National Cancer Institute History Project
Interview with Harriet Huebner
Conducted on June 12, 1998, by Gretchen Case
at Mrs. Huebner's home in Rockville, Maryland

GC: This is Gretchen Case, talking to Harriet Lee Huebner on June 12, 1998. We are at her home in Rockville, Maryland. So, I usually just start by asking a little bit about your background: Your education and how you came to work at the National Cancer Institute (NCI).

HH: Well, I've had four-plus years of college, but left New York without a degree. I grew up in New York and I came to the NIH when I married a man who had a job at the NIH.

GC: And so you came, you moved down to Bethesda in, what year was that?

HH: That was in 1953. We were at the University of Chicago before that.

GC: And what department did you start in at the NIH? Were you at the NCI originally?

HH: I was actually with Neurological Diseases and Blindness, and I started as some sort of clerk. I was working for Dr. Frederick Stone at that time in the grants section. It was a job I despised. I left it as soon as I could.

GC: What was so awful about it?

HH: Well, it was just a secretarial job. It was the only thing available at the time. Dr. Stone was a lovely man. I had no problem with that, but it was just not a job I really liked very much.

GC: So, when you left, where did you go from there?

HH: I went from directly to Dr. Huebner. But in between I had a child, so there was a two-year hiatus there. So I came to work for Dr. Huebner in 1957. I started in 1953 until 1955, then I had a break, and then started back in 1957.

GC: So, when you started back, why did you decide to go to Dr. Huebner's lab? Did you know him?

HH: Actually I didn't know him at all. I had a very close friend who worked with him and thought he was wonderful. She was planning to adopt a child and planning to leave, and she wanted to set him up so that he wouldn't be in bad shape when she did leave. And I interviewed with him and he was a charm. I had been interviewing with many people. I remember very well what happened. He interviewed me and when I got up to leave, I said, "Well, I expect, shall I hear from you," or something to that effect. He said, "No, I want you to come as soon as you can." He said, "I'm not going to do any better than you." So, that was very impressive, because everyone else was very polite and said, "Well, of

course, we have a lot of people to interview, and we'll talk with them and let you know." So he impressed me immediately. That was very much in keeping with his character and his personality. He would size people up and size situations up, and his judgment was very good, for the most part. He made some immense blunders with some people but for the most part he really was very insightful.

GC: Do you remember any of those blunders in particular that stand out?

HH: Well, he was a man who never could see any real malicious purpose in anyone–almost to a fault. So he would have people, he had one man in particular, whose name I won't mention, I think people in the field would know him, who came to the lab and absconded with data and actually published on some very hot material before our lab got it out. We were very careful. I remember Bob saying the person whose lab he was in was just furious, he was *furious*. And Bob said, "Well, you know it's too bad if a man has to steal someone else's work." He said, "He can't steal your brain. He's just stolen an idea and you have a million ideas. Just forget it." So, he sort of, he really felt that. He was a very confident man. He had great self-confidence. And it's very nice working with someone who is really confident, who really has confidence. He was just not petty in any way. As I say, he made a blunder with the man whom he invited into the lab. Of course, it seemed that the whole world knew about him and his reputation, except for Bob. I think part of it

was that he just couldn't believe that anybody would do things that were not quite right. It was one of his nice qualities, but it didn't protect him.

GC: So, what was the atmosphere like in the lab?

HH: The atmosphere was, at that time we were in the middle of uncovering new viruses, and there were many, many viruses uncovered in that lab. It was a very electric atmosphere. He had some of the best people imaginable there. He had Dr. Rowe, Dr. Wally Rowe and Robert Chanock and Bob Purcell. All of these men have gone on to become members of the National Academy of Sciences. He was very laissez faire. He just felt that, these people, he would often say, "I have people in my lab who are much smarter than I. I just leave them alone." And he did. They stayed on and on, long after they would have been Branch Chiefs. They hung around because they were qualified and they just liked the environment. He took all of the trouble to get all of the support they needed and they were just free to pursue their research.

GC: So, they were in his lab as investigators?

HH: They were investigators, yes. And they had their own sections, they had their own programs. And it was very, at one point it was very, very large. As a matter of fact, it was a very large lab *always*.

GC: What would large be? Twenty people?

HH: Oh, no, no. It was over a hundred.

GC: Oh, really.

HH: It would go from, you know, I think it was probably about eighty at the smallest, maybe seventy at the smallest, but it was over a hundred at one point.

GC: Was that hard to manage that many people?

HH: He didn't find it hard, because he actually worked with half a dozen trusted associates. And he was not a snob, so he would work with anybody who came through. He would work with, one of his tissue culture men had never finished high school. He was someone who was just kind of a very rough kid. Very bright, extraordinarily bright. When Bob recognized what talent he had in the lab, he just gave him more and more responsibility, and finally got him a GS-11, which at that time was just unheard of– someone without even a high school degree, a high school diploma. It was really unheard of even for someone with just a B.A. or B.S. It took a lot of work. It took a hundred memos, you know. But this was some man by the name of Chick Turner. He was an

extraordinary man. So, as I say, Bob was not a snob. He would recognize talent and go with it. It didn't matter where people had graduated from. One of the people he worked most closely with, Aaron Freeman, graduated from Catholic University, which was great for drama but not great for science. He was a contractor, actually, but Bob worked very, very closely with him. He thought he was extremely brilliant. He really was. So, people really enjoyed working for him. They felt that their reputations were being enhanced and they just, it was very hard to get rid of people actually.

GC: Really?

HH: Yes. He was a very good man to work with. He got along much better with peers than he did with people above him. He tended not to have any patience with people above him. He had a strong ego and he felt that what he said he wanted, he needed, he had to have. He just didn't understand them questioning it on the basis of budget or on the basis of fairness to other labs, or anything else. He just felt that his lab deserved everything he asked for. He didn't ask for anything unreasonable. I don’t know whether that was true or not. But he had no patience with them and as a consequence was always battling windmills over and above.

But with peers and subordinates, he was just marvelous. He was marvelous! He thought everybody he worked with was a genius. [Laughs] And he just, he was just wonderful as an associate. And everybody was an associate, except for his bosses.

GC: Now, when you say his bosses, the people above him?

HH: The Director, the Directors, yes. Now that wasn't true in the Cancer Institute so much. That was true in [the National Institute of] Allergy and Infectious Diseases. When he joined the Cancer Institute, Dr. Kenneth Endicott was the Director and they were *very* good friends. They seemed to be on the same wave length.

GC: What was Dr. Endicott like?

HH: You know, I really never, I never really knew him personally. He was very affable. He was a very strong man. He was like Bob. He didn't question his own decisions and, as a consequence, he had a lot of enemies, and one of them was Mary Lasker, which was not good.

GC: Oh, really?

HH: Yes. For some reason he didn't take her suggestion about using funds in a certain way. I don't really know what happened. I wasn't privy to it and nobody ever explained it to me, but I know that that was a *serious* blunder on Endicott's part. But from our point of view, he was, he left us totally alone. He felt that Bob was the hottest property he had and he was going to support him in every way he could, and he did. Now, he wasn't always, he left at some point, and, as I think, partly because of this altercation, ongoing altercation with Mrs. Lasker, and I think at that time Carl Baker took over.

GC: Right.

HH: Carl Baker, it seems to me, didn't stay for very long either. He was not there for very long. He was a very sweet man. Baker was a very sweet man, and again, I don't really know him personally. I would just see him at functions. And I think Frank Rauscher took over after that. He was a very, very dear man. He was very easy to work with.

At the NIH, when I first joined, there were very few layers between the Director and the Branch Chiefs. The Branch Chiefs pretty much ran the show; the Directors pretty much provided support. As the years went by, there were more and more and more layers between. So, it got to the point where we almost didn't even deal with the Directors. We'd be dealing with the person directly, you know, your Division Chief, and whatever– going through channels even to the Division Chief. And I think Bob found that very

discouraging. He didn't have time for that and he didn't have patience with it. And it's kind of, as I say, he didn't get along well with people who were administratively over him.

He just, I think there was, it was a kind of a, it was just a kind of hostility on his part, because many of them really would have been very easy to work with, or were trying, trying to please him. But he just went in fighting, and that, I thought, was a blunder on his part. He was fighting battles he didn't have to fight. Instead of saying, "please," he'd say, "I need this and I need it now."

GC: And they probably didn't respond well to that.

HH: They didn't respond too well to that, no. And he would say this at a time when we all knew that things were tight, that the Directors were under pressure. You can't go to another Branch Chief and say, "Well, Bob Huebner's better than you are, so he's to get everything and you're to get nothing."

GC: I guess not. Now, Dr. Rauscher was a virologist. So was he easier to work with than any of the others, because his interests?

HH: No, actually not, because Dr. Endicott I think, as I remember, was a cardiologist. He was a clinical specialist. It was either cardiologist or endocrinologist; I can't remember.

GC: I think he was an endocrinologist.

HH: Was he an endocrinologist? He could have been. But anyway, he was in the clinical area. He wasn't in the basic area at all. I think Rauscher was easy to get along with because he was just a very, *very* lovely person. He was just easy to get along with on every level.

GC: And then Dr. Upton came in.

HH: Yes, Dr. Upton. I don't remember very much going on there. I don't remember what his relationship was with him [R. Huebner]. That was not for a long period either.

GC: Right. That was not very long at all.

HH: I had forgotten about him, actually.

GC: Now, when Dr. Huebner came over from NIAID, from Allergy and Infectious Diseases, you were with his lab over there and then you moved with him, right?

HH: Yes. We all moved with him. We didn't all, but many of us did. Yes. I think some of his people became Branch Chiefs at that point and took over, Dr. Chanock took over his lab. Dr. Chanock was one of his protégées. A very brilliant man. He took over as the Chief of the Infectious Disease Lab. Dr. Wallace Rowe was head of some, I can't remember the name of his laboratory, his branch, but he started another branch. So, that was taken over. We did come away with a lot of our people, a lot of our support people.

Of course, then the lab expanded with new people who were more interested in, who were interested in cancer, not so much in viruses but cancer. Of course, at that time, the time we moved, viruses were very deeply implicated in cancer. At that time there was a question of whether adenoviruses caused cancer, and SV-40 was one of the viruses that was suspect. So, there was good reason for cancer specialists to go into the Cancer Institute. I mean, retroviruses were being discovered.

GC: Was there some reason in particular that he moved at the time that he did?

HH: He moved at the time, I think, partly, it was partly political. He just simply was not getting along with the Director. The Director of the NIAID at that time was Dorland Davis, and he felt that Dr. Davis was really a very–he had been a scientist but he was a failed scientist and had actually been in Bob's lab at one time. They had worked together.

He said that they did everything they could to keep him out of the traffic pattern, because he was just not a good scientist.

And that was another blunder Bob made. He felt that if you were no good at science, you would probably be a great administrator. That was his way of getting rid of people. He would recommend people he felt were poor scientists for various administrative posts. Everybody was happy about that, because they were happy they got a promotion and he was happy because they were out of the lab. But it was a mistake. It was a great mistake. Those people who were no good in science generally are no good at anything, or administration either. At least he had a hard time working with them and I think that was part of the reason.

And, of course, he was close to Endicott, and I think it was just that Endicott promised him whatever support he needed. Cancer was a very rich Institute, always, and Endicott said, well, we wouldn't have to fight those battles any more. As a matter of fact, Cancer was supporting him for a good part of the time we were in NIAID. Cancer was giving money to our laboratory. So, it was a natural progression, I think.

GC: What was your role in the laboratory?

HH: Well, at first I was a secretary but, as I say, he would give the secretary, or anybody, anything he/she could do. As a consequence, I found myself doing a lot of writing. At some point I left. I left the lab because I couldn't get anywhere as a secretary. I had taken the administrative test and, of course, had passed it. (It wasn't a very difficult test, frankly.) And there was still no place for me.

So, I went to one of, the science writing in the Information Office, at that time was not contracted out. It was part of the Institute. And very shortly after that, Bob arranged to get some sort of administrative position in his laboratory. He had a big enough lab that he could justify it. The Information Office was really not as interesting as the lab. One of the problems in the Information Office was that everything had to go through many, many clearances. So, you'd be the first to get the story, and I was very good at it, because I actually know shorthand, so I could interview the scientists. I knew a lot of the scientific jargon, and I could take everything they said verbatim, and so I was valuable from that point of view. But then it would go through fifty clearances, you know, wondering whether we should say this much, not say that much, even quoting the scientists directly. As a consequence, you'd find yourself scooped three weeks later, you know, by someone who just casually picked up *Science* or talked to somebody who knew someone working in the lab. It was very frustrating work.

So when Bob came through with this, I guess he missed me, because I had been handling, I had secretarial help, so I didn't really *do* any of the secretarial work. I was handling the administrative aspects anyway. I was doing a lot of the research for him, for his papers, in the library. And so he missed me. He really did. And so I went back. The lab was much more exciting, actually, than the writing. In the Information Office, the writing was not very interesting.

GC: When you say you were handling administrative aspects, what would that be?

HH: Well, doing personnel job descriptions and taking care of visas, putting through the paperwork for visas. These are things normally done by Personnel, but Personnel was very swamped and was not part of the lab. As a consequence, they weren't quite as motivated as we were to move things along. So, I became very expert in personnel matters. We used to prepare our own budgets, so I did that. So, we did, I did just about everything that is now probably being handled in a central way.

But more and more I was helping Bob with his papers, helping to write speeches. I am a writer-editor. That's one of my categories. And I sewed his buttons when he popped them. I did whatever needed doing, really, in effect. And I think that's always a valuable person–somebody who does whatever needs doing, you know, even if it's very trivial.

I remember we worked on a testimony for money for the rubella vaccine, which John Sever at another Institute was preparing, but we were asked to do the justification for Congress. I remember working an entire weekend on that, you know, for maybe twelve, thirteen hours each day. But, of course, we had a deadline. It was very, it was a lot of fun, you know, it was exciting. And that meant getting all of the statistics on the rates of the mortalities and morbidities of the disease, its impact on the economy and so on, and so on. So I did that, and then I wrote the preamble, and he did most of the science writing, and together we sort of hashed together something that was pretty much understandable to a layman.

GC: And so your education, your background, was not necessarily in the sciences.

HH: No, not at all.

GC: So, you picked a lot of this up as you went along.

HH: Yes. I picked it up because we worked very closely. Bob was very excited about what he was doing, and he would tell *anybody* in detail precisely what the situation was with any given project, research project. So, if you came to visit and you expressed an interest, let's say, in enteroviruses, you would know everything there was to know about enteroviruses, as long as you were willing to listen. He would tell you in whatever detail

you wanted, even though you might have no background in virus at all and had made that very clear to him.

So, I did know a lot about viruses. I really did. And I did a lot of research. When he started working in the Cancer Institute, we did a lot of research on carcinogens, going back a hundred years, you know, to see evidence of carcinogens at work. So, I became pretty, I'm not an expert, but actually, pretty familiar, pretty conversant with the field.

Now, everything changed with molecular biology and molecular genetics, and that was, which was obviously the wave of the future. That was much more complex. And for me, I found that difficult. With viruses you have a virus. You maybe have two different strains and twenty different strains, but it is still the same virus and you handle it a certain way. With molecular genetics, the language changed. You know, there's a whole new language. There were just whole facets that I just had a hard time with, and I was getting older, so I just didn't grasp things that well.

And it was, at that point Bob was already not really well. We didn't realize it, but he was depending more and more on his associates and they were extraordinary people. Stu Aaronson was one of them and George Todaro was another. He was depending a lot on his associates. So there was no evidence to the outside world that things were slowing down with Bob, because things were moving very well. He had always operated that way

anyway, except that he would feed ideas. He had, you know, extraordinary ideas. At this point he stopped feeding so many ideas and just let them do their thing, which they were doing anyway. So the lab maintained itself and maintained its reputation.

GC: It sounds like it was a very subtle shift.

HH: It was a subtle shift over a long period, and because he had always delegated so heavily, nobody recognized how different things were. He had always delegated. He always felt confident about that. I think that his great contribution was that he was a very, very original man, but in addition to that he had a talent for recognizing patterns. He would synthesize seemingly unrelated data and suddenly see a pattern, and sort of go off in a direction, assuming that pattern to be correct. His insights were just extraordinary. I mean, his insight, his viral oncogene theory, for instance, was ten years before people really accepted it. It wasn't a virus oncogene; it was actually an oncogene. But he coined the phrase "oncogene."

GC: He did coin that phrase?

HH: He coined it. And his feeling was that cancer was not an infectious disease. He decided that after he had been investigating the adenoviruses and SV-40 with hammer and tongs,

and then suddenly decided that they could not be responsible, because cancer was stochastic and occurred at predictable levels, and it had to be something within the cell.

His first concept was that the DNA of the cell was changed probably through the vector of a virus that was carrying this oncogene, which turned out not to be, well, it may be so in animals, in animals it was certainly so, in chickens and so on. But in the end it turned out it was probably, it was more, and that's when he went to the switched-on hypothesis, where everyone has the potential oncogene in the cell waiting to be switched on by carcinogens or genetic problems or whatever. So he himself changed.

But his concept was very, very original at the time that he proposed it, and it was because, and as I say, he had been investigating adenoviruses and the SV-40, and I can't remember some of the others. He had a whole bunch of people on contract and working with him, and suddenly said, "Forget it. It's not causing cancer." Of course, a lot of people were very unhappy about that, because their whole effort was directed toward that. And he said, "Well, no problem. There's plenty to investigate. Just drop that." Well, they had papers in the works. So, it was a bit of an upheaval. But that was typical of Bob. He would throw away five years of work without thinking twice about it, if it just didn't look right. You know, he had no qualms about that at all. I found that was not really very true in most cases. And most people were working on one series of viruses. He was working on many, many. So, he just dropped that one group and just plunged into another area.

Also, we had the resources. A lot of contractors don't. You know, they get their contract on the basis of working on a given situation. You take the situation away, they have to start reapplying. You can't just move your operation into something else. You know how contractors work. You have a mandate and you have a mission, and all of a sudden the mission is declared defunct, and where does that leave you?

GC: Without a job.

HH: Well, you see, yes. Bob's feeling was, "Well, if it doesn't work, it doesn't work. What's the point of throwing good money after bad, so just change your focus." Well, they just couldn't just change their focus without permission, without getting approval.

GC: What did you think of the whole contracting idea, the idea of having contractors or contracting out certain pieces of work? I know that was quite a controversy for a time.

HH: Well, I think the contract really was a way of extending one's own program. And I could see the appeal to NIH for this. I could see what the objections from the outside scientific world would be: It would be, because their feeling was that if these people have programs, they should go through the grants group. There shouldn't be one man at the NIH who controls $20 million in research funds, which Bob did.

GC: Wow.

HH: Yes. It was a big program. And he didn't see anything wrong with it, because he knew that whatever he was doing was important. So, it was that simple.

GC: So he needed the $20 million.

HH: He needed the $20 million. Actually he needed 40, but 20 was okay. And I can see what the hostility was on the part of the outside scientists. This was money, the money, the pie wasn't infinite, and they just felt this was money siphoned off, that people were struggling just to get, you know, two hundred thousand dollars to keep a technician going and get some supplies. And the contractors who may have been in some cases not the best scientists but were willing to work on protocols provided by good scientists. But meantime, they themselves were not the best scientists, being well supported, *very* well supported. Some of them were the best scientists, but many were not. Many were second-string scientists or young people, and Bob provided the ideas. He or his associates provided the ideas and the protocols and, in effect, they were really technically carrying out the protocols that were being provided from NIH.

GC: Now, would the contractors work on site at NCI, in the lab?

HH: No, no, no. No, they would be somewhere else. We had contracts with universities for the most part, and that's the way we did get some very good people. We had St. Louis University, with Dr. Maurice Green and the University of Southern California, Dr. Murray Gardner.

GC: Wrote this article.

HH: Yes, he wrote that. Now, Murray Gardner is a professor of pathology, and the universities were chosen pretty much on the basis of the men that Bob wanted to work with. He had no particular partiality for one university over another. So, he'd pick the people he wanted, wherever they happened to be. We worked with Hans Meier at the Jackson Laboratories in genetic strains of mice, and with Scripps, with Dr. Frank Dixon at Scripps Institute.

GC: In La Jolla?

HH: Yes. Walter Eckert I think at the Salk Institute. There were people all around, not all around the country but in many parts of the country. And he would just pick people, you know. He'd read their reviews or whatever, read their papers, and decide this was a man that would be really useful, really valuable in the program, and would provide money.

Would not impinge on their own programs. They would, let's say, give 20 percent of their time to the contract, 10 percent of their time that they would oversee it.

So, it was successful because Bob was so successful. He was so original. But you could see where it could fall apart very fast, you know, if he were out of the picture. Then you would have these groups just using the funds to support whatever research they wanted, with no peer review, in effect. The contracts did have review groups, but they were not of the same level as the grant review groups. So, there was a review but it was of a different nature, just in the beginning anyway. I think it tightened up a lot.

GC: You've talked a lot about who Dr. Huebner liked to work with. And I was wondering, when he hired people for his labs or when he brought people into the lab, did you get to sit in on any of his interviews, or did you have a sense of how he would find somebody?

HH: I didn't have to sit in, because he was very, as I say, he was very open. Nobody could ever accuse him of ever walking off with data, because he told everybody everything he was doing. And if someone came for an interview, he would bring him out, you know, and just say, "This is Doctor So-and-So and he's doing such-and-such. We're interested in doing this and that." So, everybody would be brought in on it, not in any formal setting, but just because he would walk around and say, "Well, this is what Dr. Rowe is doing and I think you'd be happy working with him." You know, everybody got a taste of the

interviewee. And the interviewee would feel very welcome. He would feel as though he was . . . in effect, Bob had sort of chosen him before he even came. He would have seen him at meetings and, you know, he would have heard his talks at meetings, he would have talked with him, let's say, at any given meeting privately, after the lectures. And so, in effect, he had sort of decided who he wanted and then would have him come in just to see the lab. The man was interviewing us, in effect.

GC: Was the Cancer Institute considered a good place to work, or a desirable place?

HH: It was very desirable. It was very desirable, because, for one thing, resources were so large that there was room almost to do anything that you really wanted to do. You weren't fighting each other for money. The thing we fought most over was space.

GC: Really?

HH: Yes. Well, that's true in any science lab. Space is the thing that's at the premium. At some, at the Dental Institute, for instance, that's not a problem because they never have enough money, so they couldn't hire millions of people, so the space was a little bit easier to handle. But in the Cancer Institute, they had plenty of money for people. One of the problems was that there was plenty of money; sometimes the positions when we were on

a freeze, the positions would be limited. In other words, you would not be able to hire anybody. But it wasn't for lack of money.

GC: So how much space did you have?

HH: Oh, I don't know. I don't know the, we talked about space a lot but I don't remember. I don't remember what the square footage was. You know, there were labs everywhere. Wherever we could pick up a lab. We would have whole corridors and then we might have an isolated lab on another floor. So, I just don't know. I think that's something the administrative people would know, because there certainly were blueprints and so on.

GC: What building were you in on the campus?

HH: We were in Building 37. We also had a whole building at Fort Detrick, a whole building for doing work with pathogen, specific pathogen defined animals.

GC: And that was kind of a bio-containment facility?

HH: Yes. It was a kind of bio-containment facility. That's right. We had a whole building there. We had trailers at one point. The trailers gave way to this building on Fort Detrick

property. So, we had a lot of space in a lot of places, and that doesn't include the contractors who were providing their space, you know, for whatever.

GC: Now, did you have the space at Fort Detrick the whole time, or was that after Nixon kind of turned Fort Detrick over to the Cancer Institute?

HH: Ah, you know, that's where I lose. We had trailers. We didn't have it the whole time, we had trailers. When we needed to keep animals away from the contamination, we had them in a group of trailers in Poolesville [MD] and that was very successful. It was very crude but it was *extremely* successful. It was actually more successful than the building at Fort Detrick, with all of its very fancy apparatus for air exchanges and so on.

GC: Oh, really?

HH: Bob was opposed to putting all of this into one building, because he knew that there would be cross-contaminations no matter what the air flows were. We had had that experience in Building 7 presumably there was never any air exchange in Building 7. The air came in from the outside, it went out and never re-entered the building. There were experiments with bacteria that proved that that just simply wasn't so. It simply wasn't so. You could put various sensor sites around and find that the same bacteria, harmless bacteria that were released into the air kept returning. So he knew that this was

very difficult. Whereas with trailers you could have an isolated trailer. The worst that would happen is that one trailer would get contaminated but the others would not. You could clean that trailer up and that would be that. We had that for many years.

GC: And would he go out there himself to look at the animals?

HH: Oh, yes. Well, he had people there, but he would go out often. Yes, he did. He visited often.

GC: What was a typical day like in the lab, from your perspective, and then we could talk about, maybe, Dr. Huebner's perspective. But what time would you get to work and what would you do first?

HH: Well, I'd get to work sometime about eight or a little after, mostly just to get a parking space.

GC: Oh, really?

HH: Yes, because parking was very difficult. Parking is always difficult. So, I got there a little bit before the workday began. I would handle things as they came up. A lot of them

were personnel matters. We would handle things as the deadlines came due. If we were working on budget, we would do that, we would work on that.

GC: Okay. So, we were talking about that you would come in a little before eight and then…

HH: Just do whatever the situation of the moment demanded. We had a lot of people, so there were always personnel actions that I could work on, things like job descriptions, and we did a lot of award writing.

GC: Let me stop you a minute. Okay, there we go.

HH: Well, as I say, I would just handle whatever issue the day presented. Usually I had ongoing personnel actions that I could always work on. So, I would do a lot of those before people started coming in. It was just easier to concentrate. We did a lot of award statements, just for getting bonuses for people who deserved them. And then we had lots of personnel evaluations. That became a big thing after Jimmy Carter. And we would work on budgets. I would go to the library and get some papers for Bob. This was, you know, pre-MEDLARS, a lot of it. It was a varied day, a very varied day. And, again, we had a lot of interruptions. You know, we'd have crises that would arise in one lab or another and we would have to sort of see to that. Materials that hadn't arrived, or just whatever arises in a laboratory. And, of course, we answered phones. I didn't personally

answer the phones, but many of the phone calls were things that I could respond to, so I spent a lot of time doing that.

GC: Did you ever see the support staff? The secretaries?

HH: I did. The secretaries, yes. I didn't do things like time cards. That was handled by other people. I would handle whatever personnel problems came up. There weren't really very many. We had a really good staff. But occasionally there would be some. That was not a big part of it. We really had very few. Not much there. The biggest problems we had in terms of personnel were visas. The scientists tended to ignore things like visas that were expiring. So that was something that I sort of undertook as, you know, my own responsibility, just to keep tabs, because everything that went through the State Department took so long and could present such a problem. I can't really think what else. There were just all of the logistics of the laboratory that somehow would filter over my desk. Even if I personally didn't handle them, I would refer them to the people who did.

GC: You talked about visas. Did you have a lot of international scientists?

HH: Oh, yes. Most of our scientists were actually post-graduates from other countries.

GC: Really?

HH: From outside the States, yes. A lot of NIH are post-docs or visiting scientists.

GC: What time would you leave?

HH: Well, Bob would come in later and we would leave whenever he left, whenever we were finished working, together. He tended to be, he liked to work at home. He was an early riser, but he liked to work at home. He would come in sometimes as late as two o'clock in the afternoon, and then he would work until ten o'clock at night, or eleven or whatever. I would leave somewhere, when my son was young, I would go home at a reasonable hour, but when he was grown and after Bob and I were married, I would stay for as long as he did. Sometimes we would come home at a usual six o'clock or whatever and then work at home. Bob was one of these people who never really stopped thinking about the work in the lab. It was really his whole focus, and I think probably part of his success was that he was completely driven. I wondered how he would handle retirement, for that reason, because he just didn't have other interests. He liked sports, but you can't spend your day watching football, whatever. He did play tennis, but you can't spend your day doing that either. It was never a problem. He took sick and that was not an issue. But he was completely focused on his work. Even our vacations were always planned around [professional] meetings. They were always planned around meetings. I kind of objected to that, because we spent the time at meetings, literally. We would take vacation time and

go to meetings, or visit with his various associates. That was the thing that was of the most interest to him. Some of them were in Europe, which was nice. That was fine, but not too many.

GC: Right. Now, did you go to the meetings, too? Did you attend?

HH: I attended a lot of them, yes. I did attend a lot of them. I didn't usually go overseas. I did go to Israel, but not as a participant, so we went on our own funds. He gave a talk in Israel and we stayed there for a couple of weeks, visiting with friends there who he had been working with, who had been in the lab and went back home. And that's pretty much what happened wherever we went: We would visit with people who had studied at the lab, been with us for some years and then went back. So, there were people around everywhere.

GC: You must have known people all over the world.

HH: Well, not all over the world, actually. There were a lot of areas that have really almost no scientists. Certainly nobody in Africa; we didn't have any Africans. We had very few people from France. We had one, one or two, very good friends who worked with him over the years, but very few, which was a surprise. At the beginning we had lots of Germans, but they stopped coming. I don't know what happened. Maybe their own labs

were beefed up, or they were going to England, or whatever. So, we had some countries, we had lots of Japanese, many, many, many Japanese. We had some Chinese. We had lots of Israelis, and lots of Italians. I think part of it is that they bring each other. A man studies and likes the lab and tells all of his colleagues and arranges, you know, so they start writing to see if they can come. They tend to bring each other. So, we would have, it was a very unbalanced group, in terms of worldwide participation. We did have a Czech, we had a couple of Czechs, and we had a couple of Russians. But mostly they were from Italy and Japan and Israel and England.

GC: How was it balanced in terms of men and women? Was it mostly men?

HH: At first it was mostly men. I would say, there were just two women when I first came, two women–I'm talking about scientists, two women scientists–Kay Cook and Janet Hartley. Janet Hartley was a *marvelous* scientist. By the time I left, which was long after Bob had left, we were almost, I don't know that it would be half-half, but it was, you know, women were just being accepted on the same levels, using the same criteria as men, and a lot more were graduating from grad schools and medical schools. So, by the time I left NIH, there were many women–maybe 40 percent in our lab. Many women.

GC: Actually as investigators?

HH: As investigators, oh yes, on that, yes. One assumes there was also a lot of [female] clerical help. These were investigators. And at that point we were beginning to get people from the Middle East, from Iran and other groups. Taiwanese and other groups were starting to come. But that was after Bob was gone.

GC: So, on other levels, not just the investigators, but then, I guess, the technicians. How was that breakdown in terms of men and women?

HH: Technicians, I would say, you know, without trying to, I would say that was almost 50-50. I think that was almost 50-50. Bob liked working with women. He got along very well with women and he felt that, his comment . . . he was instrumental in getting the very first woman executive officer of the Allergy and Infectious Disease Institute, of *any* Institute: Jenny Arliss. And when he was asked why he was pushing so hard, he said because she was terrific. She had been his secretary and, again, she did everything, and she was just more than a secretary. You know, in that era, you couldn't be anything other than a secretary, you couldn't get a job. And he decided that she was the best person for the job and he finally succeeded, and she was. She was *very*, very good. His rationale was that if you hire a man, he immediately hires a woman to do the work. So, we save the slot and we give the job to the woman. She has the prestige and she has the responsibility, and you can put that slot in your lab and get another technician. He said it facetiously but he meant it. He really meant it. And he would point out examples of that, you know. You

would hire the Joe Smith and the first thing he did was he got, you know, Jane Doe to do all the work, and he was having lunch with the fellows. And at that point, it was, actually there was more truth than fiction in it. You know, there were a lot of very good men, but a lot of them really did a lot of just, they all had staff, they all had women staff. So he was right about that. His first administrative officer was, the first administrative officer of his building was a woman, and she, too, had been his secretary. The other ulterior motive there was that she would see to it that he was well taken care of, and she did.

GC: What was her name?

HH: Connie Ludden, L-u-d-d-e-n. She was a wonderful gal. A very bright gal. An extraordinary woman, a very charming woman.

GC: It sounds like he did a lot to broaden opportunities for women.

HH: Oh, yes. Well, he had six daughters. He had six daughters, yes. So, he really did believe that women were completely equal, except in making coffee. He felt that they should make coffee.

GC: Oh, really.

HH: Yes. As a matter of fact, we had one very funny episode. We had a meeting, I can't remember what the issue was, but there was a meeting where only one woman was present. This was about some program that was in the offing. I can't even remember who the woman was; it was some very sweet lady. I think she was from Sloan-Kettering or Rockefeller, Rockefeller Center, I guess. I can't remember. Rockefeller University, rather. And at some point there was to be a break and no one had arranged for coffee. So he sort of turned to her and he said, "Well," he said, "you know, would you fix us some coffee?" It never occurred to him that that was absurd. She was very gracious, she was very gracious. She didn't say, "Well, you know, I'm certainly not going to do anything of the sort." She just walked out and she found one of his clerks who was handling the phones, and said "Would you mind arranging for some coffee?" But I said to him later, I said, "For heaven's sake, how could you have done that?" And he said, "Well, I don't know how to fix coffee." That was a very important factor later on when he did get sick. There was some question about him being in the kitchen by himself. And I said, "There's no danger. He doesn't know how to turn the oven on." He really did not. He never, *never* cooked, ever. If he took anything, it would be just a hunk of bread or something, or, you know, just cold cuts. He just didn't cook. So, he really didn't know how to fix coffee. From that point of view, he was very much a chauvinist.

GC: It sounds like only in that respect.

HH: Well, he did feel that women had different functions from men, and he was very courtly. It would never occur to him that a woman would change a tire, for instance, or would see to a car. I mean, that would be something he would do. And he was very courtly he was very much a gentleman. He really felt that men don't clean house and that women don't mow the lawn. You know, he did have these notions. Well, he is eighty-four now. He was really brought up in a different era.

GC: Did he see those kinds of divisions of labor in the lab? Did he have the same kind of distinctions?

HH: No. In the lab the men made the coffee, because no one liked doing it, no. There was one man who made all the coffee.

GC: Oh, really?

HH: Yes. It was terrible coffee, it was just awful. We discovered later he never washed the pot, ever.

GC: That's probably why it was so bad.

HH: I thought it was bad. Other people loved it. He didn't make divisions of labor in the lab, no, it was just in his personal life. I mean, when we walked into the house, he would immediately go to his desk and I would cook, you know. It would never occur to him to try to help in the kitchen. He didn't shop, but he bought the tires and he took care of the lawn and so on. He would *try* to fix everything that needed fixing. He was *really* bad at it. He was *really, really* inept. So that would be something I would try to keep from him. If something broke down, I would either fix it or have someone come when he wasn't around, because he just was no good at that at all. But he did feel that he should be doing it. So, he was, you know, a mixed bag. We all are, I think. It was never, there was never any unkindness in any of it, you know. If you pointed this out to him, he would sort of scratch his head and sort of laugh and say, "Yeah, I guess you're right."

GC: Did you find it difficult for yourself as a woman at NIH, finding opportunities to move up or to do different things?

HH: Oh, yes. Yes, it was very, it was almost impossible, actually. It was *almost* impossible early on. Yes. You needed to have a sponsor, you needed to have some man sponsoring you. It was *almost* impossible. I mean, you can see that just by the numbers of women who were in positions of authority. There simply weren't any. And when I entered the NIH, I had a degree I had had years of experience. I had worked as a legal secretary while I went to high school and college, so I had a *lot* of experience in the office. And I got a

GS-4 job, which was about as low as you could get, as a secretary, I mean, and nobody cared. When Bob said, "You're the best I'll get," he was right. He was absolutely right. I was competing with people who were fresh out of high school and had had no experience at all. We were all being offered the same grade. It wasn't a matter of innate brilliance. It was just a matter of opportunity, and there was just nothing else available.

Even after I took the administrative test and I passed it with, you know, a very, *very* high grade, I can't remember, it was an easy test, really. It was an all-day test. But, you know, I was an experienced person, I wasn't a kid. And I probably had a better grade than virtually all the men, or many of the men, maybe half the men anyway, and I wasn't even offered a job. I actually had to go beat the bushes for my own job, and that was with *great* references, and, you know, whatever. And I did. I just went out myself and I met with people, and then I had no problem. But it was very, very difficult. It was very difficult. I was working with men who really were not so bright, who had women under them who were, you know, thirty I.Q. points over them. I'm serious. I'm not being funny. We worked with some men who were really just marginal, very, *very* marginal people. Not all of them. Some were really terrific. But some of them were really marginal. They just were. They had gotten a college degree somewhere, you know, and just couldn't function. And if they were smart or if they were lucky, they had some very good gals who did the work for them. And it was a little bit of a scandal, I think. You know, it was a scandalous situation. It probably occurred everywhere, but certainly at NIH. By the time I left, that simply was not so.

GC: Now, was this true, do you think, on the science side of things, too, or was this just…

HH: Well, there were no women in science. I attributed part of that to the fact that women simply didn't get advanced degrees in the numbers that they are now, or anywhere close. Part of that was because women weren't accepted in grad schools or medical schools. It was considered a waste of money to put them through, or a waste of time. So it was a vicious cycle there, and I think, it would be hard, you know, you'd be hard pressed, I remember when there were edicts that went out that you had to have equal representation of women on review committees and we couldn't find women. You know, you'd have to beat the bushes to find women, and you'd find a few. Most of them were maiden ladies who had just gone on for their Ph.D.s because there was nothing else in the offing and they were interested and knew they'd have to have a career. You know, you just didn't have the same caliber who were popping up the way you do now, because you didn't have the same representation. It was pretty bad, actually, when you think about it. You know, we didn't think about it at that time. You sort of live in the context of your own time and the prevailing modes of operation, but when I think back, I should hate to think that a young woman would ever have to live with that situation again.

GC: Well, it's something I'm really interested in, because in any historical document about the NIH or NCI, there are *so* many more men than women.

HH: Oh, yes. Well, it's not an accident, it's not an accident. It's not because women aren't capable. It's because they weren't there. They simply weren't there. We did have Janet Hartley and she, too, was a maiden lady, but happened to be a very, very bright gal. An extraordinary scientist. And beautiful. Actually a beautiful woman, *beautiful* woman, oh yes. She looked like Ingrid Bergman. She was just a gorgeous woman. But she was really, *really* bright. Really bright.

GC: Now the technicians, just to go back to the technicians for a minute. Would the technicians…how much scientific training would they generally have?

HH: Mostly they had B.A., B.S., yes. They would have mostly bachelor degrees. We did have several, maybe a lot, who had high school diplomas who had just come up through the ranks because they just worked in the lab and were very good, and they were just picked up and promoted. They were never promoted to any professional level. They'd go to the GS-7, GS-9. Most of them, I think, had B.A.s and B.S.s.

GC: So you couldn't go from being a technician to an investigator necessarily.

HH: Not without a Ph.D. No. I mean, theoretically, you can. In the Civil Service presumably experience will count, you know, some amount of experience will make up for your

college, so maybe four to one or something, you know. I don't know, I don't know what ratio they used. But practically it didn't work. There was virtually nobody there who wasn't a Ph.D. or an M.D. who was in charge, you know, who had his own program.

GC: I wonder if any of those female technicians might have, in a different time, been investigators.

HH: Oh, I think, I think so. Females and males. You know, a lot of the males didn't go to college or didn't go beyond. They just, they didn't think in those terms. That wasn't so, you know, it wasn't so common then. Going to college wasn't so common. You know, I think I was the first generation where people went to college routinely.

GC: That it was more expected…

HH: But many of my friends did not. I had some *very,* very bright friends who didn't. My closest friend that I went through high school with was a girl who came of a Lithuanian family and she had the highest I.Q.–in those days you used to have I.Q. tests and they told you what your I.Q. was–and I think hers was like 151. This was a really, *really* brilliant girl. Her parents did not send her to college. I mean, even though she could have gone free of charge in New York, they just didn't see any point. They just thought it spoils people. So, they were just going to see to it that she married well. I don't know whether

she did or not. I left before and I lost contact with her. I mean, that was an extreme example, as she was an extraordinary girl. But a lot of people I went to school with through grade school and high school did not go on–a lot of women didn't. But I would say virtually all of the boys did. You know, it was an academic high school and I think virtually all of the boys did. I'm sixty-nine now, so I'm sort of just starting that era. I think within ten years everything had changed.

GC: Now, you said that you left the NCI much later than Dr. Huebner did.

HH: Yes. I worked with one of his, one of the people who had been in his lab that took over.

GC: Was that Dr. Aaronson?

HH: Yes. Stuart Aaronson. He was the Branch Chief then.

GC: He took over from Dr. Huebner.

HH: Yes.

GC: Okay. So, was it essentially the same lab then?

HH: No. It had changed character. It then became the Laboratory of Cellular and Molecular Biology and the whole focus changed. Stu was an M.D., is an M.D., but he never practiced. He was a molecular biologist and never was interested in any of the clinical aspects at all. And the whole lab just went that direction, which I think probably was very appropriate. You know, I think that virology in its simplest form just is not adequate any more.

GC: Did any of the same people stay on in the Lab through this change?

HH: Yes. A lot of people stayed on. A lot of people. Of course, we had a very good lab. By the time Bob left, Stu already had brought in a lot of his own people. So it had already been changing. I think it was a pretty easy transition actually, and the people who didn't like working with Stu had already arranged to go other places.

GC: So, when did Dr. Huebner leave?

HH: He left in 1968.

GC: 1968?

HH: No, I'm sorry. He left in 1982.

GC: 1982.

HH: Yes.

GC: Okay. Did he leave because of illness?

HH: He was sixty-eight. He left, I would say, yes. I think he probably should have left a year or two before that, but he did leave because of illness. He had a five-year appointment. It would have to have been renewed, but he really was not interested in renewing. He didn't really, at that point he couldn't really continue. He was in the Public Health Service for many, many years, until he reached mandatory retirement age at sixty-four or sixty-three, I can't remember, and then he stayed on for five, well, he actually left a year before the mandatory retirement, and then he had a five-year appointment in the Civil Service.

GC: Okay. The Special Virus Cancer Program, I guess…

HH: That was gone by then.

GC: It was gone by '80?

HH: Yes, somewhere around there, yes. A lot of the programs remained, you know. A lot of the research continued, but I think the program as a formal entity was over.

GC: And he was, there were four Chiefs in that program. Is that right?

HH: You mean in Special Virus Cancer Program?

GC: Yes.

HH: Manaker, Ray Bryan…

GC: I have Manaker, Huebner, Todaro, and Scolnick.

HH: Yes, right. That sounds right. Yes. I guess Bryan was not a Branch Chief, was not one of the founders. Scolnick left at some point and went to Merck.

GC: Right. He's someone, I'm going to interview him later.

HH: Yes. He's a very, very bright guy.

GC: Yes. Did you know him very well?

HH: I personally didn't know . . . I knew him well enough, but we were never friends, so I didn't know him from that point of view. He was a very, brilliant man. He was not a toughie exactly, but he was not an easy man, not an easy man. He was a good guy. I mean, he wasn't mean. He was just not an easy man to just have an interchange with. He lost patience easily. He would become exasperated with the powers-that-be and let it be known. He was very outspoken. So, you know, I never knew him on a personal level, except that we worked with him for, you know, and Bob thought the world of him. Thought he was just a genius. He thought he was really bright. Apparently he is. He has been considered a genius at Merck as well.

GC: It sounds like it.

HH: Yes. He's a very big man, very tall.

GC: Oh really?

HH: Maybe six-four, something like that.

GC: Oh. I might have a hard time talking to him. I'm five-two!

HH: Well, I think it will be better now, because I think that he had back problems and he was always in pain, and I think part of his gruff exterior was that he was always in pain. He finally at some point had a spinal fusion or had some major, major back surgery. And that seemed to have resolved it. But I remember for most of the time that I knew him, he was suffering. He would come to meetings and stand up, because he couldn't sit.

GC: Oh.

HH: So, he was just always in pain. But it didn't seem to stop him. I mean, he was a real driving scientist.

GC: Now, was he part of Dr. Huebner's lab, or did he have his own lab?

HH: He had his own lab. And he was a molecular biologist also and I think he, too, is an M.D. Probably has never practiced.

GC: Right. So, was there a lot of interchange between labs? A lot of work together between labs?

HH: No, I wouldn't say so. No. There was a lot of rivalry, actually. There was a lot of respect between labs, but there was not a lot of interchange. No. Occasionally there would be,

but I wouldn't say that was, that wouldn't set it apart. You know, people collaborated really with the people working within their own groups. Bob really didn't collaborate with people so much as he was sort of in charge. He would collaborate in the sense that they were associates, their names would be, they would be on their own papers and he would not. So from that point of view, they were true associates. But they were people who were working on his programs. They were not people working on their own programs, for the most part. There were exceptions.

GC: Now, George Todaro is another one. Was he also in a separate lab?

HH: He was in a separate lab, yes.

GC: He also knew Dr. Huebner.

HH: Yes.

GC: And I take it Dr. Huebner thought well of him, too.

HH: Yes. Oh, yes. He worked very, very well. I think he came in as part of our lab, as I remember, and then, yes, as I remember it, he came in as part of our lab. He was one of the co-authors of the viral oncogene theory or hypothesis. Actually, Bob clued him in on

it. George agreed with it and ended up writing the paper, because Bob hated writing. He hated writing. By the time he had something worked out, he lost interest in it and he just wanted somebody else to write it. So, George ended up as a co-author, but the concept was Bob's. But I will say that George did agree with it and he did a lot of experiments to show it. He, too, is a very bright guy. He was a very different person from Bob. He played everything close to his chest. Bob played *nothing* close to his chest. You knew exactly what he was thinking when he was thinking it, and what he was finding in the lab. Whenever he learned something, he told everything to everybody. George told nothing to anybody. So it was a very strange liaison. But they had a deep respect for each other.

GC: And then Manaker? How did they know each other?

HH: Manaker was a friend of John Moloney's. Manaker is a very, very gentle, very dear man, who was just not a great scientist. But he was a very sweet man. Bob had no patience with him at all. He liked him, but he had no patience with him at all. He sort of treated him like a retarded child, actually, which is not very nice, but yes. But Manaker is a very, very nice person, and should not ever have been in charge of the program.

GC: How did these four end up in charge of the [SVCP] program?

HH: I think John Moloney put this together. John Moloney was the Division Director, and he put it together. He had his pets. Manaker was one. Of course, Manaker was very pliable. Bob was part of this as were Todaro and Scolnick, because he couldn't exclude them. They were the Wunderkinder and there was no way to exclude them. It would be sort of a physics without Einstein, just pretending he never existed. They had to be included. But he really didn't like them very much. He had a very, he felt very uncomfortable with all three of them. So, he had his own people he worked with. He worked with people like Manaker and Ray Bryan and Jack Gruber, people who were sort of more administrative. And all of them very nice people, the kind of people you'd want for dinner. Very, very sweet, easy. You'd feel very comfortable being with them. Moloney himself was a very moody person. He sort of vacillated from being a really good guy to being someone you didn't want to be too close to.

GC: Yes. I don't know that I've ever gotten the whole story, but he left very abruptly.

HH: He left abruptly. I don't know what happened there. When the virus cancer program came under attack, he was obviously at the helm, and he was easily offended also. I think he may have just had his feelings hurt profoundly. He was a very, a very temperamental Irishman, who was really delightful when he was delightful, very charming. He was interesting. He got along really well with women. He was wonderful with women. He

just couldn't get along with any man at all, with the exception of those who were very passive.

GC: That's interesting.

HH: Yes, it was interesting, because, you know, I liked him. My husband couldn't stand him.

GC: Did that happen often that you would like someone that Dr. Huebner wouldn't?

HH: Well, it wasn't an issue really, because he was not, you know, we never had him to the house, for instance. He never became, it was never a personal issue. No, it didn't happen often, because Bob really liked everybody. He even liked John Moloney, except that he had no patience with him. He just felt that he shouldn't be in science. He should be in administration, he would say. He just had no patience with him. When he had to work with somebody with whom he had no patience, then he was not very kind about it. He just wanted you out of the traffic pattern, he wanted you to do anything you wanted, but not here. Really! He put it that way. He said, you know, I'll help you, I'll help you get a job, whatever, I'll give you a couple of slots, he said, but I want you out of here. And so that's not terribly endearing, actually. Even though he would help people, he would humiliate them in a sense. You know, that's pretty humiliating. And I think it's hard to do.

You know, he would never *say* that, but it was something that he was so open, it was hard for him to actually keep that hidden. His manner was so open. He was a man with no guile. My husband had no guile. And that was part of his charm. But in some ways it did not serve him well. And he would talk often when he should have been quiet. You know, there were many things that you don't like and you just don't say anything, because, you know, they'll pass, or they're not that important. But he just couldn't. He just couldn't do that. There were many situations where we had, I think, we never had a fight, but I think the only time we had some disagreements, you know, is when I really disapproved of the way he would handle some things. You know, while a man may be a stupid man, or he may have done something terribly wrong, but you just don't say it that way. You just don't say, "You're a dumb jerk, stop doing that." We all do stupid things, you know, all of us. We all do things that we don't really want to think about.

GC: Yes, yes.

HH: And I think he made a lot of people nervous, too, you know, so you tend to blunder even more when you're with someone like that.

GC: Did he make them nervous because he was so famous, or so smart, or because of his manner?

HH: Because of his manner, I think. It was his manner. I don't think he ever thought of himself as famous. His ego was such that he felt that his science was very good. He *knew* his science was good, and it really was. He's never had to retract anything, and his science *was* good. Often he lost out because he wasn't positive about something and someone else would scoop him. So, his ego was more directed to that. But he never thought of himself as very special. He really thought that many of the people who worked in his lab were more special than he. He thought that George Todaro was and that Jay Levy was, one of those who is now working in AIDS in San Francisco, he thought he was a brilliant young man. He thought Wally Rowe was special. He did think a *lot* of Janet Hartley. He thought all of them were really better than he, technologically and in originality. He saw himself as a good scientist. He didn't see himself as a genius at all. And in a certain sense, his biggest contribution, or certainly a *major* contribution, was that he could see patterns, that his insights were profound, even when it didn't include his own data. He could just see a lot of, for instance, in the carcinogenesis picture, it was after going over the literature that went back to the chimney sweeps, you know, went back a hundred years, that he decided that cancer was not an infectious disease. It was based on that he started doing experiments to show it, that it had to be stochastic, it had to be something within the cell and not something that you were catching. It was not a catching disease. I mean, no matter how one would acquire an

infection, it just wasn't, no matter what the animal study said. So, he learned from everybody. He learned from everybody.

GC: Do you think that ability to recognize patterns was what did make him so famous?

HH: I think so. I think it's what led to his very good research. I think it's recognizing the patterns and knowing how to follow up on them. So, he could sort of see things years before others saw them and plunge ahead, based on his belief in those patterns. He used to say that, you know, you wouldn't try to get to the moon if you didn't hypothesize it was there, even if you didn't see it.

GC: That's good.

HH: So, you have to sort of go on your insights, and his were very good. His were really very good.

GC: What did he think of the Special Virus Cancer Program? Did he think that was a good idea?

HH: He thought anything that gave him money was a good idea.

GC: Really?

HH: Yes. He had no problems with it, he was a very pragmatic person. If the program provided money. He thought that some of the people in it were absolutely inept, he didn't agree with some of them at all, but he didn't care. As long as the resources were there for his programs, he would support anything. He was not very fussy about that. I think that's not such an uncommon science attitude. I really don't. I think there were programs there that were just nonsense. There were some programs that *appeared* to be nonsense. You know, I'm not speaking as a scientist, but I knew from his viewpoint. He just thought they were naive. And even there, he would get information from them that the investigator himself would not see, that Bob would see.

GC: Do you remember any of those in particular?

HH: I'm trying to think. I think one was in genetics, and I'm trying to think what it was. I can't. I remember there was one program that was run by Ray Bryan, and Bob thought it was a noodle-headed program. But there was some data that came out of it that Bob thought was very interesting. Because he was a good, the science was good. I mean, you know, it's sort of like cooking up a simple meal. The meal was okay, but it was simple and maybe not worth doing, not worth cooking. But on the other hand, he would get

information from it, and he would glean more from it than the investigator might. But it wasn't worth the money that was spent on it, was his feeling. Or the personnel, the staff.

GC: This is tape number two with Harriet Huebner. Okay. How did he handle the criticism of the virus cancer program? Was he at all involved in handling that criticism or did he react to it all?

HH: He defended it. Bob was always very loyal. He did defend it and he also felt it had been very useful. He really very strongly felt that if you found anything, if you found anything, even if only one-twentieth of the program paid off, it would accrue to everyone's benefit. I remember one time when I asked why he didn't practice medicine. He was an M.D. And he said, "You know, if I were in pediatrics I would maybe cure a thousand children. If I make one vaccine, I could save millions of children forever from that disease." I think he felt that about science also, that if only one little lead paid off, it would pay off in spades forever. So, he didn't feel that a lot of it was waste. But he was never really subjected to the limits that outside scientists had been, in terms of funding, and I can see their problem with it. They, too, were working very hard on a shoestring often. So, there was a problem with the funding of the virus cancer program, I think.

GC: What did you think was the most valuable thing that came out of the virus cancer program?

HH: Well, I think a lot of things. The adenovirus, the debunking of the adenovirus and the SV-40 viruses as cancer-causing agents. The genetic strains of mice that were developed. The discovery of polyoma was another one that was suspected of perhaps causing cancer. It seemed to cause cancer in animals. There was some chance that it could have been transmitted to humans. There were big studies done on that to disprove that. There were a lot of negatives. But a lot of of Bob's oncogene work was done under the auspices of that program. Scolnick's work, he'll have to talk to you about it himself. And Todaro's work was under that program. These are brilliant men. Stu Aaronson's work was done under that program. A lot of very good programs. Maurice Green's work, Murray Gardner's work. These were all very, very renowned people. And their work was in the National Academy of Sciences publication proceedings. There were a lot of very, very good papers that came out of this. There was probably a lot of waste, but I think a lot of very good stuff came out, because they had a lot of good people. You know, these people, some of them were the leading scientists in the world, and they were recruiting other people who were that good. So, it was a useful program. I don't know that it was more useful, let's say, than if that money had gone to outside investigators who were equally renowned. You know, I think probably we would have gotten the same results. It supported a lot of very, very extraordinary people.

GC: Do you remember when the National Cancer Act was passed in 1971?

HH: No, I don't remember that. I was not really very involved with that.

GC: Okay.

HH: That was under Nixon, as I remember.

GC: Right.

HH: Yes. Was there some special question about that?

GC: I was just wondering, because it changed the way the NCI got money. Basically, they bypassed Congress to get their budget.

HH: Yes, right, exactly.

GC: So I was wondering if that affected day-to-day operations in your lab.

HH: I don't think so, really, because they always had been well endowed. I think it expanded, you know, made expansion possible, but it didn't really affect us. I remember one scientist; we frequently got these notes down from Congress saying, you know, "Tell us

how you would manage if you were cut by 5 percent, by 10 percent, by 20 percent." This is a typical exercise of Congress and then it kind of filters down to the agency, saying, you know, "What would you do if?" And I remember one scientific wag saying something like, "I would do exactly what I'm doing now." It was probably true, unless it's cut to the bone, where you're losing technicians and you're losing supplies, it didn't make any difference. You simply did not put maybe ten thousand animals on. You'd put five thousand on, whatever. But you'd do what you're doing. Scientists work with what they have, and they're very, very creative about it. They're a wonderful group to work with, really.

GC: Scientists?

HH: Yes. They're lots of fun.

GC: How did they compare to working with lawyers?

HH: Ahh. There's no comparison.

GC: Because you said you were a legal secretary.

HH: Oh, yes. I was. Well, I only worked with one law firm, so I can only, and at that time they didn't have law firms of a hundred people. This firm had four people. But they were just not in the same ballpark. Lawyers are fine. They're just much more into precedent and into things that have been rather than things that one has to create himself. Different type people entirely. I don't know. I would say scientists are a lot more fun.

GC: How so?

HH: Well, I think, for one thing, I think they're very sharp. I think they're very sharp and as a consequence they're very witty, a lot of them. They also tend not to follow, not to follow pathways that have been, the normal pathways. And I think what's interesting about them is that they don't even know they're not following these pathways. They're not doing this to be iconoclastic.

GC: Right.

HH: A lot of them have been rather unhappy kids, you know, because they never really quite fit. My husband certainly was one of those, didn't quite fit. And all of a sudden, they're riding high, because they can do it on their own. As a kid you can't. As a kid you have to fit in somehow.

GC: Right. Now, the work being done in your lab was very basic sciences. Was there any connection with the clinical center or with the clinicians?

HH: Not really. There was one toward the end when Bob was trying to work out a test for cancer susceptibility. He was working with colon cancer patients, the colon cancer families, familial polyposis and Gardner syndrome, and I can't remember some of the others, Fanconi's disease, and Fanconi's anemia. And so, what he was doing was, he found that taking, growing out cell lines of susceptible people and putting transforming viruses on them, SV-40 on them, the cancer susceptible skin lines, cell lines, were transformed very, very quickly and easily, with much greater efficiency than normal cell lines, which were very resistant to transformation. Transformation means turning them into a cancer cell, switching them on to a cancer cell. So he had a program where we worked with colon cancer families, getting skin biopsies from the arm, just taking a biopsy, growing out the skin, and then putting the SV-40, introducing SV-40 and seeing what sort of reaction there was. And it was pretty effective in predicting who was likely to get the cancer. Just at the outset we took it from people we knew had the cancer and we knew didn't have the cancer, and you could almost predict from that which of those who didn't have the cancer, would. He dropped that at some point, for an interesting reason. His own skin, he has no cancer in the family at all. He has one cousin who died in his late eighties of cancer who was a *very* heavy smoker, died of lung cancer. He may have been eighty-nine years old. But Bob came of a very large family on both sides. His

mother and father, both had, I don't know, maybe a dozen kids. He himself had a large family and there was no cancer, at all. At all. So this is a resistant family. His skin, we all took our own biopsies. We weren't going to ask people to do this without doing it ourselves. We all took these biopsies. His cells transformed with such efficiency, he was used as a positive control.

GC: Really.

HH: At that point, there was something so freaky about it that. He did it many times. He did it maybe three or four times. Subsequently we found that people with Alzheimer's have this. So, that was used, and as a matter of fact, I have the reprint on it, that was used to test that particular possibility, and it turned out to be absolute. People with Alzheimer's, and who are likely to get Alzheimer's, because Bob was absolutely well at the time this was done, transformed. But at that point we dropped it, because he knew that he just was not a cancer-susceptible person. He was just very, very unlikely. And there were a couple of others, I think, but his was the most dramatic. His skin transformed *so* efficiently. I mean, it was, it was kind of startling. And it did it again and again. Three separate tests, I think.

GC: Do you remember his reaction to that?

HH: He didn't have a clue. He didn't have a clue. He just felt that somehow he was barking up the wrong tree. He just didn't have a clue. Because he had no evidence of Alzheimer's in the family, he had no idea that this was true of Alzheimer's, you know. He didn't know anything; nobody in his family had lived that long. His parents had died young, and, you know, he just didn't have any evidence of that, that he was aware of. Alzheimer's is a pretty new word, really, you know, when you think about. He has had it for sixteen years. I think twenty years ago nobody ever heard about it, really, you know. Of course, you wouldn't remember that, but I don't remember ever hearing the word and I was at NIH and surrounded by doctors. I never heard the word Alzheimer's. So, it's pretty new. It was senility; it was called senility at that time.

GC: Sixteen years is a long time.

HH: It's a long time. He's been sick a long time. Yes. At the point that he was diagnosed, they said it was probably at the intermediate stage. So, it's been a long time. But that was an interesting thing, because it did give rise…now there are tests that are much easier, just blood tests and saliva tests, and I don't know what all else, so nobody is using a skin test, because, for one thing, it's invasive. You have to take the biopsy, and then it takes a long time to grow the cell line, you know, the cell lines, so it's not an efficient test any more. But at the time it was very, very, it was *very* efficient in predicting these genetic diseases, and it may be it's an efficient test for any genetic disease. But the assumption is that this

cell transformation is the equivalent of cancer. That's what happens in cancer. It's not what happens in Alzheimer's. There's no cancer associated with Alzheimer's. So, there's some explanation that I'm not aware of that would account for that.

GC: How long have you and Dr. Huebner been married?

HH: Twenty-three years.

GC: Twenty-three years. Did you stay working in the lab after you married him?

HH: Yes.

GC: And did that change the way you worked together?

HH: No. Actually we had always worked very closely together. We always did. He worked very closely with half a dozen people. That's the way he worked. Most of them were men; I'd say that four of the six were men. But he worked with those people. And they would work with their own people. Each would work with his own.

GC: And you said he had an office set up here at home that he would work in as well?

HH: Yes, yes. We had an office upstairs. It's, of course, not functioning right now. I just have a computer up there. We didn't use it, I mean, we didn't declare it as an office for the IRS, but he worked at home. Actually, he worked right here, he worked everywhere.

GC: At this table, the dining room table?

HH: Yes, he worked wherever he pleased. The house was just absolutely inundated with books and reprints and so on.

GC: He must have published quite a bit.

HH: Yes. Did you never see his CV?

GC: I don't have a copy of his CV. That was one thing I was going to ask you about.

HH: Well, I have, let's see if I have an extra copy. I don't think so. I have one extra copy. I have just two copies. Let's see, this is just his, this is just his awards.

GC: I think I heard somewhere that he had four hundred publications.

HH: It was 425. These are just his awards. Let me see if I can get you a copy of his CV. I think maybe I have a full copy of it. Here are some pictures of him. Have you ever seen him?

GC: I have only seen kind of hazy photocopied pictures.

HH: Let's see if I can find, I hesitate to do this.

GC: You said he had a great smile and a great laugh.

HH: He would just break into a real, a real belly laugh, very, very deep. He has a very, very deep voice. You always knew where he was, because you could hear him. He never, he didn't yell, but he just had a *booming* voice, a very compelling voice. And then he would break up into a laugh, and, you know, it was a very infectious laugh.

GC: Did he have lots of occasion to laugh?

HH: Yes. He did, he enjoyed his work, he enjoyed being with people. He was a very Rabelaisian type. He really was. He's a big man and he sort of grasped everything in a big way. He's just larger than life in many ways. He was not easily intimidated. He used to say he wasn't brought up to know what the fine points were, so he sort of… The other

comment he would make was that, because he didn't go to one of the Ivy League schools, he was too stupid to know what wouldn't work so he would try everything and it often worked.

GC: He sounds like someone I would have really liked to talk to.

HH: You would have enjoyed him, yes. Yes, he would have loved you.

GC: Thank you. I had a couple of specific questions. One of them was: One of the historical accounts I read was talking about his work on adenoviruses, and it said that he stopped working on them because of the SV-40 but also because of ethical complications, and it didn't go into any more detail. It just said ethical complications.

HH: I don't know what those complications would be.

GC: Okay. I thought that was a kind of strangely worded.

HH: No, I don't know what those complications would be. That would never have bothered him at all. It would never occur to him.

GC: Ethical complications?

HH: I mean, he couldn't imagine anything unethical about finding, getting facts. I can't even imagine that.

GC: I wonder. I don't know if you've seen Dr. [De Witt] Stetten's book. *NIH: Forty Years of . . .*

HH: No. Dr. Stetten didn't work with him at all.

GC: Right. He was in another building.

HH: Yes. He was probably, no, I know who he was. He went blind toward the end. He was a good man, but he didn't, Bob and he had virtually no interchange at all. He was probably quoting somebody's, quoting someone, I'm sure.

GC: Right. Okay. And, let's see, did you know much about his early work? He worked on Q fever, I guess.

HH: He worked on Q fever and rickettsialpox, yes. Rickettsialpox was in the Guinness Book as…was it rickettsialpox? Yes, I think so. It was in the Guinness Book as being the fastest disease to have been solved epidemiologically and in the lab. Yes. Is that the one

in New York? I think that was rickettsialpox took place in New York, somewhere in Queens.

GC: Yes. And was he part of ?

HH: He was called in as a Public Health Service officer, because there was an outbreak of rashes and maybe fevers. It turned out it was some mite that had migrated from Russia with the immigrants. They brought their trunks in, put them into the storage rooms of an apartment house that housed many of them, and these mites just proliferated and were carrying the disease. It was a self-limiting, nondeadly disease. He just got the mites and ground them up and put them into mice, and I think it was just a couple of weeks or something, and they discovered the cause of rickettsialpox. Actually that was one of the situations that was very typical of him. He worked with the exterminator, Charlie Pomerantz, and people said, "How can you work with that man? He's an exterminator and he doesn't know anything about science at all, and he has all these notions." And Bob said, "Well, he's the only person who seems to know *anything* at all about what's going on in the vector situation." He worked with him and *really* liked him and made him a co-author on the paper. He said he deserved it. He knew exactly what the origins of these mites were and how to get rid of them and who brought them in and where to find them. He said he would never have been able to trace this. He said there was nothing in the apartments that they could find. When they went into the basement, the walls, he said,

were moving with these red creatures. They're just tiny little things. He said they were just all over, everywhere.

GC: That's amazing, that he made him a co-author.

HH: Yes. He made him a co-author on it. Right. It's in his bibliography.

GC: I guess that just speaks to his…

HH: Yes. But he learned then, he said that was a lesson he had always known, is that you talk to the people who know, who seem to know what they're talking about. It doesn't matter what their credentials are. You don't pay any attention to that.

GC: So it didn't matter that this exterminator had not gone to Harvard Medical School.

HH: No. Or any school.

GC: Right, because he had the information.

HH: He had the information. And he seemed to be very, very with-it. He seemed to really understand how diseases move around. So, he really admired Pomerantz very much.

They maintained a friendship for years, although that particular study was over very quickly. Q fever was a situation where he found it was carried in raw milk, unpasteurized milk. And that was a nasty disease. I don't think it was life-threatening so much, although some people, I think, did die. But it was very nasty. Just in terms of the symptoms. It was a threat to people who were frail, but not generally.

GC: Do you know why he joined the Public Health Service?

HH: I think part of it was that he went from Medical School into the Public Health Service. That was during World War II. He was actually stationed, he was actually assigned to a Coast Guard cutter as the ship's doctor, and he stayed in. He was married. I think he already had a child. And so it was probably a matter of just, he really came of a very, very poor background. He was probably one of the few M.D.s who ever made it without a lot of help. He didn't get any help. So, he probably stayed with them because he just, he just had to have an income. And when they introduced him to the lab, he was hooked. Then he was totally hooked. He worked with a man by the name of Charles Armstrong, who apparently was just a wonderful man. So in a way it was just fortuitous.

GC: Was his reputation already built up by the time you came to work for him?

HH: It was, no, he was, well, I didn't know it, because that was when I had been working in grants. So I didn't know anything about any of the doctors. I knew about neurologists, because my husband was in neurology. So I didn't know it. But apparently it was, yes.

GC: When you worked in grants, did you work with Dr. Saunders at all? Palmer Saunders?

HH: No. I knew him. I didn't work with him. No. I worked with Fred Stone. That was in an old building, T-6, where 31 now stands. That was a very long time ago. It was one of those temporary buildings.

GC: What did the campus look like when you first came there? Was it very different?

HH: Oh, it was beautiful. It was just beautiful. It was very different, because none of the, you know, I don't remember how many buildings there were, but I would say probably half of the number. It was very treed. It was just beautiful. I came from Chicago, which was not beautiful, and New York. So, Chicago has some lovely spots, but we were students and it was not beautiful. We lived pretty poorly there. So I thought the whole area was wonderful, but NIH I thought was just marvelous. It was like a campus, really, like a college campus. And the environment was wonderful. It was very easy. It was just a very nice place. Not easy-going, but it was just, nobody dressed. When I had worked in New York, I was right off Wall Street and, you know, it was a whole different ambience.

GC: What did people wear to work? You said nobody dressed. What would you wear?

HH: Oh, they would wear jeans, you know, or the equivalent of jeans. You know, in the lab you don't dress. You don't wear high heels and white gloves, which was the way I had been going to work. So, it was very different, very relaxed. And people were very excited about what they were doing. It was never just a job, for anybody, except the animal caretakers or whatever.

GC: A lot of people have talked about how they felt the environment was very collegial and there was a lot of sharing between colleagues. Would you agree with that?

HH: Oh, yes. Yes. Well, we had a lot of young people there. A lot of young people who were really gung-ho, people from everywhere, which made it exciting. And interesting liaisons, you know, they were young people away from home, they were finding each other, which is, you know, exciting. So, there were some very strange marriages, you know, between groups.

GC: Oh, really?

HH: Yes. And one of the most interesting, it worked out really beautifully, was an Indian from northern India with a Taiwanese.

GC: Oh, really?

HH: Yes, worked out just fine, except that the wedding was funny, you know, it was with the different groups. It worked out beautifully actually. It's a very happy marriage still. So, there were some wonderful liaisons. And in any case, there were people who got together a lot. They would work until two in the morning and then have pizza. You know, it was just a fun place for the young people. By that time I was married, so I wasn't part of any of this.

GC: I was just asking, because I know you said earlier that there wasn't a lot of sharing between lab groups.

HH: Not between directors of labs. No, not between branch chiefs. There was lots of sharing among the young people. No, that's different.

GC: What was your favorite thing about working at the Cancer Institute? Or at the NIH in general?

HH: Well, my favorite thing, I suppose, would be the people. I'd get very, very attached to people. And the young people were marvelous. They were very bright. They were very beautiful, uncommonly handsome people. You know, I think maybe that's true generally of intelligent, educated people. They were just full of good will and full of excitement. I think that's the part. That's probably the same thing that an academician would say is that it's nice being around young people. And they were very, very bright. It's something that you can't finger too easily, but its nice being around people who are really, really sharp. It's just, it's a whole, there's a kind of electricity that they establish. They're spontaneous, they're fun, and basically a lot of these people now especially have been well nurtured, because they come of pretty well-heeled families, especially those from overseas. So, they're kind. They haven't been kicked around. They're kind to each other. They have very generous impulses.

GC: So it was just a good group overall.

HH: Yes. It's just a good group.

GC: What would you say was your greatest achievement and the thing that you're proudest of that you did while you were there?

HH: I can't think of anything that I did that was so marvelous. I really can't. I was always on the service end of things. I can't think of anything. I think just, I think the part of the job that was most important to the people I worked for was just helping with the promotions, with the visas, just helping keep them on board and happy. I can't think of any one thing. I was just the help.

GC: It sounds like you helped in everything. You were his research assistant and Writer.

HH: Yes. I didn't write his papers, but I did help with the writing. I helped with the introductions and so on. What I helped with was writing up reports, you know, annual reports or whatever.

GC: So, why did you finally leave the NCI? When did you leave?

HH: I left, well, because for one thing I felt the work was over my head. I mean, I could, and I'm computer-literate but I'm not *really* into computers the way you kids are. And I felt that they could use someone there who really understood molecular biology, *really* understood it, and someone who was much, much more tuned in to computers. I never became proficient in graphics. I just used it as a word processor, in effect. I could have

learned all those things, but there were so many people coming up, and I had thirty-five years. I think you have to leave room for people.

GC: Yes. So what year was it that you left?

HH: I left four years ago. I left in 1994 actually. That's five years, almost.

GC: And you were still in Dr. Aaronson's office?

HH: Yes, actually he left before I did. He left and then I left. And that was another thing that was, you know, changing. He went to Mount Sinai, and then I left a couple of months later.

GC: So, who replaced him for that brief time?

HH: I think it was run by Jackie Pierce as acting lab chief. I think she has taken over. Jacqueline Pierce. Good gal, and a beauty, a very beautiful woman.

GC: Really?

HH: Yes. Another beauty.

GC: It sounds like there were a lot of beautiful women.

HH: I really think that people who are well nurtured and, you know, intelligent, just are very pretty.

GC: When Dr. Huebner left, was any of that because of Dr. DeVita cutting back?

HH: No, not at all. He really had to leave. I think he recognized that things were.. So, there was never any question. We didn't even give him the choice. We just said that, you know, your tenure is up now. We could have renewed, his appointment could have been renewed under different circumstances, but there was no effort made to renew him. And that was with my collaboration. There was no reason for him to be renewed. No, I think he had just, he had lost it. But in a way, I think all people in a sense lose it as they get older, even if they don't lose it in that profound way. I think, his own feeling, I remember, was that, if you have to prove something without having data that stares you in the face, then you don't really have a good case. But in molecular biology, nothing stares you in the face. He was a biologist, in effect; he was an epidemiologist and a biologist. And at those levels, things do stare you in the face. You have a cluster of disease here, and you have a bug and you put it in a mouse and it gets sick. It's that simple. When you get to the molecular level, it's a whole different gestalt, really, and I don't think that

people can take on whole new concepts that way without a real background in them. It's not a simple field. And I think there's a point at which people should just leave. I don't think that people, that there should be no age discrimination. I think it's just a mistake.

GC: Really?

HH: Yes. I really do. I could have continued working for ten years, but I'm not as good as the people coming up and I know it. I know where I personally am not as good as I was. I know that, and I'm not being modest. I think there's a point at which you should just bow out. Go into something else. I think probably I could do other things. I do other things now. I tutor, and I do other things that I think are pretty useful. But they don't involve that kind of high-tech, real brilliance that the labs do. And I think, really, most scientists probably should at some point just bow out.

GC: Would you say there was a period of time when science was changing really rapidly?

HH: Yes, oh yes. As a matter of fact, it's been changing, just accelerating logarithmically. It just, it was changing quickly right at the outset, and then it seemed to just double and treble in the rate.

GC: You said you tutor. Where do you tutor now?

HH: I'm just doing, this is an informal, I am just tutoring refugee kids in English, or foreign children in English, and some adults. I do one at a time, so I'm not doing many. And I'm working with the refugee settlement group. I just do it in phases. I can do it on a sort of personal level. It fills time, but I feel it's pretty useful. It's helpful to people. And I feel strongly about it. I'm not involved in any job.

GC: I just remember you said you volunteered.

HH: Yes. So, that pretty much ties up my week. And then I visit with my husband once or twice a week.

GC: And he's up in?

HH: He's in Coatesville, Pennsylvania.

GC: Oh, he's in Coatesville?

HH: Yes, right. The VA up there specializes in neurological diseases.

GC: Well, I think I've gone through most of my questions. Would you say you felt a loyalty to the Institute, or that most people felt kind of a loyalty to the Cancer Institute or to the NIH in general?

HH: I think I feel it to the NIH, certainly, because I worked for three Institutes. I think I feel it, I don't feel it especially for the Cancer Institute, but I do for the NIH. I think it's a remarkable institution. I think with all the carping about it, it is a remarkable place, and most of the people there are still very idealistic. They work their tails off and they're extraordinary.

GC: What was the third Institute you worked for?

HH: I worked for Neurological Diseases and Blindness that was the first Institute I worked for and then the NIAID, and then Cancer.

GC: Do you think most people were fairly loyal to the NIH or to the NCI?

HH: Oh, yes. Oh, yes. I think so. Yes, I think so. There are not many people who have complained about it. They have their private gripes about one thing or another, but not about the institution at all. No, I think that was a very happy time in my life, really. You really see the results. That's nice.

GC: That's true. It sounds like it was an amazing place to work, and it is an amazing place.

HH: It is, yes. It's an amazing place to work. I think part of it is getting, you know, people from everywhere. You get perspectives from everywhere. It was just great fun, really. Working with very, extraordinary people, and very kind people, very generous in their overall approach to life. Not in terms of money. I don't know that any of them have a lot of money, but they just have a very generous outlook. It's very forbearing. It may be just true because they're smart. They tend to be expansive.

GC: Or perceptive.

HH: Or perceptive, right. But the kind of bigotry that you read about or the kind of nonsense that you read about in the paper, it would never happen with those people. It was hard to imagine in that environment that such crazy things went on elsewhere.

GC: Can you think of anyone else that I should speak to about the NCI's history?

HH: Oh, well, John Moloney would be good, if you could get your hands on him. He was the author of the virus cancer program and chief protagonist. He lives right in Bethesda. Moloney is spelled M-o-l-o-n-e-y. John Moloney.

GC: Okay. And he's still in Bethesda?

HH: He's still in Bethesda. And I wish I could remember, I think he's off Fernwood somewhere. He's not on Fernwood. I can't remember the name of the street he's on, but he's in Bethesda. He should be in the phone book. Jack Gruber is another person, G-r-u-b-e-r. He is in charge of the grants program, and he has been with the NIH virtually since I have, and he's probably seventy years old now.

GC: And he's still in charge of it?

HH: He's still in charge of grants, I think, or contracts. I'm not sure. But he's in the extramural programs and his name is Jack Gruber. It's not John. G-r-u-b-e-r.

GC: Okay.

HH: I think another person that you probably heard of with Dr. John Stevenson was Jim Duff. Jim Duff was there for a long time and he is an *absolute* honey. He is an *absolute* honey. He's the dearest. I can give you his phone number. Does this need to be on the tape?

GC: Actually it does help me if it's on the tape.

HH: Oh, okay. I don't have it in my head.

GC: That's fine.

HH: I'll get it to you. And I'm just trying to think.

GC: I'm talking to Dr. Scolnick and Dr. Todaro.

HH: Yes, Scolnick and Todaro are good. Dr. Janet Hartley. It would be good if she is still in the area. She has just retired. She is a princess. She lives on Macomb Street in the District. Her name is Janet Hartley, spelled in the usual way. She'll be a real treat. She's a real gentlewoman. A beautiful woman. I'm just trying to think who else. I'm thinking of old-timers, people who could give you some perspective. You talked with Carl Baker, I guess.

GC: Yes.

HH: I talked with him, right. He's kind of an affable guy.

GC: He is. He was a lot of fun.

HH: And I think that's about it, knowing that NCI is your interest. Not outside groups. I think that would be it.

GC: Okay. Is there anything, I've pretty much gone through my questions, but I don't know if there's anything you feel like I didn't ask you?

HH: No, really not. I think you've covered just about everything.

GC: We covered a lot of ground.

HH: Yes, right.

GC: I just caught one out of the corner of my eye, actually. Did you come to the NCI with a goal, like did you come in with goals about where you wanted to be or what you wanted to do?

HH: No. Not really. It was just a job at first. I knew I needed to work. It wasn't, when I came to NCI, it was really part of NIAID and it wouldn't have mattered to me where I was. I was just working on Bob's program. It didn't matter. So I didn't come to the NCI with the idea that I had to work for cancer. There was no particular push in that direction. It

just worked out, it just happened that way. And, as I say, it was an administrative and political move, rather than one of mission. The mission stayed the same.

GC: Okay. Well, I'm going to stop the tape then, unless you have other final comments.

HH: No. That's about it.

GC: Anything you want recorded for history, for all time?

HH: No. I don't want anything recorded for history. I think that there's too much recorded for history now and we have to start eliminating.

GC: You can't say that to a historian.

HH: No. That's true.

GC: This is Gretchen Case. I'm ending the interview with Mrs. Huebner.