UFHC 61
Interviewee: Kenneth I. Berns
Interviewer: Nina Stoyan-Rosenzweig
Date: March 10, 2003

S: This is Nina Stoyan-Rosenzweig, and I'm here at the Mount Sinai School of Medicine with Kenneth I. Berns, M.D., Ph.D. It is March 10, 2003. Dr. Berns, I guess we should start essentially chronologically. So can you tell me when and where you were born?

B: I was born in Cleveland, Ohio. on June 14, 1938.

S: Can you tell me something about your family [and] the circumstances of your childhood?

B: My father was an ENT [ear, nose, and throat] physician, who was in private-practice before World War II. He went into the service shortly after Pearl Harbor, so he was gone a fair amount of the time, between ages three and six. My mother fundamentally was a homemaker.

S: Did you have siblings?

B: I have one sister who was born in 1944.

S: So, that was after the war, apparently.

B: Well, no, remember the war lasted until 1945. [She] probably was conceived just before my father was shipped overseas.

S: ...with a father who was a physician, did you grow up wanting to go into medicine?

B: I think I sensed that I was interested in medicine sort of subconsciously for a long period of time. I guess I never thought seriously about doing anything else.

S: So, then, that wasn't a situation where there is some inspirational person in high school who guided you or anything like that.

B: Not in that sense. I actually had several extraordinary teachers in high school. I had Frau Deutsch, who was my German teacher, interestingly enough, who was a Ph.D. and a very scholarly woman. [She,] for reasons that are unclear to me, took a great interest in me and pushed me a fair amount; and I had an outstanding eleventh-grade English teacher, Grace Graham, who was a fine person.

S: What type of high school did you attend?

B: I attended Shaker Heights High School, which was a public high school in a very wealthy suburb of Cleveland. It had a reputation of being an equivalent of a private school with a private-school setting. In my graduating class, we had seven of us who attended Harvard, and a fair number of people who went to other Ivy League schools, who went to Williams and Amherst as well.

S: It sounds, just judging from the fact that your teachers had Ph.D.s and that sort thing, that they really attracted top-notch [teachers].

B: I always felt it was a very high-quality experience.

S: Did you ever watch your father's practice, obviously when the war is over and he comes back. Did he go back into private practice?

B: Well. no, maybe very briefly. Fundamentally he went to work for the Cleveland [office of the] Veteran's Administration. He worked for them essentially for about thirty-five years after the war, until he retired. He also spent a lot of time with the reserves.

S: So his military experience was important to him.

B: Oh, it was very important. In fact, it was interesting to me that he lived quite a long time, until he was ninety-two, but he suffered fairly severely from Alzheimer's disease. The last thing he remembered about his life was that he was the colonel.

S: His memory sort of stopped with the military service.

B: Well, he continued in the military until he was retiring because of his age, probably around sixty.

S: You had military service as well, but I guess that's jumping ahead.

B: Well, I wouldn't quite call it that.

S: Well, that's jumping ahead a little bit. [While] I was just wondering if growing up in family steeped in that affected you in some way.

B: Well, I had lots of experiences with it, because when he was gone on active duty, I was in my teenage years [and] I would go with him. So, I had some interesting experiences in the military. We were in Fort Knox in July 1950, when the Korean War broke out. That was a pretty exciting couple of weeks.

S: I can imagine.

B: I was twelve at that time, so that made a major impression on me.

S: You said, also, that seven of the people in your class went on to Harvard. What made you decide or think about Harvard?

B: I think that I was a good student and Harvard had a reputation for being one of the best universities. I had some interaction with the local "Harvard people." I was admitted, [without] too much difficulty, as it turned out, and went off to Harvard.

S: You started in September 1956?

B: That's right.

S: Can you tell me anything about your experience at Harvard?

B: Well, that was a rather interesting, I'm sure very informative, experience from my point of view. I think the thing that excited me about Harvard was that the faculty who were teaching us were really at the leading edge of their respective fields. Another thing that happened at Harvard was that the most senior faculty at that time taught the introductory courses, so you really had the feeling that you were at the right place. That actually started, interestingly enough, at orientation week. Somehow, in a very low-key manner, the university was successful in having you reach a point where you wondered why you had ever considered going anywhere else. It was [a] kind of interesting turn of events.

S: Almost an indoctrination?

B: Well, it was, but very subtle. I didn't feel pressured by any means, although it was interesting. I remember the provost at that time, or I don't know if you called him provost, he was the Dean of Arts and Sciences, McGeorge Bundy [National Security Advisor to Presidents John F. Kennedy and Lyndon Baines Johnson]. He already had a very strong reputation, so I think that was very impressive to all of us.

S: Just to know that people who were there or teaching really were actively involved in government...

B: ...and doing it, right.

S: How were the senior faculty in terms of their teaching skills?

B: Most of them were actually pretty adept, as I remember. One of the more interesting professors I had was Oscar Handlin, who taught a course in American social history. He was an extraordinary figure. He was not a great lecturer, but the thinking was sufficient to sort of make you forgive his inefficiencies in lecture.

S: You felt you were getting something out of the experience, in other words.

B: Right, in fact there was a very famous sociologist at that time named David Reisman who wrote, I think, *The Lonely Crowd*. It was a classic book. He was new on the faculty at that time, but I remember that he took the course at the same time that I did. I thought that was kind of interesting.

S: So your peers then were also interesting.

B: They were, the people I went to school with were interesting. I ran into several of them later on in various situations, so I think that overall it was a very good experience from my point of view.

S: You went in essentially as a pre-med [pre-medical school major]?

B: I went in as a pre-med. After I bounced around a couple of times, I finished as a pre-med, but, of course, I didn't really finish Harvard; I left after three years to go to a special program up at [Johns] Hopkins [University].

S: What was that program?

B: Well, let me just tell you, I had an experience at Harvard that was very formative from my point of view, and that was that, in my junior year, I had a tutor named Jack Fresco, who was a senior post-doctoral fellow in the laboratory of a great physical chemist by the name of Paul Doty. I did very well that fall in a course on genetics, which impressed Fresco. So, he made me start doing some unusual things. He and I both audited a course by a great biophysicist named Edsall. Then, he said, it's possible that I can get you a fellowship for this summer from the NSF [National Science Foundation]. It had just started a program for undergraduate research in the summer. So, he eventually did get me a fellowship to work in Doty's laboratory. Interestingly enough, one of my classmates who had the same fellowship was a fellow named Bruce Alberts. He, today, is the president of the National Academy of Sciences. I would say the associations were very good and there were many other young people in the laboratory who went on to very distinguished careers in science.

S: So this would have been the summer of 1958?

B: [It was] the summer of 1959.

S: So you did have undergraduate research experience?

B: Oh, yes, actually, by most standards, it was quite extensive, as it turned out. At the end of that summer, I went off to Hopkins [to] the first year of a new program that they had at called Year One program. That program had been started by another great man named Barry Wood, who had a Harvard connection as well. Barry Wood was Harvard's last all-American quarterback. [He] was a world-class football player and a brilliant medical scientist. He went to medical school at Hopkins and ten years out of medical school, he became chairman of medicine at Washington University in St. Louis, and probably developed the best academic program of medicine in the country at that time. Then, he came back to Hopkins, was the vice-president for health affairs, and after doing that for five years, which he felt was a waste of his time, he was fortunate enough to become the chairman of microbiology, which was what he really loved. So, he had developed the Year One program, because this was shortly after *Sputnik* [Russian satellite that led to worries that led to the "Space Race"] in 1957. He was afraid that too many bright, young people were going to go into science and [would] not be associated with medicine. His intention was to direct people into his program. We had a small group of around twenty-five of us in the first year. One of my classmates was a fellow named Steve Rosenberg, who currently is the chief surgeon at the National Cancer Institute and is a rather outstanding individual. Another member of that group was a woman named Alice Huang, who is an outstanding microbiologist and is married to David Baltimore, who is the president of Cal Tech [California Institite of Technology], and a Nobel Laureate. Alice and I, today, both serve on the board of trustees at Hopkins. So, they've given us our long-term connections.

S: What was that? Could you just describe the Year One program?

B: Well, it was really fascinating. It was really meant for people who had two or possibly three years of college. Since I'd had three years of college, I didn't have to take many required courses, because I'd had most of them. So they, in a sense, let me [set up] my own curriculum. As a consequence of that, I fundamentally spent the whole year doing research in a laboratory. Charles Thomas, who was a professor of biophysics at the Homewood campus of the university, had gotten a Ph.D. in Paul Doty's laboratory at Harvard, and I met him just before I left the [duty] laboratory at the end of that summer. So, we had a very good time, working together, for about a year. [We] published two major papers [together] as a consequence. I've actually always felt my career has gone down-hill ever since then, because the two papers were published in one of the first issues of the *Journal of Molecular Biology*. They were immediately adjacent to a paper written by a couple of Frenchmen named [Jacob] and

[Monod] which had to do with the operator theory for gene expression. [Jacob and Monod won the Nobel Prize for this work.] I thought that was very good company to be with.

S: Certainly, and what was the title?

B: Of the two papers?

S: [Yes.]

B: Well, we were very interested in the structure of bacteriophage T-4 DNA. In those days nobody knew how big a molecule of DNA could really be, because the organic chemists and the protein chemists weren't used to molecules of giant size. There was some question as to how big [the] DNA contained in the bacteriophage was. We knew by that point that DNA was the genetic material. There was a very famous experiment by Al Hershey and Marta Chase in which they had shown that the T-2 phage, a related bacterial visus, DNA was the genetic material. So, the real question was, was all the genetic material of that virus in a single molecule? We collaborated with Hershey during that year in a fairly close way. We were using some physical techniques to measure the size of the DNA. So, we were able to demonstrate that it certainly was a lot bigger than anybody ever thought it was. In the second paper, [we] answered another question; by that time, we knew DNA was a double helix, and the question was, were complementary strands of the DNA continuous or were there little gaps in them where the protein occurs? The second paper, I think, demonstrated something that was absolutely correct. It didn't have to be modified at all, ever. [What] that [paper said] was, in fact, that both strands of the double helix in that particular phage were continuous, very long polynucleotide chains.

S: So not only were you in good company with these first papers, but they were important.

B: Well, I think that at that time, they were pretty significant.

S: You answered some very important questions about genetic material.

B: It actually made me pretty excited, then I went back to the first year of medical school, which was not quite so exciting, in all honesty. I think I just managed to scrape through that year, to be truthful. Before the end of the first year, I decided that I really wanted to do a Ph.D. in molecular biology, essentially. So, I received a leave of absence from the medical school to go over to the main campus and work in the biology department with Charles Thomas to do a Ph.D.

S: You did a Ph.D. isolating DNA?

B: Yes, we thought, well, the DNA virus is intact, maybe we'll do the DNA [in a] bacterium, which was somewhat larger. That turned out to be a really significant challenge, because getting the DNA out of the bacterium was much more complicated, simply because [the bacterium was] a more complex structure. We knew that DNA was a stiff rod that was fairly fragile, so getting it out without breaking it was challenging. We did some biochemical work where we got it out and demonstrated that there were no more than two molecules, although we were pretty sure that there was probably only one. We did some electron microscopy. [The actual photos were taken by Loren MacHattie] who was [a fellow] working at the lab at that time; it [the DNA] was pretty clear that it was probably a single molecule. At the same time, John Cairns, who was in Australia at that point, using a very different approach to DNA of *E. coli*, was also [finding] a single molecule, or at least a single structure.

S: Well, how did you get out the DNA? What sort of techniques [did you use]? You must have been just pioneering the techniques at the time.

B: Actually, it was partially that and partially adaptation. We, of course, followed the literature closely, and just to give you a notion of how long ago that was, it was long enough ago so that we could read every paper that mentioned the word DNA, which is not something I would try to do today. So, anyway, some people in Australia had noted that there was a particular protease made by a company that seemed to be very good at digesting proteins and allowing the purification of [viral] DNA. So, we thought, well, maybe we should try to apply that to our own situation. Indeed, it allowed us to essentially free up the DNA from protein without really having to subject it to rigorous shaking, mechanical treatment that wouldn't be good for the DNA.

S: Was it possible to sequence the DNA at that point?

B: No, that came much later in my career as a general faculty member [which was when] we got into serious sequencing. You could [sequence only] about five or six bases at that time. I mean, to appreciate how far back that was, when I started in the lab in 1959, we already knew the dogma and so forth for [molecular] biology according to Francis Crick, which said that there was some information-flow from DNA through RNA to protein. So, the mystery was the fact that if you looked at bacteria, the base composition of the RNA did not match the base composition of the DNA, so there was something wrong. What was wrong was that most of the RNA was ribosomal RNA, [which was] structural. Nobody really had figured out how to get at so-called messenger RNA. That didn't happen until I was pretty far along into graduate school. So, the notion of sequencing didn't really happen until another ten years later, you know, in a major way.

S: The techniques and technology for doing it...

B: The people that discovered things about biology made tremendous advances. Sometimes it's just that somebody had a better idea, but I would guess that a majority of the great advances occurred because somebody developed a new technique which allowed better questions to be answered.

S: I kind of think that I would agree. I think, when they think about paradigm-shifts, I think a lot of the time paradigm-shift occurs because suddenly there's a new way of looking at the biology, or being able to see what's happening.

B: Well, I think that was certainly true with the case for [reverse] transcriptase, and everybody said, gee [whiz], you can make DNA from RNA, finally Francis [Crick] was wrong. Francis said, "wait a minute, if you go back to the original papers, that's a double-headed arrow between DNA and RNA," which was true, of course. He invented the dogma and he invented the triple code. There could be some argument that other people may have had similar ideas, but his contributions were really extraordinary.

S: In terms of your own Ph.D., you received your bachelor's degree in 1969 and your Ph.D. in 1964, but you finished your first year of medical school.

B: Right.

S: So, you did a Ph.D. in three years.

B: Right, actually I was the first person from my major, biochemistry, to do that. It was a little unfair, because I had a head-start because I had spent that year, 1959 to 1960, working in the laboratory doing my thesis. While that was a different project, I didn't have a lot of down-time when I started working in the lab on my thesis project. So, that helped me quite a lot.

S: You had talked about the atmosphere at Harvard and how it was formative, what about Hopkins?

B: Well, Hopkins, which I have enormous affection for, was an extraordinary place. In the first place, as I told you before, they essentially allowed me to do my own curriculum in the first year. I think that's true for places, great places, [that] are secure. As long as it makes some sort of intellectual sense, and they're developed to the point to where they have sensitivity to appreciate what makes real intellectual sense, there's a tendency to let students do what's good for the individual. That was sort of the way I was treated. They were very good to me. The administration really made sure I had all the benefits of being a medical

student all the time I was a graduate student. Then, when I came back, they had developed a curriculum which allowed one to essentially accelerate through the rest of medical school, because there were elective quarters in each year plus the summers. I actually finished the last three years of medical school in about twenty-one months, which was a little intense. It was interesting when I came back, because most of the people knew that I had gone through a Ph.D. program, which was not all that common in those days. They treated me very well. It was a superb experience. I think the thing that discouraged me the first year that I was a medical student was that, I was taken aback by one of our texts, which stated that the genes were complex proteins in the nucleus. Even I knew better than that. It gave me kind of negative view for a while.

S: Medicine as being backward, in some ways?

B: Well, at least the educational component. That didn't prove true. I had extraordinary courses. Biochemistry was [taught by Al Lehninger] and physiology [by] Philip Bard and [Vernon Mountcastte], those [professors] were quite extraordinary. Then, I decided when I was in graduate school that there must be more to medicine than I had seen up until that point. I went back to medical school and, indeed, it was a very positive experience from my point of view.

S: I suppose the people with whom you started [with] had finished by then and were gone.

B: Well, it was very interesting [when] I was a clerk on the wards, in fact, many of the house-staff were my former classmates, and that was very nice. It was all sort of family. Again, it was a positive experience.

S: Were they allowed to intensify your electives that you were taking so you could finish medical school in three years? What was the medical curriculum like at the time other than that?

B: It was the best curriculum I have ever seen. There was a lot of small-group interaction, there were a lot of laboratories in the first two years, tremendous patient contact, and a lot of faculty contact. As I watched the medical curriculum change over the years, truly, in the better places, they started coming back much closer to what we had when I was a student. We are all the product of our experiences, but I really did feel that, as learning experiences, the way that it was set up at that time was really almost optimal. I remember pathology, which has become, in many places, a lecture-intensive course. We sent a tremendous amount of time studying organs that have been preserved, studying slides in small groups with a lot of instructors around ranging from house-staff to the most famed faculty. Every week, we would go up to see a patient who illustrated the

material [we had] studied during the week. It was a very fine experience.

S: How large was the class?

B: I think at that time the class was about eighty students.

S: I do wonder now, just as class size increases, how that affects the curriculum.

B: Well, I don't know it affects the curriculum because I think they do different kinds of curricula over a fairly wide size-range; it does depend in the sense of how many faculty you have. As a faculty member, what I found was, when you see one hundred students, as a faculty member, you lose the individuality of all the students. So there are some things that are an issue. I think they have about 125 students in the class now at Hopkins, which is about the same, a little bit bigger.

S: They're pushing the limit then.

B: In terms of knowing all the students, I'd say that's right.

S: Let me just ask you what Baltimore was like at that time.

B: Well, when I went there, it was segregated still. It was only five years after *Brown versus Board of Education.* The adult wards were segregated at the hospital for another few years. About the time when I went on the wards, after 1964, they had been integrated. I think what was interesting was that the pediatric wards had been integrated in 1927, just because the chair of pediatrics, Dr. Park, had said, kids don't know the difference.

Nobody ever raised a question about that. Which I thought was kind of illustrative. It was kind of a mixed situation. It was still essentially a Southern town.

S: Coming from on high in Boston?

B: Right, it seemed that way, although we heard the reverse from people who came up from the Deep South. They thought it was pretty far north. It was right on the edge. We had a very good time. Since I spent fourteen years in Baltimore, I really began to feel as though it was home.

S: Well, since you brought up the point, let me just ask you what about the class in terms of women?

B: Well, I think that's really an interesting issue to me. In the first place, Hopkins was segregated school. It was all male, yet we had I think at least three women

who were in our Year One class at that point. So, they were the first women to get regular bachelor's degrees from Johns Hopkins, because Hopkins Medical School had been founded with the notion that you had to have a bachelor's degree. So, since they had brought them there without being able to get their degrees beforehand, they all applied to do that. That's what I thought was great about the place. If they had an issue and they had to solve it, they solved it. They didn't get hung up too much on tradition. It [was] certainly a lot easier to have women get a bachelor's degree at Hopkins than at Harvard for a woman to get into medical school. Just as an aside about that, one of my heroes at Hopkins was Helen Taussig, who was the great pediatric cardiologist, who essentially was the person that worked with Dr. Blalock to develop [the blue baby] operation. Helen's father had been a professor at Harvard and taught at the medical school. She had been an undergraduate at Radcliffe and wanted to go to medical school. There was no way at Harvard. Finally, they agreed that she could take the courses as long as she never claimed any [academic] credit for herself. So, she went to Hopkins and became one of their great stars, very appropriate. The medical school at Hopkins was made possible by Mrs. Garrett who had raised the money that they needed. She and her husband had founded the B & O [Baltimore and Ohio] Railroad, but she had a real interesting stipulation which was that women had to be admitted under equal conditions. So, they always had women in the classes, but very few. There weren't very many women when I started there. I would say by ten years later, they were at least a third of the classes or more, so it was a real transition period.

S: You received your M.D. in 1966 and then you did an internship.

B: Right there in pediatrics. I said the best year was the first year I went to Hopkins, but that was the [second] best year and I was there becoming a doctor. I took care of patients; they were yours in those days. The faculty in the department were just extraordinary people. I enjoyed it enormously.

S: Who else was in the department then?

B: Well, the chairman was a man named Bob Cook, who was a very well-known figure then. There was a man named Barton Chiles, who was a member of the National Academy [of Science]. He was a geneticist. Bill Zinkham was the hematologist; and they all became good friends. One of my best friends was a fellow named David Carver, who became a chairman of several distinguished departments and he's a virologist as well. We're still good friends.

S: What were you paid?

B: Well, that's a euphemistic phrase. I can't remember how much we were paid, but I guess we got paid $3,000 for the year. There was no maintenance. They

only provided uniforms, that was it. I guess they washed them for us. I'm not sure of that. So, it was fortunate that my wife Laura was gainfully employed as a teacher at that time.

S: So, you were married in 1964.

B: Right, just after I finished graduate school. We had met at Hopkins; she was a graduate student there in a special program called a master of arts in teaching. We met at a party and about a year and a half later we were married.

S: What did she teach?

B: She was an English teacher in a high school. She did that for a couple of years and then when we went to Washington, she had some unusual jobs while we had our first child shortly after we got there. Then, she got a job working at the Labor Department, and she was doing high school equivalency teaching for women who were underprivileged to qualify for high school equivalency and therefore be able to be hired by the federal government at that time. She did that while we were in Washington. Then when we came back to Baltimore, we had our second child and so it was a question of what she was going to do. She did some substitute teaching, but I don't think we were ready for her to go to work full-time as a high school English teacher with 185 students a day. I said, why don't you just go teach at one of the colleges and teach expository writing? One of her friends had done that in the same program. She said, well, I don't know how to get that kind of job. I said, well, you just contact them and ask them whether they don't want somebody, which she did, and she got a nice job at Towson State, a college at that time [now a university] in Baltimore. She did that until we moved to Florida.

S: So your first child Jonathan was born in 1968, and then Deborah in 1970. Essentially when you went back to Hopkins in 1970, you had two fairly young children.

B: Well, Debbie was born right after we got there.

S: You did a year's internship and then did you do a complete residency?

B: No, they offered [to let me], it was interesting, I got two calls not too far distant [from each other]. One was to go back on the faculty in microbiology and the other was to go back as [a resident] and they were going to let me skip my second year and go directly to third year of house-staff training. I figured it was better going back on the faculty, so I passed by the opportunity and then, after I went back, I was a Howard Hughes investigator. Hughes said it would be nice if I had a clinical appointment, for [Hughes'] tax issues, as far as I could tell, from

their point of view. I talked with Bob Cook about [my] possibly [having a second appointment in Pediatrics. Bob said, that's terrific. He saw pediatrics as just an outpost in the basic sciences.

[End of side A1]

S: You're talking about your being appointed to a clinical position. Let me just back up and ask you, you went to Washington and worked for the NIH?

B: Right, I did three years there. That was interesting too, because my medical school career was sufficiently unusual and abbreviated. [As a consequence,] I really couldn't qualify for any of the special [public health service] programs at that time during Vietnam, which [would have] prevented me from going directly into the regular Army situation. Then I heard that there was a laboratory at the NIH which had a [special] slot which carried with it exemption from normal military service. I simply called up my mentor and said, would you talk to this individual [in charge of the lab] and see whether it is possible. He called me back and said, you're okay, call them up. So I called them up and a fellow named Arthur Weisbach, whom I just saw recently, interestingly enough, at the opera. Arthur said, "I talked to Charlie and the job is yours if you want it." I said, "maybe it would be good if we just met briefly so we can figure this out." So, I went down to see Arthur the next day, since we were only thirty-five miles away. We had a very good interaction and I fit the position, but I still had a problem because the draft board wanted me to report for a physical. It was very interesting.

What I needed was to get an immediate commission in the Public Health Service. So, I called up the personnel office at the Public Health Service and I told them my situation. I said, I need to have an immediate commission. They said, okay, and the next thing I knew I was commissioned as a junior assistant health services officer, an officer and a gentlemen. I no longer had to report for my draft-board physical. Then I went down to the NIH in July of 1967 [on active duty]. That was kind of interesting because officially, after serving three years, I was a Vietnam veteran with all the [privileges] and all the rights that were associated with that. On the other hand, when I received my orders to report for active duty, they read, at your station, uniforms are neither required nor desired. I never felt, I have to tell you, in my three years of government service, that I was in a particularly military situation, except for when we would go across the street to Bethesda Naval Hospital to use those facilities. They would ask us what our serial number was. [I]'d say, just a minute, and get out [my] ID card; they'd see what it was and say, "you mean you don't know what was your serial number is?" [laughing.] We were a little suspect in their eyes.

S: You were in inactive reserve for a year or so and then you went into active duty?

B: Well, that's what happened. I was in the inactive reserve while I finished medical school and my internship and then I went on active duty. Then, I was on inactive reserve from about 1970 to 1998, at which time, I said, I think we should come to a parting of the ways. They said, thank you, we'll be happy to do that for you [laughing].

S: Did you end up publishing anything from your work at NIH?

B: Oh, yes, I think we published three or four papers while I was at the NIH. That's when I first started working with animal viruses. My [original] lab [head] there, Arthur Weissbach, actually left the NIH after the first year I was there. So, I moved down the hall to another [unit], the [Vaccinia] Laboratory of Biology of Viruses. Between those two, we published several papers on the virus, which still makes me a smallpox expert. I then began also working with [adeno-associated virus].

S: I guess you published a few things while you were in medical school, but that was related to your...

B: Right, I was going to graduate school. There was no time for any research when I was actually in medical school.

S: What started you on the adeno virus?

B: The adeno-associated virus. I went into a laboratory [at the NIH], which was like a department, and there were two factions in the department who didn't really interact very much. One night there was a party and, in the course of the party, I told this fellow, Jim Rose, who was in the other group [in the] department, a joke which amused him enormously. He was the butt of the joke, actually as it turned out, but he really appreciated it. The next morning he approached me with a problem that he had. He had published a paper characterizing the DNA of adeno-associated viruses as a linear duplex [double stranded] molecule of a certain size, and a fellow named Lionel Crawford, who was in London, had just published a paper where he had indirect evidence that the size of the DNA in the virus was only half of what [Jim] had reported. [Crawford] also knew that related viruses [had] single-stranded DNA genomes, as opposed to double-stranded DNA genomes, so Crawford said, "this is a great mystery. One of the possibilities is that there are two kinds of addino associated virus particules. One contains a plus-strand and one contains a minus-strand and, when you purify the DNA, they come together to form a double-helix." So, Jim was very upset by that. He said, "how can we prove or disprove Crawford?" I said, "well, let me think about it for a day." So, the next day I said, well, this is how we can approach this problem, and we did the experiment, and it proved that, without

any question, Crawford had been absolutely right. So, we published the paper in PNAS [Proceedings of the National Academy of Science] at that time, about that particular issue. Anyway, that got me started on adeno-associated virus. It had only been shown to be a real virus probably four years [earlier]. So, it was actually really new. There have been moments when [I was accused of] being the father of the field. The problem is, of course, that Jim Rose and Dave Hoggan, who was also at the NIH, had already started on the virus and I learned from them, [so] I try to correct that misnomer.

S: You then moved back to Hopkins.

B: In 1970, that's right.

S: Just as a quick question, when you were in D.C., did you actually pick up and move there?

B: [Yes], we lived right across the street from the NIH, so I walked to work every day. It's almost as good as here.

S: That's ideal, especially for D.C.

B: Yes, it was very nice. We went from living at Hopkins in an efficiency in what we called the compound. The rumor was that it had actually been built as public housing. but it had not met the standards. so they turned it over to the medical school to use for house-staff [and] students. Anyway, we had a first-floor unit and we communicated directly to the people upstairs through a large hole in our [bath]room ceiling. We left from those conditions to a three-bedroom house in Bethesda. The rent had been raised. I think we paid $160 a month for this house that was really good-looking. We really felt like we had reached a new level of living standard.

S: Did you end up with any medical school debts?

B: No, my parents had covered the tuition component while I covered the living expenses.

S: That's sort of an ideal situation.

B: That was very nice. I think it was a little different in those days. There were rumors at Hopkins that some people never paid their tuition and they [the school] didn't [go after] them, figuring that, later on, they would do well with them.

S: [They thought] that they would be loyal.

B: Exactly. [Now], it seems like, even relatively speaking, it seems like tuition is higher. It is, but you know as I figure, when I went to Harvard, I think the tuition was $1,000 a year. I think of what my father was earning and the proportions when our son when off to Cornell, the proportions between what I was earning and his tuition was about the same [as] between my father's [income] and my tuition. So, I'm not sure that it's gone up out of proportion. I remember buying a car in 1970, a brand-new car, it cost me $2,000. It lasted [ten] years.

S: That's amazing. All right, so 1970 you're back at Hopkins and a professor of microbiology?

B: Well, assistant professor. I started working in the laboratory and Dan Nathans was the head of the virology division in microbiology. He had sort of been the person that handled the [recruiting], although Dr. Wood was still the chairman at that time. When I arrived in 1970, essentially Nathans had two laboratories and he gave me one and he kept the other one. We had a visitor from the National Science Foundation one day and I showed him my lab, which he thought was appropriate for an assistant professor who had just started. Then I showed him Dan's lab and he looked around with a kind of strange look on his face. I said, "what's the matter?" He said, "where's the rest of it?" I said, "this is it." He said, "I don't believe you." I told that story on a million occasions and sometimes Nathans would refer to it when he would give a talk later on wondering whether he made the right decision. However, in that small space that he had remaining, he did the experiment that won him the Nobel Prize in 1978. So, whether I did him a favor or he did himself a favor, we'll never know. It was an interesting place to be, because Nathans was on one side and the laboratory on the other side was that of Hamilton Smith, and the two of them won the Nobel Prize together in 1978. It was the first prize given for work with recombinant DNA, so it was good company.

S: So you were there for six years.

B: Right, I was there for six years, and after three years, I was asked if I wanted to run the Year I program that I had first come there for as a student. I thought that was kind of nice, sort of full-circle. I still was [an] assistant professor for one more year. I was the only person at the medical school who was in charge of [an] educational program [who] wasn't a department chair, so [it] was kind of an unusual position to be in. It was fun, although it was a lot of work. We would have about thirty students a year and I was their advisor. I would have thirty [under]graduate advisees, [quite] a burden.

S: It is.

B: It was very stimulating. I also served as the representative of the medical school

[to] the university on the main campus. Again, I found myself at moments in an unusual [situation]. We had to deal a lot with the Homewood [main campus] people because students [in the Year I Program] would take a lot of their courses at Homewood. Fortunately, the dean was a fellow named Surskind and Sig had been my official Ph.D. mentor because he was in the biology department, Charles Thomas [my actual mentor] was in [bio-]physics, [but] I was a biology student. It made it much easier, since he [Sig] was a good friend, to deal with him. Again, it was the same old [issues], can we do this? Oh, I don't see any reason why we can't do that. So, there was very little formality and you got done what you needed to get done. Of course, a lot of these students were unusual, so you always had to have various exceptions made, but there were never any serious problems. It was a very easy time, and I hate to say it, but it made me decide that I actually liked administration on a certain level.

S: You had the clinical appointment as well.

B: No, I never saw patients while I was there. I had maintained my license, but I was full- time in the laboratory and teaching. I used [to] go to rounds every Saturday and I'd see all my old friends from pediatrics and then we'd have coffee afterwards. It was really kind of a nice existence. I have a lot of very good friends from that program.

S: It sounds ideal in many ways.

B: It was very nice.

S: You were working on the adeno-associated viruses in your lab?

B: Right.

S: What was the research that it focused on?

B: Well, it was really a continuation of characterizing the structure of the DNA [at that time]. We began the first sequencing efforts. We were interested in the ends of DNA, which turned out to be [an] incredibly complex pursuit at that time, in terms of their organization and their composition. So, it would take us quite a while to sort of piece that together.

S: Complex in what way?

B: Well, they had all sorts of inverted little [sequence segments] and they could fold up on themselves and form strange shapes. Also, it turned out that overall, there was a structure of 125 [bases] which was a palindrome, but not a perfect palindrome. When it [was folded] up [to maximize potential base pairing], it

looked like a T, and the two cross-arms of the T were a little bit different. So it turned out that, during replication, this whole sequence of 125 [bases] would get inverted. This was a completely novel situation. We had to figure all this out starting from scratch. It took us a while to sort of roll out. I mean, even today, doing straight-forward sequencing would confuse you considerably, if you didn't know what you had to look for.

S: It looks like you're also still working with vaccinia.

B: We did continue work with vaccinia, and we made a rather startlingly discovery. A fellow named Yechiel Becker in Israel had figured out how to get the DNA of vaccinia [virus] out in one piece. [It] was the same size as the T-4 phage DNA [I had worked on first as a student]. And I was interested in particular [because] they used the same protease that we had used for the [isolation of hemophilus DNA]. So, I thought there was [a] kind of connection there. What I realized was that there was a very simple experiment that we could do. We could see if we could reproduce his results, and then we could simply denature the DNA [separate the complementary strands] and see whether the molecular weight [fell] by a factor of two or more. Thereby, asking the same the same question we had asked of T-4 DNA: namely, are the complementary strands intact? So, we did that experiment, and by this time Al Hershey, [our old] collaborator, [had] started the use of sucrose gradients, which made measuring molecular weights of DNA much easier. So, we did the experiment. As always, when you do an experiment with two possible answers, you get a third answer. And that was, when we denatured the DNA, we discovered that [the molecular] weight didn't fall, [which] was kind of strange. So, we decided the DNA probably was cross-linked, and the question was, where is it cross-linked? So, we did a bunch of experiments, but fundamentally, it was cross-linked at the ends. What actually happens is that one sequence just [folds] back on itself and becomes the other strand; so we were able to show that the DNA had these hairpin cross-links at that ends. That was [a] rather interesting discovery.

S: Certainly.

B: It turns out all the poxviruses have that structure.

S: It's actually characteristic.

B: Yes, so that was about it for vaccinia.

S: I was just going to ask you, what was the size of your lab group?

B: I don't know. It was not huge, we had probably about six to eight people in the lab. I've never had a large lab. I might have gotten as high as ten or eleven,

but nothing compared to many large labs.

S: It was small enough that you could really know what was going on with people.

B: Yes, I used to look at all the data, which turned out to be very convenient at times.

S: Keep things from going awry?

B: That's right, that's right.

S: When you, in 1976, went to Florida...

B: Yeah, it was interesting. My position with Howard Hughes came to an end. I think primarily because my mentor, Dr. Wood, had died and it was a personalized situation in those days with the Hughes Institute. So, I continued with them for a year or two after that, but it was clear that I had to find another source of support. It turned out to be a time when there were no Research Career Development Awards from the NIH. They decided those weren't a good idea, and so the only place I could really apply was to the American Cancer Society. I was [site visited] by a man named Harris Bush, who was the chairman of [Pharmacology] at Baylor, a notable character. We had a very nice interaction. He said to me at one point, "what are you doing here?" I said, "what do you mean?" He said, "well, you're going to be a M.D./Ph.D., why aren't you a [chairman]?" I gave him a strange look, I think I had just become an associate professor. He said, well, you probably haven't been asked, but you will soon. He said, "when I was thirty-seven, they offered me the job at [Baylor] and I was an associate professor at Yale, so I went." I said, "gee, that's really nice, but it would be better if I got this award from the Cancer Society." He said, "well, I'm going to try." He said, "there's only four or five of them, but we'll try." I got the award from the Cancer Society. But when I was thirty-seven, I was offered the job at Florida. I thought that I was in a situation where, either I should stop telling my chairman what to do, that was Dan Nathans by that time, which he frequently actually would do, or I should accept responsibility and go do it myself. I knew I wasn't going to quit offering advice, so I decided I ought to try it myself. So I went down to Florida.

S: Had they recruited you or were you applying at that point?

B: Well, somebody had nominated me for the job. There was a search process. I probably wasn't their first choice, but eventually I was the first one who was foolish enough to accept the opportunity.

S: This was 1976, and you went down as professor and chair of...

B: Microbiology, it was called immunology [and medical microbiology]. It was a very interesting position, because I went as the chair and I was the youngest person in the department. Nobody ever held that against me. They were very good people. They were tremendously under-paid.

S: What was the salary?

B: I had one full professor who was making $20,000 a year. So, one of my conditions for going there was that the salaries would be doubled in the first two years.

S: Everybody's?

B: Everybody. I mean, they were all good people, there as no real [dead wood] at that time. I told them that and they laughed, but they accepted me. They said, you'll learn. Then, when their salaries all got doubled, I could do no wrong from their point of view. I'm sure I did, but they were very supportive. They were supportive before that. It was a very tight-knit group and we immediately went out and recruited three or four new faculty, most of whom are still there in leadership positions.

S: Such as?

B: Well, I recruited Bert Flanegan from David Baltimore's laboratory. Bert is [now] the chair of our [bio-]chemistry. Nicholas Muzyczka had worked with Dan Nathans, and Nick was the head of the Center for Gene Therapy and is actually the interim [director] of the Genetics Institute at the moment. Bill Hauswirth, who is an outstanding eminent scholar is there. He is very close. Actually, when I went down there, Bill wanted to come down to Florida. He had already been briefly a faculty member at the School of Public Health of Hopkins, but then he came to my lab as a research associate, because he really wanted to get into molecular biology. So, I said, look, Bill, you can come down, but I can't put you on the regular faculty, because I don't think that it's appropriate for the chair to bring people with him as faculty members. So, Bill came down with me and he'd visit on several occasions. The old faculty said, you know, Hauswirth is at least as impressive as the guys we're interviewing, why doesn't he get a regular faculty position? So, I said, okay. So, we did, and I told him that he was going to work with me for a while part time and develop his own area of research. I said, Bill, you have to develop your own area, otherwise, when it comes time for promotion, you'll have to face the old argument of, well, he's really not an [independent] investigator, etc. So he did. He became a real expert on mitochondrial DNA. He was excellent with mitochondrial DNA, and then he got involved in [the eye]. To keep him there, when I [went] to Cornell, [he] was [offered] an eminent scholar position, which I thought was terrific. So, he

became a real expert on [the] molecular-biology [of the eye]. Then he realized that the [virus] that he had worked with many years before could be used for [gene] therapy in [the eye]. So, he [developed adeno-associated virus as a vector to use in the eye] and we're still collaborators.

S: How many people were in the department when you went there?

B: That's a good question. I think there were five or six in our department. Eventually, we got up to as many as twelve when I was there.

S: It really doubled.

B: Yeah, I think that's where it was. One person left to become a chair at the University of Mississippi, Bill Clem. Bill and I are pretty good friends and he said, "you proved it could be done, so I thought I might as well take a shot at it myself." So, he went to Mississippi and is still the chairman there.

S: What was Gainesville like in 1976?

B: It was somewhat smaller than it is today. There were probably about 75,000 people in and around Gainesville. I'm not sure how different it really was. The major difference was that it didn't have the Center for Performing Arts and it didn't have the Butler Plaza, and oh, the Oaks Mall first started while we were there. There's been a lot of development west, but I'm not sure how much it's truly changed. When we went back, it didn't feel so different to me.

S: What was the feel of the place, in the community, but also at the medical school?

B: They were somewhat inhibited by the constraints put on by the [bureaucracy] of state institutions. I think I was supposed to [be the voice] of the faculty, that I mentioned to you, in our department. It was an interesting place, because, already, it was a place that, academically, was much better than people really appreciated. I think some studies had been done which said, if you looked at publications, etc., that compared to sort of public perception, the medical school was notably underappreciated. So, it was really a pleasurable place. There were a lot of people who had come from places like Hopkins and Minnesota who were very good people. I think it was a new place, so it was unconstrained. We had Herb Kaufman there, who is a loose cannon, but a real power in ophthalmology. He had been the very first person to ever successfully use an antiviral [drug] clinically. There were other people of that ilk, so I thought it was quite an interesting place. I think that the first dean, Dr. Harrell, had been very academically oriented. His successor had been the chair of microbiology, Dr. Suter, and was also academically oriented. It was always a place where they felt the academic component was a serious aspect, as well as being clinically

excellent. That was all there, but it probably was not a place that was on the absolute cutting-edge. When I went through the search process, I had an interview with a fellow named John Adams, who was the head of psychiatry; he was very bright and had come from Stanford. He said, "what do you think the difference should be between Stanford and the University of Florida?" I said, "well, I'm not sure there really should be any real difference. Obviously Stanford has a good start, but there's no reason why the University of Florida shouldn't aspire to the same academic excellence." It was a very congenial conversation. At the end, he said, "gee, I hope you come." I said, "well, that's up to you." He said, "no it's not, I'm not on the search committee." I said, "now I'm nervous." Anyway, we were friends after I did come. I think there was that aspect, and shortly after I arrived, Al Stetson, who was a very serious medical statesman and who was the dean VP, took me up to meet Bob Marston, the president. Bob had just come after a year's leave from being the director of the NIH. Bob said, "your mandate here is to bring modern biology to the University of Florida." I said, "yes sir, thanks a lot." That's a pretty steep [responsibility] to have.

S: Single-handedly

B: Right, but that's when we went out and recruited. Our new faculty had that in mind. In fact, in a sense, I think that did happen. Before I left, we had started working with IFAS [Institute for Agricultural Studies] trying to get them going. They were really impressed with some of our people, Hauswirth, in particular, they [formed a] very close linkage at that time; Nick Muzyczka was very much involved as well. So, that went very well. I've always had sort of a good relationship with the people at IFAS over the years. At that time, Bob Marston, because of the kind of [financial] situation, no better than it is now, in the state of Florida, had moved the biology units essentially out of [The College of Liberal] Arts and Sciences. So, Biochemistry, which had been a joint department, became totally a medical school department, and Microbiology, which had been a joint department between IFAS and Arts and Sciences department, became a totally IFAS department. There was very little biology left in the Arts and Sciences department. More recently it's been better.

S: You mean in the sense that there's more biology?

B: Right, I think they're starting to do some fairly serious things in biology because of some of the people there.

S: It's certainly was twenty years old at the medical school when you arrived. You're coming from Hopkins, which had certainly been on the cutting-edge of medical education when it was started, what was that transition like?

B: I tried not to say ever "this is how we used to do it at Hopkins."

[loud buzzing for several minutes of the interview]

B: The consequence was that the students, when we recruited, frequently didn't look outstanding on paper, although they met reasonable criteria. Many of them went out of here and did extremely well. We were able to get them really very fine, post-doctoral positions. We would have a lot of outsiders come to visit. The consequence of that was that the program became fairly well-known. There was a study done, I guess by the NSF, probably in my sixth or seventh year, the department was actually ranked [together] with the [University of] Alabama [at] Birmingham department, which was twice as big, for having the most improved graduate program over [the] previous [five] years. I thought that was a nice recognition of what we were doing, although I thought it was [also] a natural consequence of what it had been when I got there.

S: The people in the department, were any of them doing clinical work?

B: Well, when I first went there Joe Shands was in the department, and Joe also had an appointment in infectious diseases. He was very clinically oriented. Herman Baer, who was an M.D., was working with gram-negative bacteria, doing a lot of things that were clinically related. Later on, both Parker [Small] and I actually did some clinical things in pediatrics. That was through the [unusual attitude] of Dr. [Schiebler, Chair of Pediatrics], who somehow felt that we were competent to do some of these things. I raised that issue with Dr. Schiebler and he said, that's why we have a house-staff, to protect the patients from some of our faculty [laughing].

S: This was in that period between 1976 and 1984 when you were working with the patients?

B: Yes.

S: What were you doing?

B: I would hate for this to be too public. We may have to ban this from publication for thirty years to protect Dr. Schiebler at least, but one month a year, I was an infectious diseases consult in pediatrics. Prior to this, I was teaching medical students, which was very good fun. We would make rounds and see all the patients we had in infection diseases. Students would tell me what they thought about them, which was interesting.

S: About the patients and their diseases?

B: Right, and the students were good.

S: What sort of contact did you have with the medical students in the medical school?

B: Well, I ran the virology course, that was the primary [form of contact with the students]. We started a novel approach in terms of testing. Every student received an individual oral exam for the final in virology, which the faculty were very concerned about me doing. Then we did it and then they [the faculty] didn't want to stop it. In fact, after I left, they continued doing it for a while.

S: What was the rationale?

B: Well, I thought that the rationale was that we would actually be able to have the students try to figure some things out and we could really test them much more effectively on what they really knew. It turned out that, after five or ten minutes, you could really tell if a student knew anything. He's not hiding behind adroit guessing of multiple-choice answers.

S: I guess that's certainly a test of their skills...

[End side A2]

S: It tests their ability to, not just memorize, but [to do] more creative thinking, the ability to put...

B: Well, to put two-and-two together. Their ability to memorize the stuff is of no value whatsoever if they can't actually apply it a little bit to clinical situations. They were actually quite able to do that. We got a variety of that approach at Cornell before I left as well.

S: Was Manny Suter dean when you were there?

B: No, Manny had left [and was] at the AAMC, by the time I got there.

S: Who was the dean?

B: The [first] dean was Al Stetson.

S: I've heard Manny described as a dean who was focused first on education, on curriculum.

B: [That was true] Al, [however], was interested in science and other things as well. He had succeeded Lou Thomas as the head of experimental path[ology] at NYU [New York University] before he came to Gainesville. He lasted only about two years, I think, maybe three years, before he died [prematurely]. It turned out I was his last chair appointment. So, then we had Will Deal, who had been

deputy [dean]. So, Will was the dean [most of] the time when I was there, although there was a brief period of time when he left us to go do something at the AMA, but then he came back very rapidly after a year.

S: Like your grant support, what was the source of your funding?

B: For the first couple of years, I was [supported by] the National Science Foundation, and then I was really primarily on NIH funding.

S: What were the size of your grants?

B: Oh, they weren't very big, $150,000 a year in direct costs or something like that. In those days, faculty salaries were not on grants. It's kind of interesting. That's one of the big changes. They [had] just [gone] to 15 percent before I left. An agreement had been reached that the faculty would try to get 15 percent.

S: From grants?

B: Right. Now, I think they're looking for 65 [percent].

S: That is quite a change.

B: It's a significant change, and the consequence has been that the faculty are pulling in a lot more money to the college.

S: Do you think it affects faculty research? Does it force the faculty to be more active with research?

B: Well, to a certain extent, it certainly does. I think the bigger factor in achieving that was that, when I went back as the dean in 1997, I basically said that the lab space you have is dependent upon the number of research dollars you're bringing in. If you don't have any research dollars, then you're not going to have any space left in a very short period of time. It was really quite spectacular. When people who hadn't been funded for years [heard we were] going to have lots of space to be able to use for people who were funded, suddenly they got their own money. There's nothing like a little stimulus, and people responded very well to it. So, I think that was one thing different.

S: You had said earlier that you found that you liked doing administration when you were at Hopkins, what was your approach to administration? Would you say that you had a philosophy towards administrating?

B: You mean when I was a chair the first time? Well, I think that we had a small department, so there was a lot of communication all the time. We were on the

same floor and just saw each other constantly. We used to have lots of faculty meetings. People would always say that they [all could vote, but the chair always had one more vote]. I think maybe, with the exception of a single graduate student who was up for admission, we never had anything but a unanimous vote. I think we essentially had a pretty good consensus on just about everything. I think that was my major approach, and then I had an excellent administrator, [Muriel Reddish]. I let her administer; she kept me out of all sorts of trouble.

S: What percentage of your time was doing administration and what percentage was research, would you say?

B: You know, it's really hard to say, but I would guess that I always had my time where I could think about doing research.

S: Let me just ask about your research and publication in that period.

B: It was all working with adeno-associated virus.

S: What was your focus at that point?

B: Well, it was the same issue. It was the structure of the DNA we finally, towards the end of period, published the sequence of the entire genome. It was a major effort. We had several publications on it, and we mapped all the transcriptional units and then we made a discovery, which had a fair amount of significance, which was, we discovered first, that the virus integrated into the human genome, and then we started characterizing that.

S: To what extent was being integrated?

B: Well, that's very hard to say. I'm not sure we could answer that question today. What we know is that [the virus] gets integrated into a large percentage of cells that [have been infected], but the way that we really measure that meant that we had to infect [using] a fairly high multiplicity of [infection]. So, there were lots of viral particles that we are attacking the cell, if you will. A significant number probably got in, so it's really hard to say what percentage of the virus that [in] effect the cell actually integrate [into the celluar gnome]. What we knew was that you [could] get [nearly] half the cells that were infected to be able to carry a virus in a latent state. It turned out that, as far as we could tell [little or none of the viral DNA in the latently infected cells was extra-chromosomal]. Then, we sort of characterized the integrated DNA.

S: In that period, you also spent a year in Israel?

B: Yes, from 1982 to1983. That was one of my [most] subversive achievements. I went to Israel and I was in a laboratory of a man named Ernest Winocour, who is a very distinguished, somewhat older scientist. We [had] met on several occasions. I went to his laboratory because he worked with another small DNA virus called SV40, which was a tumor virus. Ernest had developed a system to study SV40 recombination in cell culture. He was looking at recombination between SV40 and [the DNA of] a bacterial virus called OX 174. AAV, [adeno-associated virius], was not all that different from Viax174, so I went to do a project which was to study the recombination between SV40 and another animal DNA virus, as opposed to a bacterial virus. Really, what I wanted to study [was] the ability of AAV to integrate into a defined DNA sequence in a cell. SV40 was a good target from that point of view, [because it had already been sequenced]. So, I went [to do] this project. There was a student, [Zehava Grossman] in the laboratory, a woman about my age who, after I was there for a couple weeks, told me that she wanted to do her thesis project essentially doing what I was doing, in other words, work with me. So, we worked together very closely and actually got material for a couple of papers. It was interesting, but then Ernest [Winocour] had to start taking AAV more seriously, since he had to approve [her] thesis. The upshot of that was that he switched his research from SV40, where he was a [major international] figure, to AAV. He [had] played a rather major role in [the study of] DNA [animal viruses]. Dan Nathans had gone to Ernest's laboratory in 1970 and learned how to work with SV40; [later in 1978 Dan won the] Nobel Prize [for his work with SV40]. Maxine Singer, who is a very famous scientist and just stepped down as the president at Carnegie Institute of Washington, had worked in Ernest's laboratory to learn how to work with [animal virus] systems. These people [and others] were interested in how to work [with SV40 and learned from Ernest;] then I converted them to work on a different system. Anyway, we became close friends and had a joint grant for about fifteen years after that.

S: Did you continue to go back and forth?

B: I didn't work again in Israel. I've been back in Israel on many occasions since that time, at least a half a dozen times or more, but I haven't been there since 1997. Ernest actually came over and spent time in my lab, both when I was at Cornell, where he spent quite a few months, and then he came to Gainesville and spent a couple months.

S: You were at Gainesville, for the first time, for eight years.

B: Right, that's what Al [Stetson] told me to do when I went there. [He] said, "this is a job you want to do for six to ten years." He said, "if you haven't done it by ten years, you're not going to do it, and if you have, you should make way for some fresh blood." He said, "it takes at least six years to get stuff done." So, I stayed

eight years.

S: What did you set out to accomplish?

B: The question was, could you create a department [with an] academic environment which allowed faculty, as well as students, to develop? It seemed to me that we'd been able to do that. The first people we'd brought in, by that time, had all gotten tenure, without exception. In fact, I think there's only one person we ever hired, [who] failed to get tenure.

S: Well, I guess you were modernizing the medical school as well.

B: Well, one could say that, although I do with some trepidation. There's a lot of people out there who are contributing to that effort.

S: It wasn't single-handed, in other words.

B: No.

S: So, after 1984, when you reached the eight-year period, were you actively looking?

B: Actually, no, I had been approached. When I was in Israel, I had been asked to look at the job at Alabama, which I did briefly, but I decided I didn't want to do that. As soon as I got back in 1983, I was approached by Cornell. Then I was approached by Michigan. I was already pretty far-down-the-road with Cornell, but I did look at Michigan when they sort of offered me lots of opportunities, but I felt that since I had started with Cornell [I should go there]. I had thought it would be nice to spend part of my life in a world-class city. There aren't too many, and I don't know how many besides New York there are in the U.S. There are a lot of nice places, but...

S: But world-class...

B: So, I went to Cornell in 1984.

S: You went in as chairman of?

B: Microbiology.

S: You still have a general appointment in pediatrics.

B: Yes, they were nice. I mean, I have an appointment here too. I didn't do anything in the department really, except talk. I had nice conversations with the chair and some other people that I liked in the department.

S: Since you brought up the idea of world-class city, when you were in Gainesville, were you living in town or out of town?

B: We lived out of town, not way out of town, but we were the western edge of civilization [of the metropolitan area]. [We] were just about a mile closer in than we would up living the last time.

S: A friend who lives in Manhattan came to visit us and she was just almost horrified at the teeming life of Gainesville, animal life, not human life.

B: There was a moment when I started thinking about snakes, before I got there, and then I realized that your likelihood of getting hit by lightning was greater, so at that point I quit worrying about it. Even the fact that I discovered that Marion County is the place in the U.S. where there are more lightning strikes than any other place in the country didn't change my attitude. We only had one or two snakes.

S: What was New York...I guess salary, you said salary in Florida was low, even though it doubled when you were there.

B: Well, for most. Let me just say one other thing about Gainesville. I think the outstanding character, in many ways, was Gerry Schiebler. What was nice about Schiebler, from my point of view, was that he went out of his way to make things possible for the basic science chairs to do, which otherwise they wouldn't have had the flexibility to do. So, he used to be more than willing to trade his AEF [clinical] funds for some state money; and the AEF funds were much more flexible, we could pay seminar speakers, we could take them out to dinner, that sort of thing. Also, we set up a situation where some of my faculty were actually housed in pediatric space, and we had a couple of his faculty who were housed in our space, so that they would have close interaction with basic scientists. I think he was very much ahead of his time.

S: He really had a vision.

B: He really did. It was kind of interesting. So, we had a very nice relationship there. He was sort of one of the drawing-cards when I went back. Of course, by then we worked very closely together the second time. I'm going to get in trouble here, because I only have about ten more minutes before I have to bail out.

S: Well, I also need to run.

B: Yes, you need to go across the street to see Louis.

S: Let's say about another five minutes. Let's just get you situated in New York for the first time. Was there a significant salary increase and everything because you're living in New York?

B: There was a significant salary increase, but not anywhere near the increase in the cost of living associated with moving to New York. I had a pretty good salary at Florida. Probably in terms of being paid by state funds, I could have been among the top ten in the state, I don't know. It was not a huge sum, but by the standards of the time it was significant.

S: Did you end up living in Manhattan?

B: No, we moved to Westchester because I couldn't afford to send my kids to private school. It was hard enough to afford to live in Westchester, but anyway, they finished school and went off to college. We were here thirteen years, and the last four and a half years we lived in the city; [it] turned out that, without having kids [it] was cheaper to live in the city than Westchester and a lot more fun. The last four and a half years, I felt like I was on perpetual vacation because we really [could] take advantage of all the cultural activities. We lived on Sutton Place South and we could walk to Carnegie Hall and we could walk to Lincoln Center and Broadway. It was a very good location and we tried to take full advantage of it.

S: So, New York really was an exciting place to be.

B: Absolutely, very exciting.

S: What about Cornell, at the college?

B: It was a very interesting place, it was very clinically oriented. We were sitting in a complex with Memorial Sloan-Kettering and Rockefeller University, and it was a very exciting place to be in that sense. I had lots of friends at both places, people I knew before, friends I had made while I was there, so I had a good time in that sense. I think the issue from my point of view was that, after I had been there ten years, the truth of Al Stetson's dictum became apparent in the last three years. I think that things sort of plateaued, although there were some things that I was working on and research was going [well]; that was not the issue. I thought I could probably use something different. I always felt very well taken care of, again, at Cornell. I had very good relations, I thought, with the president of the university, and certainly we interacted a lot with the deans.

S: So, they brought you in, did they have something they wanted you to do at the department after you came in as chair?

B: Probably they wanted me to introduce modern biology. [laughing.] There was a

large element of that as well.

S: Just the molecular biology...

B: Yeah, I mean, it just wasn't going on at that time. There was [to some] extent, I would say, in cell biology. Don Fishmen was the chair at that time and I think Don was pretty well into it, but the other basic science departments were much more traditional. We changed those over while I was there.

S: That's interesting that it made it into 1984 still being...

B: That was part of the issue. I think that's always been a challenge for Cornell. There was a great temptation on the part of the trustees and the board of overseers to feel that the basic efforts, the scientific efforts, could really be managed by Rockefeller and Sloan-Kettering, and they could concentrate on the clinical, but it doesn't work that way.

S: You really need to have energy in the institution itself.

B: Right, and it enables you to interact that much better with your neighbors.

S: Okay, let's stop there.

[End of Interview.]