

**Oral History Interview: Dr. Marvin Schneiderman, who made important contributions in statistics, particularly in the cancer field at the NCI**

**Interviewer: Dr. Carl Baker, former Director of the National Cancer Institute**

**March 29, 1996**

Baker: Marvin, we certainly thank you for allowing us to draw on your time and your expertise and your interesting background. So, first, before we get to the questions I sent, could you give us a little bit of your background, where you went to school and some of the jobs you've had, and also include a little bit of the history of Harold Dorn's bringing into NIH some of you interesting statisticians and mathematicians.

Schneiderman: Well, I'll try. I'm flattered that you would want to talk to me about this, but I'm happy that you're here. My own background. I was born in New York City and all the schools I went to were in New York City, including college. Had there not been a free City College of New York, I probably wouldn't have gone to college. In fact, my parents were immigrants and I was the--

Baker: From where?

Schneiderman: My father was born in Poland, in Warsaw, and my mother came from the area now known as Belarus, White Russia, from a little town just outside Minsk, where I discovered, many years later, when we had detente with the Soviets, one of the Soviet lab people who worked on environmental cancer also came from this little town. I'll think of his name after a bit [Alexander Shabad]. It was quite exciting to find somebody from there. He told me, however, that during World War II the Nazis had completely wiped out the town. There was no longer any town, no longer any people living there. As I said, I went to school in New York City, the City College of New York. I was a math and statistics major. But I did a lot

of other things. I'd always liked to write, and so I found myself doing public relations for the Athletic Association. That, of course, was great because it got me into the basketball games free, and City College, at the time, had a great basketball time. Nat Holman, the famous professional basketball player, was the coach. My first job after I got out of college was with a corrugated box manufacturer. In spite of being a math major and a Phi Beta Kappa that was the only job I could find. That lasted for about a year. I took some Federal Civil Service Exams and I found myself, a year after I got out of college, working for the Census Bureau for the Census of 1940, which was a temporary job; when the Census was over there would be no job. But everybody told me, "Take the job. You'll be in Washington. You'll find another job when the Census was over." And that really was true. At Census I had become a Section Chief in the Agriculture Division. Although I said I'd lived and grown up in New York City, actually I'd spent a fair amount of my high school years on a poultry farm owned by my aunt and uncle. My own family had broken up and we came to live with my aunt and uncle, so I knew a little bit about agriculture, at least poultry and I had some interest, in general, in agricultural economics, and so I wound up in the Census of Agriculture as a Section Chief. The Census was winding down, the international political scene was miserable, and I found a job working for the Office of the Quartermaster General in the War Department. And, because of my agriculture experience, and by this time the Census experience had added a great deal, I found myself in the Office of the Quartermaster General in charge of the statistics on food and food purchasing and food consumption for the Army. That was kind of fun. I found myself working with people in Agriculture and in the equivalent

agency, i.e., the price control agency, that what's his name--Henderson-- was head of. We knew really very little about actual food consumption in this country and we knew very little about what was really needed to support a large military. I remember one of the incidents I got involved in. We used to send over to Agriculture and to Commerce, Foreign and Domestic Commerce, the secret stuff that we had computed saying how much of what we needed. And one day I got a telephone call from my equivalent in the Commerce Department who said to me, "You guys have indicated that you want so and so much chocolate?" And I said, "Yes, that's cocoa for the soldiers and the sailors and so on to drink." He said, "Did you know that you asked for about three times the world's production?" (which showed you how little we knew really). And I said, "No, I didn't know that."

Baker: Well, that set you up for when Endicott enlarged the need for mouse supply.

Schneiderman: Yes. Right and that job lasted--the Quartermaster job--lasted for about a year, a year and a half, and I was drafted. The Quartermaster General had requested that I go to Quartermaster Corps when I was drafted. Now, obviously, I realize now, they were just going to send me back to the same job in Washington, but as a sergeant probably, if I ever made sergeant. I had no desire to do that. I felt I was in the Army which was fighting a war that my country had gotten into for good reason and I ought to be doing what soldiers ought to be doing, so I requested transfer to the Air Corps, and I got a transfer to the Air Corps. I wanted to fly, if not a pilot, or something of that sort, maybe a navigator. That appealed to me. Navigators had to know a little mathematics. So I applied for navigator school. But, by the time I applied for it, they had decided that I

was not physically up to it. My eyes were not 20/20; they were 20/30 or 20/40, or something like that, so I was rejected for that. The only other thing that was in the Air Corps that seemed to be possibly related was Meteorologist, so I applied for Meteorologist School. I was at Sheppard Field in Texas at the time. And I stayed on, and on, and on, and never got called to Meteorologist School. I had married by this time and my wife had a Red Cross scholarship to be a social worker, and she was going to school at Washington University in St. Louis. When I discovered that I'd never go to meteorology school, because they had essentially closed them down, I requested a post as close to St. Louis as I could get so I would be near my wife and, by God, they sent me to Scott Field, Illinois, which was really quite close, 20 miles or so, at Belleville, Illinois. I don't know how I got all these things that I asked for. Everybody told me, you know, "If you're good in languages they'll put you in mathematics" kind of thing in the Army, but they really put me just about every place that I wanted to be. So I was at Scott Field for some time and I was put in the Statistical Office there, mostly sort of population statistics--how many people were on the base and this was a school and what was going on in the school and how many people they were graduating--that kind of statistics. I had a very interesting commanding officer, Major Copeland, who was a veteran of World War I, a very nice man, and very pleasant, and very friendly. He lived on the base. And some of this stuff that I see in "Beetle Bailey" reminds me of Major Copeland. He used to try to hide from his wife who could see the lights in our office in the Headquarters Building from where they lived. And Copeland used to say to us, "You guys got anything to do tonight?" "No, sir." "Would you please come in the office and put the lights on? I

told my wife I'm going to be working tonight." As I said, a very sweet man, a very nice guy. He came to me one day and he said, "Marv, he said, you're physically able, you're a healthy young guy. They're going to switch you to the Infantry unless we do something." I said, "Oh, my gosh, I don't want to be in the Infantry." By this time I'd decided I really didn't want to be shot at. I had been cleared for flight crew to be a radio operator/gunner, but he didn't like that idea. He didn't think I ought to pursue that. He said, "Maybe you can go to OCS?" And so I went to OCS. And I got out of OCS, and I had done very well in OCS. In fact, I was number one in my class, in Miami Beach, in Florida. That gave me the opportunity to choose any post I wanted in the United States. And there was a Statistical Control School at Harvard, in the Graduate School of Business, and I asked for that and I got sent to that. That was great. Essentially, although we were in uniform, we were in the process of really getting an MBA from the Harvard School of Business, which really improved my breadth and knowledge and information enormously. We had all the Harvard people teaching us, the famous economists and that sort of thing. One of the instructors was Eugene Zuckert, who later became Secretary of the Air Force. A tremendous education, a real Harvard education and I have appreciated it ever since. It also got me sent, within the United States, to, as I said, first Harvard, up at Cambridge, then I got sent to Wright Field in Dayton, Ohio. By this time my wife was working for the Red Cross in a military hospital in Louisville, Kentucky, and Dayton and Louisville aren't too far apart and we could get together fairly frequently, which was very nice.

Baker: What year was this?

Schneiderman: '44-'45.

Baker: I graduated from medical school in Louisville in '44.

Schneiderman: Oh, really? She was at Nicholas General Hospital in Louisville. I eventually was stationed in Chicago for a while, one long winter--I learned to appreciate Chicago winter--God, it was cold.

Baker: I interned in Milwaukee. It was about the same.

Schneiderman: God, it was cold that winter. Eventually they sent me to Wright Field, Wright-Patterson, which was the Headquarters of the Air Corps research activities, and I became a Statistical Control Officer and, among other things, got into such things as life tables for equipment, particularly aircraft engines, essentially forecasting survival of flight crews and, therefore, how many crews would have to be produced. I became the Chart Room Officer, where we had all this continuing information on what was going on in quite some detail. I often thought that the Army must have worked on me in some way unknown to me because my instructions, of course, were, "You know, you're the Chart Room Officer but, when you get out of this room you're not supposed to tell a soul of what you've seen in here," and by God it worked. Something happened so that really, when I'd get out of the Chart Room, I literally didn't remember what was there. I didn't put the pieces together.

Baker: It reminds me of the different language used on ship, which was rather dirty, shall we say, but when you went ashore you immediately stopped talking that way, and then when you went back on ship it was the common language again and you had no trouble separating.

Schneiderman: No trouble shifting. It was astonishing. You know, I knew in detail-- We'd get visiting generals who would come through and I would have to conduct them through and tell them what the charts meant and what the implications were and, in a sense, I was helping doing a lot of the basic

planning for some of the Air Corps activities. But honestly, the minute I got out of that room I didn't know what was in there. You know, I knew what the titles were of the charts, but I didn't know what they meant. I was always very impressed by whatever it was they did in my training that led to that. The war ended and I was still in the Air Corps and they didn't know what to do with us, obviously. We were in Dayton, Ohio.

Baker: What was your rank then?

Schneiderman: I was a First Lieutenant. They didn't know what to do with us. So, a couple of us suggested that they ought to send us to Ohio State, which wasn't terribly far away, for further training. So, they sent some of us to Ohio State to take courses in economics. That was the closest they could figure. They didn't want to send me in for mathematics or statistics; they sent us up there for economics. And we had some very good people. I learned a great deal. In fact, we had one of the early Keynesians, a guy by the name of Wolfe, who taught us. We used Keynes' original text essentially. And then there was another one by a German economist. I still remember it. It was called "Employment, Interest, and Money." And reading that Germanic text convinced me I didn't want to be an economist. It literally put me to sleep. I'd read four pages and then I'd have to shake my head to wake up.

Baker: Like reading Immanuel Kant?

Schneiderman: I don't know what it was like, but it literally put me to sleep. I was still in the Army and the Annual Statistical Association Convention was in Cleveland and we were in Dayton, and I said to the Commanding Officer, "I want to go. I'm a statistician." And he said, "What are you doing on the job?" And I said, "Sir, as you know, I'm playing chess; I'm not doing anything here." He said, "Yes, I'd heard that you were doing

that." I was a lousy chess player. But there wasn't really much of anything else to do. So he said, "Sure, go ahead. Go to the meeting. We'll pay for it. It's only Cleveland. It won't be any problem."

I went to the meeting in Cleveland and I ran into a man who had been a teacher of mine here in Washington. The Department of Agriculture had some very good statisticians when I first came here, as did the Labor Department, the Bureau of Labor Statistics, and the Department of Agriculture had a graduate school, and I went to the graduate school there to learn some advanced statistics, and a couple of my teachers were guys from the Bureau of Labor Statistics, one of them being Jerry Cornfield, and I ran into Jerry at the Statistical Association Meeting. And he said he was glad to see me, and I was very glad to see him. And he said, "What are you doing?" And I told him, "Not much." And he said, "Well, if you're in the Washington area, if you get a chance, come and see me. I'm leaving the Bureau of Labor Statistics and I'm going to the Cancer Institute." He said, "I don't know a damn thing about cancer, except my father died of it." And he said, "I'm going to the Cancer Institute." And he said, "You know, who knows? I don't know what's going on. Maybe there will be a job for you?" So, as I said, I was still in uniform, and I succeeded in getting myself transferred to Andrews Field and was mustered out of the Army. I was at Andrews Field about 5-6 months and I ran into Jerry again and he said, "We've got a couple of vacancies here at the Cancer Institute. Put in an application. Maybe we'll have you." I said, "Jerry, I don't know anything about biology, I don't know anything about medicine." He said, "Neither do I." He said, "You know, I know some sampling statistics." He was one of the people who really developed sampling theory when he was at the Bureau of

Labor Statistics. Harold Dorn was in charge of the group that Jerry was in. There were a couple of other statisticians there that Dorn was not particularly happy with, that he encouraged to leave. I had an interview with Dorn, and he accepted my application, and I got hired by the Division of Public Health Methods, I think it was called. It was in the Public Health Service.

Baker: And what year was this?

Schneiderman: This was '48. Jerry was going to be my boss. And I remember that in order to get on the job--I was living in Southeast Washington at the time--I had to go into downtown D.C. to what was then the Federal Security Agency--FSA--and we had to take a physical and that sort of thing. I took the physical. There was a shuttle from downtown out to NIH, which I thought was way out in the country at the time. I got on the shuttle with one other guy on the shuttle going out to Bethesda, and he too had been hired by Jerry Cornfield. It was Sam Greenhouse. Sam had been working for the U.N. and decided that there was no future for him in the U.N. The two of us got hired on the same day. And we got out to NIH and they put us in what amounted to a large bullpen with about 5-6 desks in it. Jerry was head of the group.

Baker: In the old temporary building?

Schneiderman: In old T-6. Jerry was head of the group, and so his desk was closest to the window. Nate Mantel was there. Mantel had done some very interesting stuff during the war on testing pesticides and insecticides for the military in Florida. And Nate had been there before we came, so his desk was closer to the window than anybody else's other than Jerry's. There was Jake Lieberman there and Sam Greenhouse and me and that constituted the Math/Stat Group. I found myself very quickly working

with some of the lab people. You know, they all came to Jerry and Jerry parceled out the jobs. Among them was George Brecher, a hematologist, with whom I'm still friendly, and lots of other great people. My impression was this NIH was a fabulous place. I'd never come into a place with such high intellectual standards and levels. I mean these people were-- I thought everybody I ran into was a genius, and they probably were.

Baker: I don't know, but I think we appreciated then, as we do now, that high quality and the standards, as you say, were set very high.

Schneiderman: They were set very high. And there were such really good people.

Baker: And everybody got along, or most of them, got along well.

Schneiderman: Well, you know, I got to know Andervont and several of the other people. They brought Mike Shimkin in from California, and Mike was then head of not only the Statistics Group, but also the Epidemiology Group. In the Epidemiology Group there was one statistician, I remember, Doris Sadowski, Doris Abrams, a very competent woman, who married a guy by the name of Sadowski. Her boss was Alexander Gilliam who had worked on the epidemiology of polio. You know very competent people. And when I read the papers these people wrote I was so proud to be part of this group. " My God, they're good," I'd say to myself. The NIH emphasis, as you well know, was that this was a scientific research agency. And what does a scientific research agency do? What do the people in the agency do? They do research and write scientific papers. And, by the time I left NIH, I had published about 150 papers. And it didn't seem as if I had worked on them. You know, they were just the natural products of what you were doing, what you were working on, and the result of the collaborations that you were doing.

By the mid-fifties I'd gotten some concerns about human experimentation. I was concerned about some of the ethical problems in human experimentation. Harold Dorn invited, from Great Britain, Sir Austin Bradford Hill to come and work with us. Bradford Hill essentially invented the randomized controlled clinical trial, and Dorn thought that was something we ought to know about because the NIH was beginning to do a fair amount of clinical research. The Clinical Center hadn't been built yet, but we were doing clinical research and Dorn thought it would be good to have Bradford Hill at NIH. But Hill didn't come; however, he eventually sent one of his bright young men, Peter Armitage. He worked with us for a year and, though Armitage is maybe 4-5 years younger than I am--at least 5 years younger than I am--he really became my mentor, in addition to Jerry. And eventually, when the Cancer Chemotherapy activity started up and we started doing clinical trials, I got a Rockefeller Public Service Fellowship to go to the London School of Hygiene and to work with Armitage in developing schemes for ethical experimentation on humans, and that's what my doctorate is on. It's a mathematical system for essentially minimizing the number of persons on whom you have to do research before people are convinced that you've got an answer they ought to pay attention to.

Baker: Where did you get your doctorate?

Schneiderman: Here at American University in Washington, D.C. I did the research for it at the London School of Hygiene and Tropical Medicine where Bradford Hill was Professor and head of the department. Richard Doll was in the department, along with Peter Armitage, and several other very competent people. It took me a little while, as an American, to get used to the British and the way the British operated. There I was, an

American, and we found a house right on the subway line, the Metro--the Underground--right on the Underground line that came from where we lived to one or two blocks away from the School of Hygiene. If I got on the Underground at 8:30 in the morning, I was at the School of Hygiene by 9:00 I discovered that I was the only one there at 9:00 o'clock. The Brits started wandering in at 9:30, 9:45, 10:00. When I came in, the cleaning people were hard at work, and they looked at me very suspiciously. I'd go into my office and every 5 minutes someone would open the door and look in to see what I was doing. "What's this crazy guy doing here this early in the morning?" I did the research the year that I had the fellowship. I did the research with Armitage that led to my doctorate here in the United States. The department was a great place to be. Bradford-Hill was a marvelous teacher and a very good head of the department and a very useful guy for guiding young people. I remember one day I'd seen something in one of the American medical journals that really upset me, a rather badly done trial, and I wanted to talk to Hill about it. I had the journal in my hand and I came into his office, and his secretary asked me what I wanted to do and she said, "Well, let me go in and see the Professor and see if he can see you." She went in and she said, "He'll see you in 10 minutes." So I fidgeted for 10 minutes and she led me into the office. I came into the office. I said, "Professor--" He said, "Sit down, Schneiderman." By this time he was very friendly with me. He called me "Schneiderman," not "Mr. Schneiderman." "Sit down, Schneiderman. Sit down. Sit down." He had behind his desk one of these old metal military storage cabinets, you know, painted battleship grey kind of thing. He opened this up. He wouldn't let me talk about what I wanted to talk about. He said, "Just a moment, just a moment."

He opened this cabinet, took out a bottle of sherry and two glasses, poured one for himself, one for me, and he said, "Do have this first. And sip it." So I sipped the sherry and then finally he let me talk. That's the way he worked all the time. It was marvelous. He let me talk. He let me tell him what was bothering me. And he said some soothing words and gave me some kind words of advice. He had a marvelous sense of humor. I remember the first lecture I attended in the London School of Hygiene. It was in one of these slanted amphitheater type rooms, and I was sitting way up in the back. And he was giving this lecture, his first lecture on the need for controls in medical research for the people who were taking the Masters in Public Health Statistics, MPH--Master in Public Health. He was going on in the lecture, when he suddenly stopped and pointed to me in the back and said, "See that American sitting back there?" And everybody, of course, turned and looked. He said, "He's the father of twins," which I am, and he said, "You know, he had one twin baptized and he saved the other for a control."

Baker: Was that true?

Schneiderman: The story is true. That's what he said. And I would think that his students never forgot the need for control, and that twins might be very good controls for each other.

Baker: That reminds me of a discussion I had with Jerry Cornfield about the controlled trial on prayer.

Schneiderman: Oh yes, that used one of--

Baker: And so he cited a reference for me to go look up.

Schneiderman: In the *Journal of Chronic Diseases*. There was a paper on a controlled trial in prayer using the sequential technique, one of the techniques that I had worked on with Armitage, actually it used one of Armitage's

techniques.

Baker: So, even though I'm not much of a statistician, if at all, at least I appreciate the significance of controls. That's where you need to look carefully.

Schneiderman: Well, you know better than I that a large number of medical people think that historical controls are satisfactory. I mean, you know what happened in the past and you know what's happening now. And there were lots of people who really felt this and thought what we were doing with controlled trials--by this time there was a Cancer Chemotherapy National Service Center and we had the trials activities going on--and Ken Endicott pushed very hard for the controlled trial. He was much impressed by Bradford Hill. Hill, who had come and given a couple of lectures at NIH. As I said, Hill had a marvelous sense of humor. He was a visiting scientist at Hopkins, and I was to pick him up at Hopkins and bring him to NIH to give a lecture. He introduced me to his wife, who was a very plain looking kind of square lady, most unstylish from the American point of view. You wanted to say, "Gee whiz, you're not a very particularly exciting looking woman, but you certainly could be more attractive than that." I don't know what brought it up, but, as I said, he introduced me to his wife and then he said, "You know, you Americans, really, you've been taken over by the Puritans." I said, "Oh, really?" And he went on, "We were here in Hopkins and we came to register at the hotel that they'd put us up in and I signed the register, 'Sir Austin Bradford Hill and Lady Queenie Hill.'" And Mrs. Hill was sitting there. He said, "The clerk looked at my signature and looked at what I had written down and he said, 'That's Mrs. Hill, isn't it? I hope?'"

Baker: That is what?

Schneiderman: "That's Mrs. Hill, isn't it? I hope?" And she piped up from the back seat and she said, "You know, I've never been so flattered in my life."

Baker: Well, this background certainly gave you a lot of input for dealing with the problems that you got into at NCI, this variety. I was reminded, though, when you talked about Hill giving his first lecture on trials to students at the School of Public Health, what he should be doing, of course, was to give it to the medical students, because the medical students didn't learn this sort of thing and still don't very much.

Schneiderman: Yes, and still don't very much. Somebody, I don't know who it was, who as dean at Georgetown at the time, at the medical school, had been very much impressed by Bradford Hill, and I remember he came to NIH to talk with, I think it must have been Rod Heller who was Director of the Cancer Institute at the time. The Dean wanted several classes in statistics for his medical students and he said, "Bradford Hill said all the statistics a medical student needs to know he can learn in 4 lessons." And he asked Rod who could come and give 4 lectures to the medical students at Georgetown. We had worked a lot with Heller and were very friendly with him, and he suggested that the Dean talk to Jerry and Jerry's group, and I wound up as the lecturer. The first thing I said to the Dean was, "You know, Bradford Hill said it could be done in 4 lectures. Maybe Bradford-Hill could do it in 4 lectures, but I don't think I could." So he said, "Well, would you object strenuously if we gave you what amounted to 10 lectures on Saturday morning?" I said, "Saturday morning?" He said, "Yes. Some of the students will come."

Baker: Yes. I had my statistical course in medical school on Saturday morning.

Schneiderman: He said, "Some of the students will come." That turned out to be a tremendous experience because I got to know what it was that medical

people wanted to know, what medical people could understand, and it took me away from talking just technical stuff to my fellow statisticians. I must have done this lecturing for about 10 years and I really enjoyed it very much. I certainly learned as much as the students did, probably more.

Baker: Well, I used to see a lot of Jerry Cornfield about that time because I was in T-6 with Ralph Meader, and it was fun to go over and talk to Jerry about anything. I learned a lot from you fellows. I learned to appreciate many things that are very important and I value my experience with you guys tremendously.

Schneiderman: Well, you were very supporting of us.

Baker: Well, I always thought you guys were doing a great job and you had brains and you had quality standards and I learned a lot from you.

Schneiderman: Now I'm going to brag. I'd been at the Cancer Institute, I think, two years and I went to a meeting of the Biometric Society on statistics in biology and medicine. I guess I was giving a paper and Sam was giving a paper and Jerry was giving a paper and Nate was giving a paper. And I went to this meeting and every session that I attended I found I could say to myself, "We've worked on that. You know, I think we're at least up to what these guys are talking about," and most of the time we were ahead of them. I was astonished. There wasn't a session in which we hadn't worked on the problems that were being discussed in that session. Jerry was phenomenal.

Baker: Well, all of you guys, I really appreciated. Sam too is a warm individual like Jerry was.

Schneiderman: A marvelous consultant. Jerry taught me a great deal, not just in statistics, but how to behave toward people and how to operate. I

remember I was there a very short while and he'd gotten a paper to referee, and he was very busy so he gave it to me to referee. And he said, "Let me see what you've written before we send it out." My first review tore the thing to pieces and he said to me, "Marv, go back and do this again." "Remember, the guys who wrote this were M.Ds. and Ph.Ds." He said, "They couldn't have been very stupid. Look at what they've done that's good. Take another crack at it." And, of course, he was right.

Baker: Do you remember we had sessions on trying to develop mathematical models for cancer systems that we met Tuesday afternoons after work?

Schneiderman: With Jesse Steinfeld?

Baker: Yes. He and I and you and Mantel, and Jerry came most of the time.

Schneiderman: Yes. I'm trying to remember. It wasn't just Jesse, but several other people. Don Tshudy came regularly. I was very impressed with Jesse. He was a very sharp guy.

Baker: Well, he and Jerry ended up with a paper on math modeling and they used isotopes for following nitrogen metabolism in cancer patients, obtaining the data which grew out of those sessions, so I thought that was a--

Schneiderman: There was a guy by the name of Kuff, I think.

Baker: No. I don't remember his being there. Maybe.

Schneiderman: Kuff, or Kopp, or--

Baker: Ed Kuff.

Schneiderman: Ed Kuff.

Baker: Yes. He was later Head of the NCI Laboratory of Biochemistry.

Schneiderman: Yes. Well, he attended from time to time.

Baker: Well, anyway, that was where we had fun with Mantel because, you remember, after we developed the equations for the compartment models

there was an indeterminate area for the equations (the curves for the equations had gaps in them) and everybody was confused at first, and Mantel came in the next week and said, "Well, you have to do this in three dimensions. There is a saddle point there. And when you express it in two dimensions it's indeterminate.

Schneiderman: You don't see it. Yes. And then, of course, at this time Harold Dorn was collecting the incidence statistics which eventually became the SEER Program, and he had Sid Cutler in charge of that. Sid had gotten his Master's in Public Health at Columbia, which was probably, at the time, the best public health school in the country. And Sid was an extremely well organized guy.

Baker: I was always impressed by him with his willingness to get into the real world and tackle the practical problems to generate the data he needed.

Schneiderman: To generate the data. Yes.

Baker: I got after Bob Miller for not generating data. He was very good at milking the literature. But he finally set up some programs that generated data.

Schneiderman: But Sid was so good because he was so well organized. I was so envious. Sid was one of these clean desk people. You know? And he used to get so much done. I couldn't do that. You know, we were different people. By this time the Cancer Chemotherapy National Service Center was running full blast. I guess Armitage had come when we were sort of just setting it up. And we set up the sequential scheme for screening drugs in animal studies. The sequential schemes I set up for human testing were parallel to what we were doing in the animal testing and there was real overlap and contact between the two groups. I became the first statistician for a randomized controlled trial in cancer.

Eventually we added some people within our own group, and then parceled the statistical work out to the statisticians in the rest of the country when other groups were formed. I went to London the year after Armitage --two years after -- Armitage had been at NIH. And when I came back the controlled trials were running very well and very extensively and, to tell the truth, I'd gotten a little bored and a little unhappy with them and I wanted to do something else. And I got into epidemiology. I got into Cancer Prevention. By this time Bill Haenszel was head of the statistics group. Dorn had done the Veteran's Study on Smoking and Cancer. Bill Haenszel and Jerry Cornfield and Mike Shimkin had carried it forward in more detail, and it looked like there was much more fruit in the prevention area than in the treatment area. I think we're certainly much more successful in our treatments now than we were then but, you know, we didn't do what--was it Dusty Rhoads of Sloan-Kettering remarked about 1951 or '52--that in ten years we would have the cure for cancer. And we didn't have the cure, obviously, "the" cure. We've certainly learned a great deal and we're certainly treating the disease much better.

Baker: Well, why do you think we're not further along than we are on cancer, because of the number of outstanding people and the man-hours and money that's been spent here is sizeable.

Schneiderman: I think it's just a much more complicated process than we thought. I was looking at the abstracts in the AACR meeting next month--you know, the abstracts are an inch and a quarter thick--and looking at the things that people are doing, and I was impressed favorably by all the things they were looking at and impressed unfavorably because I saw no papers, no abstracts, pulling these things together. It seems to me people went off

on their own. The analogy that I used, I think, in that piece that you read, was the one about the wheel with spokes. You had a wheel with a hub and a spoke grew out of the hub, and the spoke was kind of interesting and so it grew, and grew, and grew, and grew, and the next two or three spokes, which were also just as important to make the wheel run hadn't excited as much interest and so we had one long spoke and several short spokes, and a wheel doesn't run well that way. I don't know how you do the integration, but I really felt that we were not doing the integration of--

Baker: Well, this is why I got into systems planning.

Schneiderman: Yes. I remember that and, as I remember, people resisted you very strongly.

Baker: Oh, yes. It's very unpopular with the academic scientists still.

Schneiderman: So, as I said, I'd gone into the epidemiology. I was concerned with prevention. I did some arithmetic and, by God, if we just put together what we knew at the time, it said that a third of all cancers were preventable. All we had to do was apply the knowledge. I wrote this in a paper which was in a volume that Joe Fraumeni edited. And I think that if a third were preventable at that time, probably we should be able to prevent at least half of all cancers now, by what we now know, and not knowing anything more. I got into a minor row with Albert Sabin about this. Sabin was an easy guy to get into a row with. And it was the program on where cancer research stood, or something of that sort, which we had at NIH. And I'd made these remarks that I thought a third of all cancer was preventable, and Sabin's was the next paper after mine, or a couple after mine, and he got up and he sort of attacked us. In effect he said, "Okay, you people have found out about smoking--Harold Dorn and Mike Shimkin and, for the American Cancer Society, E.C. Hammond

and Dan Horn-- but," Sabin said, "You guys have found out about smoking. But what have you done lately?" And I really got pissed off and in my ungentlemanly worst I got up and said, "This is what we've done. You want to know what we've done lately. I'd like to know what have you guys done at all?"

Baker: Well, let me go back to the question about why we're not further along. And I agree with what you said. A one word answer is "complexity." We have not appreciated how complex biological systems are and I still think we're going to have to learn some new ways of dealing with this complexity. The interacting metabolic pathways with all the feedback loops accounts for much of what living organisms do, and somehow we haven't got a hold of that yet.

Schneiderman: Yes. And they are whole integrated, completely integrated, systems and we're trying to, I think the word is--somebody used the word "reify"--to extract a small piece and then try to understand what that did without being aware of all the feeds to that small piece.

Baker: This is why these developments in chaos and fractal geometry, I think, are very important.

Schneiderman: May be of some real consequence. Yes.

Baker: Because it allows you to look at it in that integrated fashion.

Schneiderman: My next door neighbor here is the head of the math department at Georgetown. He's a very nice man, very bright. And I've gotten him to talk once or twice about the chaos theory from this point of view and I've been very much enlightened by what he's had to say. I wondered whether there really was anything much to the chaos theory and he really thinks there is.

Baker: Well, I've been reading a lot in that area and it's fascinating. There is a

book called *Complexity* by Waldrop, which is one of the best I've seen for expressing how living organisms integrate.

Schneiderman: I don't know that.

Baker: He's a science writer, so it's written supposedly for the educated layman.

Schneiderman: I found one by a science writer on chaos theory itself and I'm trying to remember who the author was.

Baker: Well, the one called *Chaos* was a fairly good seller. That's a good introduction to the subject.

Schneiderman: Yes. I think that's the one that I've got.

Baker: Yes. I can't think of his name at the moment either but-- Gleick. James Gleick.

Schneiderman: Yes, that's right.

Baker: Yes. I enjoyed that thoroughly. But this one by Waldrop you might want to look into. It, I think, pulls this together.

Schneiderman: What's it called? *Complexity*?

Baker: That's just the name of it, yes, *Complexity*.

Schneiderman: One of the great things about working with Harold Dorn and the people that he brought around him, and I think whatever success we had with that group was really the result of Harold Dorn's choosing people to work with him who could work with each other and choosing people who had very much broader background than the usual statistician or mathematician. Jerry Cornfield, as you remember, was a history major. Do you know how he got into the statistics and mathematics?

Baker: No.

Schneiderman: He'd taken an examination for a social science analyst because he'd been a history major, and he passed with a good grade, and he got assigned to the Bureau of Labor Statistics. It was at a time when jobs were very hard

to get. He said to himself, "What the hell am I doing in the Bureau of Labor Statistics? I don't know anything about labor or statistics. What am I doing here?" At the BLS he ran into a guy by the name of Jack Karro who asked the same question. Karro had been trained as a lawyer, except that he knew some mathematics. So Jerry and Jack rented an apartment together, lived together, and Karro tells the story that Jerry says, "Do you know any mathematics? I've got to learn mathematics. How am I going to learn mathematics on this job?" And Karro said, "Well, I took a lot of mathematics. I've got some textbooks. Maybe I can teach you." Karro says in three months Jerry had essentially consumed all of Karro's textbooks, he said, "And then he left me in the dust."

Baker: No. I didn't know about that.

Schneiderman: And I think because of the different backgrounds--Harold Dorn's was in sociology, population; Sam Greenhouse had been in the U.N. and done whatever the U.N. people do--

Baker: What is that?

Schneiderman: Whatever that is. Nate Mantel had been, as I remarked to you, involved in these insecticide-pesticide experiments. I had gotten economics at Ohio State and Harvard and --

Baker: And the military.

Schneiderman: --and the military experience, and some as an undergraduate at a time where we were still at the tail end of the Depression and jobs were very hard to get and here I was in a school that was free with no prospect for a job, and I convinced my family that it didn't cost much if they let me stay in school and take every possible course that I could, which I did. So I think we came with a wide background to bring different fields together

and to be able to accomplish some of this integration. I am very concerned about seeing how intensely specialized people have become.

Baker: Well, you know, the competition through publications and competing for grants makes a lot of pressure on investigators to be at the very forefront and be up to date on the latest information and it tends to make it narrow specialization, and there are very few people who are involved in integrating across these various lines, and this is again why I used systems networking to develop a budget for the Cancer Institute. I wanted a device that would help us decide priorities, and so I focused on trying to keep up with the main thrusts in these various disciplines. And, of course, you can't compete experimentally that way at all. But I was helped out of the lab, so to speak, by my allergies.

Schneiderman: By the mice.

Baker: But I'd always been interested in looking across the whole spectrum here, and I thought that interest in diversity and experience helped me to develop good budgets for the NCI.

Schneiderman: Well, it certainly helped you when you were working with the Ludwig people.

Baker: Well, I didn't do as much formal planning, but I'd already done some of the planning so I knew how to weigh one area against the other.

Schneiderman: Well, you know, politically you hear people talk about the law of unanticipated consequences, meaning that things happen that you don't expect.

Baker: Well sure, you always allow for that.

Schneiderman: You've got to allow for that. But more things are going to happen that you don't anticipate if you don't know very much, if you only know your narrow area.

Baker: Yes.

Schneiderman: And, as you point out, this need to be at the forefront of your narrow field in order to get grants has this negative effect, and I think it's really kept out people who might be generalists--

Baker: Well, my point was that that's not all there is to it. I was as good a supporter for the grants system for exploratory research as anybody, but I thought we had other things we needed to do that involved multidisciplines.

Schneiderman: We needed some Leonardos. We really did.

Baker: Well, cancer problems often require multiple disciplines, and the Chemotherapy Center, of course, was a good example of where you brought those together. And so Endicott's philosophy of integrating rubbed off on me, but I took it further, I think, in trying to formally use systems planning, but it's not very popular.

Schneiderman: It's interesting, you know, Irene, my wife, is a social worker and her approach to what she was doing in social work developed out of the group here at Georgetown and they essentially were systems planners. Social work from a systems point of view is what Georgetown taught. I'll remember the name after a while of the guy who did it [Murray Bowen]. But I think I'm with you; I think you've got to look at this as a large integrated system.

Baker: Because cancer is that.

Schneiderman: Yes.

Baker: And so are your cancer programs. And I was talking about program planning, which had nothing to do with trying to tell a scientist how to do his experiments. You know, people are afraid of planning because they think you're going to tell them what to do, but that wasn't the idea at all.

I'm amazed how many people didn't see the difference between program level and project level.

Schneiderman: Well, I think it's part of the training that we get when we're students. You wind up doing your doctorate on a specific, narrow problem.

Baker: Well, you have to. And in statistics and epidemiology it's even worse because to collect data it takes a long time and one guy alone can't do it. So, training in epidemiology and statistics is a very complicated subject.

Schneiderman: And you commit a lot of your life to a small number of problems.

Baker: And you can't get a Ph.D. unless you narrow it down. You just haven't got time, or resources. I mean one of the great advantages, I think, of being at NIH was--and how we competed with lower salaries--was that there were resources that could be made available, and part of it was collaboration with people right around you.

Schneiderman: Yes. I found the collaboration marvelous. I'd remarked earlier that I did some work with George Brecher, with whom I'm still friendly, even though George is out on the West Coast. Well, after I'd worked with him for a little while, I found that he had been a math major at Heidelberg when there were rumblings of Hitler. George's father--George's father was a Czech--but intriguingly was an American citizen--I don't know how that happened--George's father said to George, "You've got to get out of Germany, you've got to get out of Heidelberg and," he said, "I think you've got to get out of mathematics so that wherever you are, you can make a living."

Baker: I never got to know George Brecher, unfortunately, but I know Endicott was a great admirer of George too.

Schneiderman: And that's what George did. He got out of Heidelberg. The family sent him to medical school in Switzerland. And he never went back to

Czechoslovakia.

Baker: Which school in Switzerland?

Schneiderman: I don't know.

Baker: You don't remember?

Schneiderman: But he went to medical school in Switzerland.

Baker: Well, we'd better move on, I guess. Briefly, the rest of the time you were at NIH and what are you doing now?

Schneiderman: Well, I left NIH in 1980, in part because the retirement prospects at that time were extremely good. I would get a much larger pension than had I stayed on a couple years. Financial things were very important at that time. I had been working with--oh gosh--I'd been working--I don't know whether I should say I was working with, or against, Vince DeVita at the time. Vince and I had strong opinions on almost everything, and we generally had different opinions. And I had gotten caught up in the fluoridation controversy at the time and the anti-fluoridationists sued me for libel, slander and so on. I must say the Cancer Institute and the Justice Department were very supporting.

Baker: I didn't know about this. I guess this happened when I was in Switzerland?

Schneiderman: Yes, I think so. They were very supportive and the case got thrown out of court eventually. But I guess I was getting to feel restless again. I guess I'd been restless all my life.

Baker: Well, that's why you get a lot of things done.

Schneiderman: Maybe. So I took the very good retirement and I went to work for a group called the Environmental Law Institute.

Baker: Called what?

Schneiderman: The Environmental Law Institute. They were concerned with

environmental issues and it fitted in with what I was doing in Epidemiology. In fact, I'd been permitted, before I left, to spend a day a week with them to bring what knowledge we had of environmental factors in cancer to them so they might be able to work on it. What they were doing, largely, was writing legislation for states on environmental issues. It didn't last terribly long, my being there. I didn't really have too much to contribute to them. I gave them what I could and they were able to run well without me. I worked then for a private consulting firm, Clement Associates, which really was a spin-off of EPA. A bunch of people had pulled out of EPA and established this company which could do things that EPA couldn't do by way of, again, investigations of environmental issues. And I stayed with them for a while, maybe 2-3 years, and again I didn't see myself-- I was much spoiled by NIH. NIH had a research problem and you fought it through to the end, until you got some reasonable conclusion. If we were doing some consulting with this firm for some private firm, the private firm had a budget of \$200,000 dollars and that's what you had, and when you used up the budget you were finished, and this left me most dissatisfied. I didn't like leaving the problems up, what I thought, were up in mid-air just because we'd run out of money. I most liked the resources NIH made available to me, the physical and intellectual both. So, then I went on to the National Academy in the Board on Environmental Studies and Toxicology because I thought I could help them write the various reports they were doing on environmental issues. The first one I worked on was with Tom Chalmers on smoking in aircraft.

Baker: Well, he's an old friend of controlled clinical trials.

Schneiderman: Yes. Oh, yes.

Baker: He's dead now, you know.

Schneiderman: I know. Tom was chairman of the group writing the report on smoking in aircraft which led to, essentially, the non-smoking now in airplanes in the United States. And I worked on several other problems there up until last year. And since then I've been doing occasional consulting, but nothing terribly much. I did something for the State of Maine recently. We summer in Maine, so I'd gotten interested in some of the environmental issues in the state, and I was on one of their advisory boards.

Baker: Why don't you write a couple papers on the need for integration for understanding biological systems including cancer?

Schneiderman: I don't think I know enough.

Baker: That's all right. You've got a good head on you still.

Schneiderman: It's a good idea. I may. We have a computer up in Maine.

Baker: You see, when I said I admired you guys in the Math/Statistics area, it's because you guys can think clearly and not everybody can, and you're used to thinking, and I've found thinking is like weightlifting; the more you do, the easier it gets.

Schneiderman: The easier it gets. Yes.

Baker: So, it would be a waste for you not to keep your brain functioning.

Schneiderman: Have you ever run into any stuff by a guy by the name of John Paulos? P-A-U-L-O-S?

Baker: Yes. I've read two or--

Schneiderman: *A Mathematician Reads the Newspapers?*

Baker: I read his book on *Innumeracy*.

Schneiderman: *Innumeracy*. Yes. It's interesting, because I wrote a paper for the 40th Anniversary of the Cancer Institute, I wrote a paper on the numerate

sciences. That my title, "Biometry and Epidemiology, the Numerate Sciences."

Baker: Yes, I enjoyed reading it. Well, very good.

Schneiderman: I just sent a clipping to Paulos, by the way.

Baker: He teaches across the river here somewhere.

Schneiderman: Temple. He's at Temple.

Baker: That's right. There is another guy at George Mason University named Trefil who's pretty good on science for the layman, but it's not mathematics.

Schneiderman: Oh really?

Baker: Yes. That's right, he's a physicist.

Schneiderman: There was a piece in last Sunday's *Washington Post* by Senator Hollings, ostensibly by Hollings--I'm sure one of his staff people wrote it--and it talked about something being sold at 360 percent below cost.

Baker: Well, that's a pretty good trick.

Schneiderman: The only way I figure out you can do this is if you buy it, they pay you. So I sent the whole clipping to Paulos.

Baker: Okay. We'd better turn to the questions that I sent you.

Schneiderman: The first one says, "Your views as to the five or more most important scientific results highly significant to the Virus Cancer field during the period '50 to '80. Indicate the key scientists involved in each result."

Baker: Now, I realize you were not in the middle of that program, so some of these questions are not exactly appropriate, but your impressions are still important as viewed from outside those running the program.

Schneiderman: Well, as I try to recall it, there were two things that I was concerned with in the program. One was I didn't see how it related to what the epidemiologists were doing. I remember pushing Bob Huebner, saying

to him several times, "Bob, what should we be looking for in human disease to be consistent with the virus etiology?" I never got any help on this and we never did anything.

Baker: Well not quite. Maybe you had more effect than you think because, unlike most of the people, he did trap mice in Maryland farms and in Brooklyn houses because he realized that you needed that kind of information in addition to the laboratory side.

Schneiderman: You know I'd forgotten that.

Baker: And, in fact, I think we all got fooled by being overly impressed with the more than 200 viruses that caused animal cancers--yet most of it had nothing to do with man. And Bob Miller wrote a paper we should have paid more attention to on the epidemiology, and he pointed out early on that you didn't have any data that indicated it was transmitted horizontally.

Schneiderman: Well, this, as I said, I was reflecting--When I spoke to you I was reflecting Bob Huebner's position, Bob's thoughts on this, because I used to talk to Bob quite extensively.

Baker: So that's taught me the lesson that you can get a lot of results by how you manipulate the experimental design you set up in the laboratory, and it may have nothing to do with what's happening in, shall we say, the real world.

Schneiderman: Yes. Well, the laboratory is real world too, in a different sense.

Baker: But they're different.

Schneiderman: That was my major thing. I remember, didn't John Moloney have a leukemia virus, a viral leukemia, that he worked with?

Baker: He had both a sarcoma and a leukemia, two separate viruses.

Schneiderman: Yes. I remembered the viral leukemia. I remembered the leukemia and I

thought it was viral leukemia, which seemed to me-- Interestingly, John set me to looking at some human data which left me very unhappy because of the peripheral political fight that was going on at the time. I was looking, trying to find these human data that John sort of was pushing me toward, and discovered that the State of New Jersey for several years did not record race on the death certificates, and they didn't record the race because of the civil rights push at the time and, of course, it was extremely important. It may not have been race--it may have been class--but race was often a surrogate for class. It was much more difficult to get class information on a death certificate, but you could get race information. And I was really upset by this. People were ostensibly trying to make the world better and not have racial prejudice and all kinds of good things which backfired.

Baker: Yes, a lot of conflicting things. Well, were you involved in looking for clusters, on whether clusters indicated horizontal transmission?

Schneiderman: Nate Mantel actually developed some theory on clusters. And we were looking at so-called leukemia clusters and breast cancer clusters, and things of that sort. We'd come to the conclusion that almost all the clusters that we investigated could almost be called coincidental.

Baker: That was what I learned from all this.

Schneiderman: Yes. There is so much potential for clustering that even when you found a cluster it usually didn't mean anything. In fact, I don't think we found one cluster that had any real meaning to it, except perhaps from industrial exposures, some of the stuff that we looked at in northern New Jersey. There the clusters were meaningful.

Baker: Well, I guess you appreciated that the Viruses Cancer Area, before Gross and Bernice Eddy and Sarah Stewart's work nobody thought viruses had

anything to do with cancer causation, but with the Gross finding, once it got confirmed, it changed the whole outlook so that now the outlook was that viruses caused cancer, and certainly in many cases it did, and that view held until the oncogenes came along, and now you've switched from looking at the viruses to looking at the genetic information, and the oncogenes being present in our own chromosomes has changed the whole outlook again in this area.

Schneiderman: There was one other thing that I remember--

Baker: But would you assume that those were the main thrusts?

Schneiderman: Yes. Clearly. Plus one other thing. It seemed to me the laboratory people were in the process of developing some very interesting tools, the so-called "probes," which I thought was a substantial advance and actually made possible for much of the work in genetic engineering that people are attempting now. Those are my recollections of what I thought were important and some of the things I thought were "unhappy," as it were.

Baker: Okay. The second question turns to the administrative and managerial aspects, I believe.

Schneiderman: "What do you think are the key administrative and management decisions affecting the virus cancer field and who made them?"

Baker: Again, you may not have full information on that.

Schneiderman: I thought the key thing--The one note that I made for myself was about contracting out a lot of the work, getting people to provide -- outside NIH -- an extension of the intramural activities and facilities. I remember it also created a great row. People said that, again, Bob Huebner was expanding his own activities enormously in competition with Stanford University or the University of Chicago, or Harvard or

whatever.

Baker: Yes. The Zinder Committee was very concerned about that and that really, as Moloney puts it, led to the demise of the program.

Schneiderman: Did John think that the contracting out led to the demise or the criticism of the contracting out led to the demise?

Baker: Well, to some extent both. There has always been concerns of particularly the academic community scientists that that money would be better spent in grants, and contracts was too confining on what you were allowed to do, for example, and that depends, of course, on how the contract was written. But anyway, that--

Schneiderman: I remember there were academic people who thought that this was unfair competition, that Huebner and his people were being so much better supported than anybody else.

Baker: Yes. And that led to great problems. Now, I always related Huebner to General Patton. If you wanted the job done, why shouldn't he organize a group of people working together, which he did, mostly on the West Coast, very effectively, but you had to not let him have all the gasoline, like Patton. If Patton had had his way nobody else would have had any. And Huebner was kind of like that. But, I mean, you could control that by budget.

Schneiderman: There were great entrepreneurs in there in all sorts of fields

Baker: Yes. And he was one of them.

Schneiderman: I think of Bob Good, whom I haven't seen or heard of in years. When he went to Memorial Sloan-Kettering, he too seemed to be gathering in everything.

Baker: And that's not very popular. It makes you think that the criticism about fairness can interfere with getting a lot of work done.

Schneiderman: Well, you know, they're both valuable things and sometimes you have to balance the work done.

Baker: Yes. That's how it is. You can't go too much in either extreme. Well, very good.

Schneiderman: And your third question. "What do you consider to be the main activities and effects of your participation in the field during this period?"

Well, essentially I thought the only impact I had was attempting to press for looking at clusters and seeing if they gave us evidence of horizontal transmission kind of thing. I do remember--and at the time I thought it was a cop-out, but that's just because I was probably feeling snide at the time--people saying, "When we talk about viral cancers, we don't mean viruses like the cold virus." I didn't know what they did mean, what they were talking about. I didn't understand this. And in not understanding it, I had two choices. I had to say either I've got to learn more so I can understand it, or I could be snide about it and say it's something not worth understanding. And I chose the lazy man's route; I was snide.

Baker: Well, as I say, I realize you were not right in the middle of all this.

Schneiderman: No. I wasn't in the middle. "Who do you think were the main leaders who influenced the direction and course of events in the Virus Cancer field?"

Baker: Here I'm talking about science politics, in a sense, like Sidney Farber would be a person who--

Schneiderman: In chemotherapy.

Baker: In chemotherapy. But he also was supportive in the virus area. And, of course, Mary Lasker is a very important person in the general area of support. But scientists like Joe Melnick were very helpful on advising. So, some of the advisory people, I'm talking about, who provided

leadership, not in the strict science sense, but on how programs got developed and supported. Does anybody come to mind there?

Schneiderman: Nobody comes to mind for me other than the names I've already mentioned. I thought of Moloney and I thought of Huebner. I guess I thought of the people I was sort of physically close to.

Baker: And you may have sat on some committees, on either a review or advisory--

Schneiderman: I was on some NAS committees at this time which might have been related, but I really don't remember them.

Baker: But you weren't in on any of the internal viral committees and who was on committees that come to mind? You weren't involved directly in that?

Schneiderman: No, I wasn't. "How aware are you of the membership of key NCI or NIH committees?" Oh, you've just been talking about that.

Baker: So the next one is on resources?

Schneiderman: "How significant is the availability of quality controlled resources such as tissue cultures, cell lines, virus preparations, antibody preparations?" Essentially, using none of these, having no need for them--

Baker: But you just already said that some of these methods that were involved in developing these allowed biotechnology to evolve.

Schneiderman: Yes.

Baker: So it was not only the methods but the resources that resulted from them.

Schneiderman: I was thinking of myself in this area not as a scientific or technical person but as an educated layman, in which I thought that some of the things that people did in the laboratory opened up avenues for other kinds of research.

Baker: They certainly did. But my question here is about the resources'

availability themselves, because there was a lot of education about whether industrial people could make virus preparations, for example, of sufficient quality. There was an academic scientist who often said that industry could not produce viruses, etc. of sufficiently good quality.

Schneiderman: A little academic snobbery in some of this. In fact, there was enough academic snobbery that at one point I found that when somebody would say "basic research," I would get a negative response.

Baker: Well, I never quite got that far.

Schneiderman: That didn't last terribly long but it did--

Baker: Serendipity, I think, is overdone.

Schneiderman: It was there for a while, really. What are you talking about? Are you talking about what you're having fun with in the lab, or it's intellectually interesting and exciting to you, but what does that have to do with what I'm interested in? What's that got to do with the disease that I know of, that my friends and my wife and other people have had?

Baker: My answer, of course, was to make sure that you defined cancer research very broadly so that you could cover both ends of the spectrum.

Schneiderman: Well, I suppose you succeeded in defining it sufficiently broadly so that a lot of the AIDS work was done in the Cancer Institute.

Baker: Well, it probably still is.

Schneiderman: And it's a disease of viral etiology, except for what's his name who says it's all excess drug use. Gosh, I'm losing all the names [Peter Duesberg].

Baker: Yes. Well that's the biggest problem we have, I think, in trying to reconstruct the history here. It's hard to remember the names. So, I will be checking out a lot of this from some of the documentation. Let me elaborate on this business of quality. A lot of the very good virologists, some of whom had come out of the polio area, and we were working

with them in cancer, said that industry can't make the stuff good enough. And I said, "Well, you guys have done very well in exchanging samples when you develop new virus preparations and antibodies and what not, but by the time you send around samples for testing, you haven't got any left to work with. You need bigger batches and industry can do that." "Well, it won't be good enough." And I said, "Well, we're not going to make you use something that you don't think is any good, so you can test the stuff the same way you tested your own stuff." And I knew I was over the hump when Moloney came in one day and said, "We just got a big preparation of virus from Pfizer and it's as good as anything we've ever made, and we've got buckets of it." And, so that got them over the hump where they were willing to accept the new way of operating. Now, again, I learned from the Chemotherapy Program that industry can do a lot of things you can't do; so use it.

Schneiderman: Sure. There's no question that industry can, as you pointed out, make large quantities of stuff and they can make it pure if they have to.

Baker: In the interview with Varmus, he admitted that the quantities of the virus that were supplied through the Viruses-Cancer Program were very useful in getting the oncogene information spelled out, but that's as far as he would go in thinking this Program was helpful, I think. And I read in a book recently that Baltimore, who also received sizable quantities of virus, made a crack that it wasn't as good a stuff as they had made. So, it's interesting. I'm trying to get some feel for how important those resources are.

Schneiderman: It seems to me what it led to is almost all the work that people are now doing on AIDS.

Baker: Yes. I think so, both the methodology and some of the materials. And

also, a lot of the materials that we made in the Program are now available commercially, of course.

Schneiderman: I think it made it possible-- It really made it possible for a lot more people to do research in the field whose strengths were not in making the purified materials, but in using them and exploiting them. That I can mill good flour and get you nice flour still does not give me a good loaf of bread until I get a baker who knows what to do with it.

Baker: But, on the other hand, you can't bake the bread if you don't have the flour.

Schneiderman: That's right. You know, that was my point.

Baker: And so this is the flour we're talking about.

Schneiderman: This is the flour. In many cases this is the flour.

Baker: And it's a question of whether commerce should handle this, or the Government. As I think has been demonstrated, on the more fundamental end of the spectrum, the commerce is not going to do it normally unless they can see a market for it. And you have to do some developmental research before you ever get to that stage. And unless the marketing looks very promising, it won't get done. And the universities won't do it.

Schneiderman: Yes. Well, the universities get into things like Warfarin, or something like that, that the University of Wisconsin got into, but that's very rare. Most of the university administrations, although they want money for the university, think there are lots of things a university ought not do.

Baker: Well, there's a real change, of course, in the biological and medical field of academic scientists now getting involved directly in commercial ventures and allowing NIH scientists to receive patents. This has changed drastically, I think, the money aspects of research. I'm not sure

it's for the better.

Schneiderman: I wrote myself a note that now I can't read. "If you could have changed anything in the Virus Cancer Program field as it developed, what would you have liked to have changed and how?"

I think we really have talked about this. I wanted the lab people to--

Baker: More epidemiology?

Schneiderman: --be more epidemiology oriented, and the epidemiologists to be more treatment oriented. If these are viral diseases, what do we know about treatment of viral diseases?

Baker: Very little in those days.

Schneiderman: Very little. And we know very little now.

Baker: Well, we at least have some drugs that are antiviral. We didn't have any in those days.

Schneiderman: That's right. And it seemed to me that that was a missing piece. If these were viral diseases, we should have attempted to develop antiviral drugs and tested them and I don't think we got into any of that.

Baker: Well, it got started, but it didn't go very far, partly because we didn't have any success. There was a small program seeking antiviral drugs as part of the Program, but almost all results were negative. So it wasn't that there wasn't any trying to do it; it didn't grow much because we didn't have very good leads. And, you know, to set up a screening program the size of chemotherapy in viruses, when you had no drugs, it didn't seem the thing to do at that time. Well, let me turn this thing around. I felt that the epidemiology people were too far away from the lab scientists.

Schneiderman: I think you're right.

Baker: And Wynder and I met over breakfast in the Zurich airport. I talked to him about this problem of needing to get the biochemistry wedded to the

epidemiology. So that led to the biochemical epidemiology, which is now called molecular epidemiology. And I think there has been some real improvement on that. Fraumeni, in part, has been sympathetic to this, I think.

Schneiderman: You know, lots of people were quite critical of Ernst Wynder and called him a "promoter" and "self-aggrandizer" and that sort of thing. On the other hand, he was one of the very few people who tried to integrate the human stuff with the laboratory stuff.

Baker: And also educate children about health. And he founded *The Journal of Preventive Medicine*.

Schneiderman: I thought Ernst was a very effective person, attempting to do things that other people ought to attempt to do.

Baker: I was and am a backer of Wynder.

Well, we're getting pretty much down the line here.

Schneiderman: "If you could have changed anything in the Virus Cancer Program field as it developed," we've talked about that. "Do you believe the developments in the Virus Cancer field laid significant foundations"--oh yes--"for molecular biology and biotechnology?" Absolutely.

Baker: Yes. That's a loaded question, but I think it's important to bring up.

Schneiderman: Absolutely true. Not only the initial thing that I talked about, the probes sort of thing, but when I think about all the AIDS research that's going on now, where even in AIDS we have some moderately effective drugs, moderately effective antiviral agents.

Baker: Well, we certainly can follow what's happening in the cells upon infection with techniques that--and Wally Rowe and Dulbecco's quantitative measurements was a very important step forward in quantifying these things accurately.

Schneiderman: Yes. And, of course, with no immodesty whatever, in some of the controlled clinical trials in AIDS they are using some of the techniques that I developed with Armitage.

Baker: Yes. Sure, because that's going to stand. The next one is about--

Schneiderman: "How do you think the political climate and public knowledge and opinion affected scientific progress and funding, including considerations of the Virus Cancer Area, in the '50s and '60s?" There I have mixed feelings. I remember visiting friends and family and traveling around the country and laymen asking me questions about this. You know, "How are you doing on this? What progress are you making?" And very often I got back the feedback that we were overselling. People were saying to me that, "You mean that's all we've done? You know, I read in the paper that..." It's so hard to not oversell.

Baker: Well, I was always very careful when I was Director not to promise. I pointed out here were opportunities for expansion. I was very careful not to over-promise. But I think DeVita made a mistake in saying we'd have something by the year 2000.

Schneiderman: Well, he was following Dusty Rhoads.

Baker: And then, of course, Mary Lasker and her group, I think that's where the overselling came from.

Schneiderman: She was always overselling.

Baker: And it can backfire.

Schneiderman: Yes. On the one hand, you have to have something positive to sell or people don't buy it and you don't get any money.

Baker: Well, there was always enough there. It was how you presented it. For example childhood cancers are certainly--and testicular cancer now and leukemia and so on--are being cured. And so you could point to some

things. And now, in understanding the pace of new findings for understanding what's going on is magnificent these days. It makes you wish you were back there.

Schneiderman: Yes. As I said, there's the book of abstracts. I started looking through it and 99.44 percent of them I don't know what the researchers are talking about.

Baker: Well, it's hard to keep up with the jargon even.

Schneiderman: It sure is.

Baker: Well, I guess we're down to the last one, which is, is there anything else you'd like to say?

Schneiderman: I'm delighted that you've come over and spoke with me about it. It's great.

Baker: Well, we both had a wonderful time at NIH.

Schneiderman: We had a very good time at NIH.

Baker: I don't know. It's very hard to evaluate how good NIH is now compared to then, because we're not really there. I've been a little worried that it seems like a lot of scientific decisions are being made politically now, more than they used to, and Congress is micromanaging too much.

Schneiderman: Again. You know, they were micromanaging early on and then sort of got talked out of it. I thought Rod Heller was one of the most marvelous testifiers before Congress. I remember going once or twice when Rod was testifying, and he was always very low key and very gentle, and the southern accent in the speech came out stronger when he was testifying.

Baker: Well, he had a great knack for relating to people as soon as you met him, and he got things done because people felt like helping him. And he had a marvelous skill. It was not a skill it was just his nature I think.

Schneiderman: He gave you the feeling that he was the family physician to the whole

Congressional committee, and he would consider their problems and their issues and their troubles and he would do something that would be useful to them.

Baker: Some people, of course, thought he didn't do enough in cancer substance, but he didn't--

Schneiderman: But he wasn't a cancer specialist really.

Baker: He got things done though anyway.

Schneiderman: He was an administrator.

Baker: And people wanted to help him. Yes.

Schneiderman: It was his job to get money. It was his job to get staff. It was his job to get space in buildings. And, by God, he did it. He did it extremely well.

Baker: Mike Shimkin, you know, preferred to have had somebody that was a great cancer guy I think.

Schneiderman: Well, Mike was a very good and competent and exciting guy to work for. Mike was great fun.

Baker: I enjoyed him tremendously too.

Schneiderman: And I enjoyed Mike, and he was very supportive of what we were doing. It was good knowing Mike and knowing what he was doing and having him do what he was doing.

Baker: Another example of the wonderful people we've had the pleasure of working with. Well, thank you very much, Marvin.

Schneiderman: Well, thank you, Carl. As I said, I enjoyed your coming.

*Interview Conclusion*