigating the amount of money he had in the program--I can't remember this kid's name, he was a young man, a totally humorless young man--I think his name was Collard or something--I can't remember--and Bob would say, "You know, when I get really frustrated," he said, "I just do my work." He said, "They can't take that away." And in the last analysis, that's how you stand or fall. And that's probably why people were frightened of him. He really did put out. He did a tremendous amount of work. He did a lot of things that, you know, created problems that didn't have to be, which was unfortunate, but I don't think you get very smooth political types who do good science. They just don't even come together. You just don't. You get them driven, the kind of a big ego, you know, and strong drive to show them and those are the people who really come through.

Stevenson: You know, I think…

Huebner: It used to make my hair stand on end sometimes. You know, people would try to help and he would almost turn them off.

Stevenson: I used to, I sort of acted as hand maiden to all these virologists in terms of getting them things to work with, and one thing or another, and people used to ask me, you know, "How do you get along with these people?" and so forth, and I said, "Well, you first have to understand that they are all uniquely individuals and that they all have a pretty high opinion of themselves and their capabilities and," I said, "in many cases it is not unfounded. But," I said, "you just have to basically treat them in some ways like small children demanding an excess of your attention or whatever and just face them up to the fact that everything is not unlimited and you're going to try to get them the best share of what you can, but that they can't expect to have the moon with a fence around it."

Huebner: Well, I learned a lot from both of you, I have to say. That is, there is nothing more frightening to me, or more impressive, than someone who totally keeps his cool and his sweet reason and you both did. You both did that. You know, you always responded in a calm way, in a logical way, and so you sort of watched a "Rumpelstiltskin" act and say, "Well, yes, I really do understand. Of course, you need the moon and the stars, and of course we're going to do our best to get them for you," and that would sort of leave Rumpelstiltskin with his mouth open. He doesn't know what to say at that point. And it's such a nice thing. You know, I remember Bob and I…I can't remember what this was. We never really had any fights. I never remember arguing with Bob. But when we were first married he must have comported himself very differently in his home, and I think he came from a place where you screamed your head off and the one who screamed loudest won, and that was the end of it. You know? There were absolutely no protracted grudges or anything. Well, I never, we didn't do that. And I was just nonplussed by this. You know? He would suddenly start raising his voice and screeching and banging the table, and I didn't say anything. You know? I just thought, and while this was going on, I said, "Well, I'm not going to live with this. This is absurd." And all of a sudden he stopped and I said, "Well?" And he just said, "What happened?" And I said, "Well, you tell me what happened?" And he said, "You know, you scare me so." And I said, "Well, I never said a word." And he said, "You know, when you're really quiet, I'm really scared." He was right You know? And that was the end of it. You know, Bob learned fast. One thing you have to say about Bob is he learned really fast. And actually I wasn't doing this as a technique. I didn't know what to say. I'd never interacted that way anywhere. You know? We just didn't yell in my home. We didn't scream at each other.

Duff: Yes. I think the first meeting that Rauscher, Bob and I had in Rauscher's office, he got into one of these screaming matches and, I don't know, a few days later he called up and said, "You know, let's go have lunch." And it was like nothing had happened.

Huebner: Yes. No, no. He just didn't see this as anything important. And I realize…

Duff: It was like nothing had ever happened. You know? And he come back and calmed down real fast.

Huebner: You know, when we'd have the kids over I realized, you know, there was no harm in any of it except that it's something that, you know, you can either live with or you can't live with.

Stevenson: The one biggest thing I appreciated about Bob was the fact that you knew exactly where you stood with him.

Huebner: Yes.

Duff: Yes.

Stevenson: He was not.

Huebner: He had no guile at all.

Stevenson: None whatsoever.

Huebner: And he had no ill will either, really. He had no ill will.

Stevenson: That's right. And if he had a problem, he told you what the problem was. You tried to get it fixed, and that was the end of it. He never carried a grudge or anything else.

Huebner: And if you didn't work it out, that was okay too.

Stevenson: Every day you started with a fresh sheet of paper and you had an agenda for the day and there were no hidden agendas or anything else with that man as far as I could tell.

Huebner: No, never. There never was. I can tell you. I have been married for over 20 years and, you know, not much of that in a rational way, but there never was. I'd known him for well over 30 years.

Duff: I think one of the interesting things about being in this program was that we were able to see it from, you know, almost the beginning, where it started, until it sort of peaked, and then went through the Zinder Committee or, you might say, the program began to decrease, and went from contracts to grants and so on. It was also a phase.

Huebner: Well, money was a big part of that.

Duff: Money was a big part. That's right.

Huebner: I think as soon as money started being pulled in, you started getting the outside community.

Duff: Right. And that was the very thing that was on my mind, and that is, sometime during that period--I'm not sure whether it was the Cancer Advisory Committee--said, you know, that chemistry wasn't doing it, or the chemical people weren't doing enough, and they should be doing more, and the chemical people said, "Well, if they had the money that the virology group had, they could be doing more," and then you began to see, you know, that the eyes focused on what is virology doing and you began to see some movement of monies, you know, into the chemical end, and then you have the epidemiology.

Huebner: Well, there was a point at which they decided that every cancer was caused by chemicals, that it was environmental, and that cancer was a million diseases caused by a million things.

Duff: Right. And I think about the time of the Zinder Committee, that's when you began to see the people on the outside saying, you know…

Huebner: That was Jeremy Rifkin. That was the name of that reporter. Remember that? The *Science* reporter. Rifkin. Yes, a totally humorless young man.

Stevenson: That hasn't really changed.

Duff: Grantees had more money. You know, they could do more on the outside. And, you know, I think that whole Zinder bit was to, you know, do away with the contracts and put everything into the grant program.

Huebner: Well, again, if grants had all that it needed there would have been very little pressure. They wouldn't want to rock any boats because they had what they needed anyway and they wouldn't care. Because, in effect, they were claiming that we were supporting second-rate scientists, you know, just to boost their own programs.

Stevenson: Well, I think when you come to a political decision that you're going to throw money at a problem, regardless of whether people have rational ways to approach the problem and do something with it, you're going to start supporting second, and third, and fourth-rate scientists because you've run out of good ideas to test. That's one of the problems with AIDS and a bunch of biomedical research. The fact that you have a disease that people want cured doesn't necessarily mean that you know what to do about it.

Huebner: Yes. You know, even intelligent people make such idiotic statements. I have a cousin with MS, and his wife is a very intelligent woman, a really extraordinary woman, and yet she said, you know, "There is no cure or anything even alleviating MS on the horizon because it's not an important enough disease." And I said, "You know, if there was a lead it wouldn't make a difference if only 4 people in the entire world had that disease;

there would be scientists working on it." If they had a lead, they would. In other words, they don't care whether there are 400 or 4 trillion people who have this disease, and she just did not understand that. In other words, it's a political decision not to put money into MS, which is just not true. And you're getting the same thing with AIDS, with the people who are suffering with AIDS. The feeling is that they were being ignored. You know, there is a lot of passion and you can't really argue.

Stevenson: No. You can't deal with it rationally, like these attacks that come from people like Jesse Helms and so forth.

Huebner: Oh, Jesse Helms? He's unreal.

Stevenson: But, when you look at all the money that's spent on the treatment and therapy of cancer and heart disease and so forth, and you add that into the research expenditures, then you get a true picture of what "that" disease, or that group of diseases, cost. But, with the AIDS you've got all the research, treatment, and everything else wrapped up into one number and you can't compare that number with heart research or cancer research and have it be a meaningful comparison. And yet they do it all the time.

Huebner: His point is that, you know, he's back to the witches. I mean, he's saying, "Well, these people deserve to be sick." I mean, who deserves to be sick? Nobody deserves to be sick. It's none of his business how people behave or don't behave.

Duff: Also, it's interesting to look at the big spin-off from the Virus Cancer Program into AIDS research.

Stevenson: AIDS would have been 10 years behind without that.

Huebner: Yes, actually, if Gallo hadn't absconded with that virus it would have been 10 years behind.

Duff: Right.

Huebner: The French would have still been potsying away with it.

Duff: And you look at the people now, you know, what Murray Gardner is doing, and Jay Levy, and Essex, and the chap down at Duke.

Stevenson: Dani Bolognesi.

Duff: Bolognesi, and Gallo still.

Stevenson: Yes, those were all brought up in the Virus Cancer Program.

Duff: They're all part of the Virus Cancer Program.

Huebner: That's right. I think one of the things that happened with the Virus Cancer Program is that it became a little bit too--aside from the money, which was "the" big primary factor--is that it became a little too insular. I think that the old biologists felt very insecure around the molecular people. They really did. You know, Sabin and Huebner and Lennette are not molecular people. They really didn't understand it. And I think that's the way of the world now. I mean, you know, it's like descriptive biology; you can't walk around saying that a rose is put together with this many petals anymore. It's just not enough. And I think these people just didn't bow out and make room, you know, for the whole new crop. It happened so fast.

Stevenson: One thing I wondered about. Was Bob ever associated with Langmuir at CDC in the Epidemiology Intelligence Service?

Huebner: Langmuir? Not, aside from getting…I mean Brian Henderson was the one person he worked with at CDC. I remember Langmuir. Why do we remember him?

Stevenson: Well, Alex Langmuir was the father of the epidemiologists that used to go out all over the country, and sometimes around the world, and investigate outbreaks.

Huebner: That must be it then. But I don't think he ever really worked with him. I think they were probably more competitive than anything.

Duff: And did Brian Henderson come from CDC?

Huebner: Oh, I forgot. You were speaking of that. Yes. he was from CDC.

Stevenson: Who? Brian Henderson?

Duff: Brian Henderson. He went to work with Murray Gardner.

Stevenson: Yes. He was one of Langmuir's boys.

Huebner: Yes. Right. Maybe that's how I know the name, because he got him from Langmuir.

Stevenson: He was a very legendary figure in epidemiology, and he would get these very bright young physicians and put them through a boot camp, and so forth, in terms of going out and collecting samples and learning how to do epidemiology. I worked with one of his protégés Ralph Paffenbarger.

Huebner: Oh, Paffenbarger? Yes, he was a guy that Bob felt very close to. I remember that.

Stevenson: Yes. Well, I worked with him at Cincinnati.

Huebner: Yes. He was a jogger. Paffenbarger was a very physical type.

Stevenson: And he was involved in the Framingham Heart Study, and then he came to Cincinnati and we got in on this virus, enteric virus stuff. And that's when I was getting viruses from Bob and so forth.

Duff: And where is Brian Henderson now?

Huebner: Brian is at USC.

Duff: Is he still there?

Huebner: He left there and he headed up the Salk Institute for a while, and then he left Salk and he went back to USC. It just happened, I mean a little bit ago, not so long ago. Brian was a good guy. He's a very bright guy. But he's very, you know, an epidemiologist, if anybody, has to have a really open mind. You have to go into the field with no preconceived ideas. Brian is a very, very religious

Catholic, and one of the things that, and it didn't turn me off him because I really like him; he's a lovely man, is that he decided that the pill was bad. It was just morally bad. It was just a bad thing. And, you know, the pill is probably "the" greatest scientific accomplishment of man as I see it, as a woman sees it. I'm exaggerating, but it's, you know, very, very important. And he started looking for problems. He discovered the people on the pill had liver adenomas. Well, he never looked at people who were not on the pill. You know, that's not the way to do epidemiology.

Stevenson: No. Indeed not.

Huebner: You know? And it turned out to be nothing, of course. You know, it fell out. And I can't remember what else. But he said, I remember once, he said, "It's the worst thing that we ever did." You know? He said it's just anti-everything: it's anti-humane, it's anti-human, it's anti-God, and it’s anti-everything. And I thought, you know, this is not a man who should go into epidemiology. I think he was a very good doctor. And maybe he got over that, I don't know, but at that point, every time there was a study that came out with his name on it I sort of disregarded it because you don't know how much of it was prejudicial. And he had very strong feelings about religious issues and that's rare. I think that's rare in a scientist.

Stevenson: Are there any other comments that you didn't put on tape with Baker, or whatever, that you would like to add, Jim?

Duff: I can't think of anything at this point. I always go back to this time when--I think it was about the time--when DeVita came in as the Director of NCI. *The Washington Post*, I think, was going to do some kind of an expose, like on the various parts of the Cancer Institute, one being like the clinical investigations, you Know, studies that they had going on. Another one would have been the Virus Cancer Program. And I remember this young reporter coming to me who had been out, you know, talking to various people, and his thesis was the Special Virus Leukemia Program/Virus Cancer Program should never have been started to begin with.

Huebner: Oh, gee.

Duff: And he was quite young. And I remember trying to explain to him, you know, why I felt that it got started. And you had to go, you know, way back into that period of time when you, probably Carl Baker, and other people were around who saw it get started. Now it sort of reminds me of at about that time, you know, the polio work was coming to a close, the Salk vaccine, the Sabin vaccine, suddenly made research on polio seem less important, and there were a lot of trained virologists that were available to go into the Special Virus Cancer Program, just like at the end of the Special Virus Program there were a lot of trained people to go into the AIDS research.

Stevenson: So what, in essence, you're saying is that the Virus Cancer Program was the W.P.A. for polio researchers and that AIDS is the W.P.A. for Virus Cancer work.

Duff: But it just seems that, you know, something comes along to pick up all these people.

Huebner: Do you remember when we used to get crazy calls from people saying, you know, "You people don't want cancer to be solved because that's how you make your living?" I remember getting one of these women on, and this was right after Moloney lost his son, and I said, "You know, people in the Cancer Institute lose people--develop cancer also." And she was just dumbstruck. I mean, I don't know what she thought we had, some magic bullet or something.

Stevenson: Some immunity to it?

Huebner: Yes. She was just dumbstruck. Chanock had lost his son, Moloney lost his son.

Duff: Who was the first one you named? Manaker?

Huebner: No. This was someone who called, an outside person. We got a lot of those calls. They'd read about Bob in the paper and they'd call and say, "Well, you don't want to cure cancer at all." She said, "I know why there is no cure for cancer; because that's your business." And she knew a perfect cure. I can't remember what it was, but it was something, you can imagine.

Stevenson: Herbal tea?

Huebner: And then you got a lot of people who just felt it was a visitation. You know, you blamed the victim. I mean, obviously, the way Helms would do. A Helms classic, I thought. I thought the AIDS was not worthy of him because his real classic was that there was no reason for abortions in rape because everybody knows that a person who is raped never gets pregnant. And this is a man who has an IQ of 85. It just astonishes me. And I understand he knew he lost this last election. I don't know what happened; they must have split the opposition vote somehow. He came very close to losing it. I mean, they should be so ashamed of sending a man like that to Congress.

Stevenson: Okay. I guess, with that, I'll wrap up the tape.

The interview concludes