

INTERVIEWEE: Dr. Wolfgang Joklik
INTERVIEWER: Jessica Roseberry
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PLACE: Dr. Joklik's home; Durham, NC

JOKLIK INTERVIEW NO. 1

ROSEBERRY: This is Jessica Roseberry. I'm here with Dr. Wolfgang Joklik. He is the James B. Duke Professor Emeritus of Molecular Genetics and Microbiology. He's a former Chair of the Department of Microbiology and Immunology. This is May 16, 2007, and we're here in his home in Durham, North Carolina. And I want to thank you, Dr. Joklik, for agreeing to be interviewed today. I appreciate that very much. And I wonder if we might be able to start with just a little about your own interests in science and how that began, if that's all right with you.

JOKLIK: Now, do I just speak? Need to do anything? Just speak.

ROSEBERRY: Just speak.

JOKLIK: Right.

ROSEBERRY: Mm-hmm.

JOKLIK: Okay. Well, I was born in Vienna, in Austria, and when I was eleven years old, my family moved to Australia, where my father took over the Australian branch of an Austrian company, The Styrian Steel Works, makers of fine steels, molybdenum steels, tungsten steels and so on. This was shortly before the war broke out; and of course we were very surprised by the war breaking out when we were in Australia! So I grew up in Australia. And for the duration of the war, from 1938 to 1945, I was classified as having the nationality of a country with which Australia was at war. Now, there were my parents and my brother, who was eighteen months younger than me. We went to one of Australia's top schools, Cranbrook School, in

Sydney, which was modeled on the great British Public Schools, like Eton and Harrow. Interestingly enough, we had no difficulties in that school whatsoever, in spite of being Austrian. My parents put us into Cranbrook School three days after we arrived in Australia, and at that time we spoke essentially no English. So for the first trimester—it was a trimester system—we were put in a class two years below our age, for the second trimester into a class one year below our age, and for the third trimester they put us together with the other kids our age, and by that time we spoke perfect English. So it didn't take any time at all, really, when one has to learn another language! Well, in any case, the Headmaster of that school was Sir Ivan G. Mackay, who was Australia's top soldier in the First World War. And during the First World War, my father was a graduate of the Annapolis of Austria-Hungary. He was a naval officer and served in the Austro-Hungarian Navy. And it often happens when individuals get together who were in opposing armed forces of countries at war, they like to talk and exchange experiences. So my father and Major General Mackay established a nice relationship.

Now, the Geneva Convention permits all male citizens of the country with whom you're at war who are over the age of eighteen to be interned. Not imprisoned, but interned. And, of course, the day that war broke out in 1938, on September 1, the Secret Service was at our home at 2:00 a.m. and my father was interned. He was put in a camp about—oh, about 100 miles west of Sydney. And his wife, my mother, and my brother and I, we visited him once. But about two months later, one day, he suddenly reappeared at home. Apparently what had happened was that Major General Mackay had assured the Secret Service that my father was a businessman and no problem, and obtained his release. Unfortunately eight months later, General Mackay went to the Middle East as the Australian general in charge of Tobruk. That was a major campaign in North Africa: Rommel against Montgomery. And the day that General Mackay left Australia,

the Secret Service was here again. (*laughs*) My father was again interned, this time for the next five years. And so my brother and I grew up without the presence of a father. We were able to visit him three times in those five years. He was in a camp about 300 miles south of Sydney—Tatura, it was called, a native Australian aboriginal name. There were hundreds of German and Austrian nationals there whose only problem was that they were interned! Other than that they had hobbies, had lectures, read, and did whatever they wanted, and so on. I finished Cranbrook School and then went on to Sydney University. Oh, by the way, I should add that Cranbrook, a very elite private school with high fees, which, of course, in the absence of a father and therefore an income, we couldn't pay, declared that we would be educated without fees! To thank Cranbrook School for that, my brother and I, some time ago, established two scholarships at Cranbrook, two full scholarships, one in memory of my mother and the other in memory of my father! As for our education, students in Sydney who scored highest in the State School Leaving Certificate examination, that is, the top scholars, received a free University education, and both I and my brother were in that group! So Sydney University didn't cost us anything, either!

I graduated from Sydney University with a Bachelor of Science (B.Sc.) degree in 1946 and then with a Master of Science (M.Sc.) in 1948. Then I went on to Oxford University in England where I obtained a D.Phil., (Doctor of Philosophy, Ph.D.) in 1953. Then I did a year's postdoctorate work in Copenhagen, Denmark, and returned to Australia as a Research Fellow in the Department of Microbiology of the Australian National University in Canberra. I was there from 1953 to 1962. In 1958, I had a sabbatical in the United States at the National Institutes of Health (NIH) in Washington, DC, in the Division chaired by Dr. Harry Eagle, a world-famous scientist who was the principal developer of the mammalian tissue culture system. Tissue culture, of course, is the technique of growing cells, including mammalian cells, in vitro. After a

year with him, I had to return to Australia under the terms of my fellowship, which was a United States Public Health Service Fellowship. I had to return to Australia for two years, and during those two years Dr. Eagle left NIH and became Chairman of the Department of Cell Biology at the Albert Einstein College of Medicine in New York. And in building up a Department, he wrote to me and asked me whether I would join him there. My wife and I, we discussed that; and since it seemed that one could build up a bigger operation in the United States than in Australia, we moved to New York in 1962. I was at the Albert Einstein College of Medicine from '62 to '68, and in 1968 I was recruited to become Chairman of the Department of Microbiology and Immunology at Duke, at Duke Medical Center. And I went, so since 1968 I've been at Duke. When I came to Duke the Department consisted of six faculty members; when I stepped down in 1993 there were thirty-four. In 1985, the National Research Council carried out a survey, as they do every five or ten years, to evaluate Departments in Medical Schools; and my Department of Microbiology and Immunology, together with two others, was declared to be one of the top three Medical School Microbiology and Immunology Departments in the country. So we had a very high reputation. When I came, Joe Beard was a well-known virologist in the United States. He started off in the '40s, then '50s, '60s, and it was he who really developed the concept of tumor viruses, the fact that viruses cause malignancies, which—I'll come to that in a minute—is extremely important. He started off working with papilloma virus, which causes cervical carcinoma, but did little with that. His main interest was RNA tumor viruses, or retroviruses, as they then came to be called. The two viruses that he worked with primarily were avian retroviruses, viruses that cause leukemia in chickens, which was extremely important because United States citizens eat a lot of chicken. It is possible to take cells growing in tissue culture, infect them with one of these tumor viruses, and then follow, using

chemical and molecular techniques, the various changes that take place as cells become malignant. That seemed so promising that in 1971, President [Richard] Nixon, in his State of the Union message, declared a War on Cancer. The National Cancer Institute set up a Special Virus Cancer Program that involved establishing Comprehensive Cancer Centers throughout the nation. And I thought—by the way, I should mention that I married, in Australia, in 1955, Judith, who was born in Perth, Western Australia. And we had two children, a son, Richard, and a daughter, Vivien, who were born in 1957 and 1959, respectively. And Judith, I'm afraid, developed cancer, breast cancer, in 1967, and so in 1968, when she came to Duke she already knew that she had breast cancer, and she was under the care of outstanding physicians at Duke. But she succumbed to the cancer in 1975, seven or eight years after its first detection, which was slightly longer than average, but, on the other hand, there was nothing one could do. She was on chemotherapy, of course, but she died in 1975. And because my wife had breast cancer, and I was a virologist and techniques had just been developed for really being able to observe the first stages of cancer formation, I thought that Duke should have—should be one of the first medical schools to have a Comprehensive Cancer Center. And I called a Faculty Meeting and talked to the other Chairmen, and there was considerable enthusiasm for applying for the designation of a Comprehensive Cancer Center at Duke. And Dr. [William] Anlyan and I went to see President [Terry] Sanford, and he was enthusiastic, and so we applied for being awarded and obtained a Comprehensive Cancer Center. We built two buildings for that: one for basic cancer research and the other one for clinical cancer research. One is the Jones Building and the other is the Morris Building. And then we appointed Dr. [William] Shingleton to be the first Director of the Cancer Center, and the Duke Comprehensive Cancer Center has always been in the very top flight of Cancer Centers since then, because we have had extremely good Directors of the Cancer

Center. Joe Beard, at Duke, was one of those who established that viruses can cause cancer. Peyton Rous was the one who discovered human cancer virus in 1961, for which he was awarded the Nobel Prize in the late '60s. Joe Beard was a national force in the tumor virus field. His wife—they came to Duke, I think, in 1937, and his wife also started working in the lab early, and I see that on most of their scientific publications, she was a co-author. So I knew Dorothy. She was a very nice lady. Joe Beard was a very forceful character, and at meetings spoke up without hesitation. (*laughs*) They were a very nice couple. They had a farm between Durham and Chapel Hill, and when they retired, they moved down to Florida. Joe had established a small company by then and took it down with him. There is a very useful enzyme that tumor viruses elaborate, the reverse transcriptase, and Joe Beard's company supplied that. I remember buying it, or obtaining it, from Joe down in Florida.

ROSEBERRY: What is reverse transcriptase?

JOKLIK: Reverse transcriptase transcribes RNA into DNA. Joe Beard established a close relationship with Lederle, which was actually owned by American Cyanamid, and the CEOs of American Cyanamid were active down here in the Triangle; at Duke and at the University of North Carolina. Joe Beard worked on equine encephalomyelitis virus which causes staggers in horses, even before he started on working with tumor viruses; and when he was still young, he established a vaccine against it. It turned out that inactivated equine encephalitis virus, inactivated either by heat or by formaldehyde intercerebrally, protected horses against infection. And so the head of this particular section of Lederle, or I forget now whether he was actually head of Lederle, but [William Brown] Bell encouraged Joe Beard to take out a patent on that, which was very—in those days, that was in the '50s—unusual, but they did it in any case, so that later on when inactivated virus was used to protect horses against encephalitis, that patent

yielded considerable amounts of royalties. And Joe and Dorothy Beard then set up the Dorothy Beard Fund into which these royalties were paid. They were administered by Duke officials and became available for supporting the scientific work in the Beard lab and for having scientists come to Joe's lab. And I think that in one particular case the fund even provided the money to purchase a house for one of the people coming to work in the Beard lab. Joe retired in the early '70s—he was born in 1901—but continued working a bit after that. In 1973, I think, Dani Bolognesi was the young scientist who came and continued his work.

ROSEBERRY: So they were in the Department of Surgery.

JOKLIK: That's right, yes. I don't know why they were there; they were MDs, and they obtained some financial support for their research from the Department. And, of course, basic research on cancer would interest a Department of Surgery.

ROSEBERRY: I know that he had great significance, and I wonder if she, as well, had an impact on Duke, or was her role kind of to support him, or—?

JOKLIK: They were a team. Dorothy carried out an important part of his research. She may have given some seminars and lectures and so on, but I don't think that was really her style. Joe did that. I don't think that she published any papers on which she was an author and Joe was not. In other words, they were always together. So it was also—always also, Joe's work. And he was the disseminator of the research discoveries that they made.

ROSEBERRY: How would you maybe kind of sum up his work, or the work of the Beard lab? I know that you've kind of outlined it, but—.

JOKLIK: Well, as I said, it was extremely important in identifying viruses as causes of cancer, and not only identifying them as such, but then also using them. In other words, taking cells, tissue culture cells, and infecting them with a tumor virus and determining how the tumor cells

differed from normal cells. Now, there was no great biochemistry done there, but primarily observation, that's always the important first part of an investigation like this. And of course, I mean, also inoculating animals and seeing how the tumors in animals developed.

ROSEBERRY: You mentioned that you got to know them, and—

JOKLIK: Well, not only because we were at the same institution from '68 to '73, but even before that, before, when I was at the Albert Einstein from '62 to '68, at major meetings, lectures on viruses were given, Joe Beard would certainly be there and either give a lecture or participate in a discussion and so on. So I certainly knew that Joe Beard was a distinguished virologist.

ROSEBERRY: And how would you describe both of them?

JOKLIK: Joe was a very forceful person. I mean, when he at a big meeting stood up to make a point, I mean, there's no doubt that a point had been made and established. So Joe was a very dominant person, although very, to people who knew him, very friendly. He traveled in Europe. He knew all the tumor virologists. One was Werner Schaefer, whom I myself got to know very well. And I remember that he was one who visited Joe Beard here at Duke, and we had dinner together at home. So Joe Beard certainly had an international reputation. He would have been known anywhere. Virologists anywhere would have known who Joe Beard was and the research that he was doing, and followed it. Dorothy was much more behind the scenes. She was active in the lab.

ROSEBERRY: Was it unusual for a woman to be in the lab at that time?

JOKLIK: Well, let us start off with a wife. That would have been very unusual, yes. Very unusual. I don't really know what influence Dorothy had on the scientific scene at Duke, because when I came in '68, I mean, she was just about to retire. When I knew her she did not work much in the lab anymore, because by that time she was sixty or so, sixty-five. But for a

long time before that she would have trained newcomers to the lab and been engaged in specific research activities.

ROSEBERRY: So when you came in 1968, I wonder what your vision for the Department was. You mentioned the Cancer Research Center, and I know that that's incredibly significant. What were some other—?

JOKLIK: For the Department?

ROSEBERRY: Yeah.

JOKLIK: Well, it was Microbiology and Immunology. When I came, there were just six faculty members, three microbiologists and three immunologists. One of the latter, (Bernard) Amos, was very distinguished; in due course both I and he were elected to the National Academy of Sciences. So, when I started recruiting, my aim was a Department of some 55 percent microbiology which included bacteriology, mycology (fungi) and virology, and 45 percent immunology. When I retired there were thirty-four faculty members. Virology, which was my field, never amounted to more than 15 percent of the department.

ROSEBERRY: But your work was primarily in virology?

JOKLIK: Totally. Only virology, yes. Molecular virology, not clinical virology (because we were a basic science Department). But of course each of the virology faculty members had major labs. I generally had ten to twelve people in my lab, and so did the others. Well, not quite as big, but still.

ROSEBERRY: I wonder if you might be able to talk a little bit about Dr. [Catherine] Wilfert. She had a joint appointment in Pediatrics and Microbiology.

JOKLIK: Cathy Wilfert is an outstanding MD and scientist. She's the wife of Sam [Samuel] Katz, who's the Chairman of Pediatrics. I'll always remember when in 1968, when we were

both recruited, it turned out that we arrived on the same day and sat outside of Dr. Anlyan's office, who was the Chancellor for Health Affairs, each waiting for our interview with Dr. Anlyan. And we each said, I'll come, if you'll come! (*laughs*) So Sam Katz and I joined Duke on the same day in 1968.

ROSEBERRY: Good day.

JOKLIK: We'll always remember that.

ROSEBERRY: Good day.

JOKLIK: (*laughs*) Right. Well, Sam is the husband of Cathy Wilfert. They had two children, and I think Sam, however, had a previous marriage and had five children from that. So together they had seven children. And Cathy and Sam were both MDs. Sam had a major role in developing the measles vaccine in Dr. [John F.] Ender's laboratory at Harvard and then having it become known and having it appropriated worldwide. Sam did very little experimental work after he came to Duke. I mean, he was a teacher and he was a communicator. He was one of the most important people in the United States in simply popularizing, as I said, the measles vaccine in particular and antiviral vaccines in general. Cathy, on the other hand, was both an MD seeing patients and also establishing a lab. And she said that what really got her interested and established, I think it must have been in the mid '80s, was when HIV, the AIDS virus, was first recognized. She said she would start with that, and what particularly interested her is what happened to children born of mothers who have HIV. And she found that azidothymidine, the trade name of which is Zidovudine, limits the multiplication and replication of HIV in children. And that is really what she established. This is something, obviously, of worldwide importance, and she is widely recognized as the originator of that. She has received outstanding acclaim for it and quite correctly, because she has done an absolutely fantastic job. When she retired, she

concentrated on work overseas, and the last few years, I mean, she has spent a great deal of time in Africa, in West Africa, in particular, controlling AIDS epidemics. AIDS, of course, is extremely important in Africa. I mean, there are millions of patients there. And she has organized campaigns in many African countries to reduce HIV. And for that, as I said, she has been recognized widely. She is a terrific person, absolutely wonderful. When she came, I immediately gave her a “joint” appointment in my Department; her primary appointment was in Pediatrics. When she was promoted in Pediatrics, I promoted her at the same time. So Sam popularized the measles virus vaccine and Cathy popularized inhibiting AIDS in children born to AIDS sufferers. They saved millions of people.

ROSEBERRY: Now, was her lab in Microbiology and then she—?

JOKLIK: No, no, no, no; her lab was always in Pediatrics.

ROSEBERRY: So how did the joint appointment work?

JOKLIK: Cathy gave some lectures on clinical virology, but not many, just on AIDS, to first-year medical students. There is a so a textbook, a medical textbook, *Zinsser Microbiology*, that I inherited from D.T. [David Tillerson] Smith who was an earlier Chairman of my Department. He was Chairman until 1963 when Norman Conant took over before I came in 1968. Zinsser was a Harvard microbiologist who started this textbook, which became the major microbiology textbook in the country. When he retired in about—when was it, in the 1930s, late 1930s, he wondered who would continue the book, and D.T. Smith said he would, and so it came to Duke. And when I became Chairman here I was asked to continue it, so I became Editor-in-Chief for six edition, the editions of '72, '76, '80, '84, '88, and '92. Each edition, I must say, cost me one year of my life, (*laughs*) because, I mean, it was so much work. Not only did I write a larger

segment than did any of the thirty-three other contributors, but I also organized them. And, of course, Cathy Wilfert was a very important contributor.

ROSEBERRY: I know that one year it was Dr. Wilfert, Dr. Willett, Dr. Amos and yourself: all Duke editors.

JOKLIK: That's right. Yes. We saw a lot of each other. We met and saw and also interacted a great deal. I don't know, I think Cathy might still—might be in Africa right now.

ROSEBERRY: Tell me about just women in basic sciences in general, kind of from your perspective, I guess, beginning in '68.

JOKLIK: Right. The fact is that few women entered science forty years ago. I once gave a seminar to graduate students at Penn State where a good friend of mine was Chairman of microbiology, and of twenty-two graduate students, nineteen were girls. But the future of these girls was primarily to become lab technicians or something like that. Very few would have gone on to medical school or obtain a regular faculty position. Now, on the other hand, women head scientific societies and everything. By now many of the leading positions in science and medicine are held by women.

ROSEBERRY: Was it felt, generally, that kind of women couldn't or weren't able to—?

JOKLIK: Yeah, I mean, it's the same as what the Taliban is still saying in Afghanistan. They're years behind the times!

ROSEBERRY: I know that you mentioned that your Department did have quite a few women.

JOKLIK: Yes. For example, quite early when I came, in the early '70s, I wanted an electronmicroscopist, and I hired a woman to be our electronmicroscopist who's still at Duke. She's still active here. She is married to a professor of pathology and has an academic appointment.

ROSEBERRY: What's her name?

JOKLIK: Sara Miller.

ROSEBERRY: And were there other women in the Department that—?

JOKLIK: Yes. There was Hilda Willett who was a highly respected full professor. I had a girl who was not a faculty member as the head of the Tissue Culture Facility. And in the Immunology Division there were two women. One was (and is) Becky [Rebecca] Buckley who has won several awards and is a member of the National Institute of Medicine; she is a highly distinguished immunologist. And Frannie [Frances] Ward who also was a highly regarded immunologist. When I stepped down from the chair in 1993, ten of our faculty were women. This was much higher than in any other Department.

ROSEBERRY: And that was throughout your Chairmanship, the ten?

JOKLIK: I started with two. Hilda Willett and Becky Buckley were here right from the beginning. I always looked for the best applicant, and if that was a woman, I hired her. Sara Miller, the electron microscopist, is a very good example. She is excellent and has done extremely well.

It is generally said that when they graduate from high school, girls are three years ahead of boys. And that is really very true for one of my grandchildren, one of Pat's grandsons. He lives in Richmond. He was at Roanoke College, and for the first three years he was interested in other things and so on, and didn't do very well. In his fourth year, three years later, you see, all of a sudden he became interested and did outstanding—got outstanding grades in fourth year, all A's and so on, and got a very good job after that. But many boys, of course, don't, you see. So that forty years or so ago, whether a girl was good or not, she was discriminated against and not employed. Nowadays they are employed, and it turns out that since they *are* ahead they *stay*

ahead for at least another five or six years while they do doctorates and so on. I think that by the end of this century we're going to be a very female-dominated society.

ROSEBERRY: Now, were there policies in place in Departments?

JOKLIK: No policies. It's just that you didn't appoint women. Whereas I wasn't interested in that. I mean, the best one that applied got the job.

ROSEBERRY: Let's see. I wanted to maybe return, if it's all right, to the cancer research that you did and explore that a little further, if that's all right with you.

JOKLIK: Certainly. What I'm proud of is that I provided the spark here at Duke for the creation of the Comprehensive Cancer Center. When President Nixon in his 1971 State of the Union message proposed a war on cancer via Comprehensive Cancer Centers to be established in top medical schools in the country, encouraged by recent findings that viruses can cause cancer, and I, a virologist at a top medical school on the one hand, and a wife with breast cancer on the other, proposed that we apply for such a Cancer Center. I discussed my proposal with key department Chairmen who were all enthusiastic; called a general meeting of the faculty, explained my proposal to them, and obtained their support; and, with Dr. Anlyan, went to see President [Terry] Sanford and obtained his support also. So Dr. Anlyan set up a Cancer Center Planning Committee with me as Chairman, and we got going. We appointed Dr. Bill [William] Shingleton who was a member of the National Cancer Board, as Director of the potential Cancer Center.

Then we wrote the application to be awarded the Comprehensive Cancer Center. And, of course, we also needed money in addition to that awarded through the grant. Of course, by now everybody was interested and the Jones family in Charlotte, through Ed [Edwin] Jones, a member of (I think I remember correctly) the Duke Board of Trustees, presented us with a gift of

\$8 million. The Basic Cancer Research Building of the Cancer Center is therefore called the Jones Building. And the same was true for the Clinical Cancer Building for which Ed Morris of Greensboro provided a sizeable gift so that the building is known as the Morris Building. So for both buildings about one half was provided by our successful grant application and the other half by extremely generous private gifts. And Pat, my wife, and I endowed rooms in the Clinical Cancer Building. My first wife, Judith, died of breast cancer in 1975, four months after Pat's first husband had died of lung cancer. This was before Pat and I knew each other. In the fall of 1975 Dr. Bob [Robert] Jennings came to be Chairman of the Department of Pathology after Dr. Tom (Thomas) Kinney. At a welcoming party for him Pat and I first met. About eighteen months later we were married. We have been married now for thirty-one years.

So the Comprehensive Cancer Center was built. And Dr. Anlyan, Dr. Wayne Rundles, and I selected Dr. Shingleton to be the Center's first Director. He was a great success. He was followed by Dr. Robert Bast, then by Dr. Michael Colvin and now by Dr. Kim Lyerly. All did outstanding jobs; the Cancer Center is very highly regarded. It is supported by an annual major grant of \$2-3 million from the National Cancer Institute (NCI). For the first ten years of its existence I was Director of Basic Cancer Research, as well as Chairman of the Department of Microbiology and Immunology.

ROSEBERRY: Are there other people or accomplishments of the Department that you'd like to mention as well?

JOKLIK: Yes; let me think. When I stepped down in 1993 we had a national search for my successor. The person who succeeded me was a member of my Department, Dr. Jack D. Keene. He made outstanding discoveries about how messenger RNA is translated into proteins. He has done outstandingly well and is known worldwide. He is invited to speak in Symposia all over

the world, in China, Japan, Europe, et cetera. Another is Peter Cresswell, an immunologist, who about ten years ago was elected to the National Academy of Sciences. I have already mentioned Dr. Rebecca Buckley, an outstanding immunologist who has been elected to the National Institute of Medicine of the National Academy of Sciences

ROSEBERRY: I know that basic science research in general has changed quite a bit, and I wonder if you could—

JOKLIK: Well, it hasn't really changed, but the more one discovers the more one finds out how extraordinarily little one knows. For example, a field has developed now where small RNA molecules, only about twenty nucleotides long, control whether a messenger RNA is translated into protein or not. And that has turned out to be an extremely important and interesting field. When I stepped down—I stepped down as Chairman in '93 and then as a faculty member in '96—absolutely nothing was known about that field, and that was just eleven years ago. And in eleven years that has turned out to be a field of enormous importance. How many more fields like that are there going to be? Because it wasn't even suspected, that small RNAs control the expression of genetic information. That was not known. Nor is the following: raising my hand like this involves a huge number of molecular reactions, none of which we know!! We have no, I mean no, idea!!! Another example is the following. Here I am, getting old, and my major difficulty is remembering names. That is very common. And often the names come to mind ten, fifteen minutes, or hours later: but why is that? Now, it is known what the molecule is that's diminishing with age, but why can't we supply it? One would think that that should be possible. *(laughs)* But it hasn't been done yet. So there's no question that we have discovered a lot in science, but in relation to what there is still there that we have little or no inkling of, it's negligible.

ROSEBERRY: Were there changes kind of within the timeframe that you were chair? Were there—?

JOKLIK: Well, certainly, yes. I was studying viruses. I started working with viruses in the early 1950s, and, of course, being a biochemist, I wanted to know in biochemical terms, in chemical and molecular terms, how viruses multiply, how they change cells and all that sort of thing. I was one of the leaders of molecular virology. For example, last year—oh, interferon is one, have you heard of interferon?

ROSEBERRY: I've heard of it. I don't know what it—

JOKLIK: Interferon is a cellular antiviral agent. It causes—it limits the spread and development of viruses. It was discovered in 1957, and I was one of the early workers on interferon. And very recently a society was founded called the International Society for Interferon and Cytokine Research. It's a big International Society. Its annual meeting in 2006 was in Vienna. Every year the Society elects one scientist as an honorary member, and in 2006 it was me. So I flew to Vienna and summarized for them what I discovered concerning interferon in its very early days. Things like that crop up all the time. For example, I founded the American Society for Virology also. Virology is always included in microbiology, but, of course, viruses are not microbes. Bacteria and fungi and so on are cellular organisms. They are cells like mammalian cells. And viruses are not cells. They are RNA or DNA genomes surrounded by protein. So about fifteen or twenty years ago a few of us started thinking that there really ought to be a society for virology. I was one of those who said that, and I finally suggested that those interested in forming an American Society for Virology should meet in Chicago, in a meeting room at the airport. About fifty of us met there, and we all voted forty-eight to nothing, I think, to found an American Society for Virology. They elected me as Chairman of that group. And when, a year

later, we had the first meeting in Cornell, Ithaca. I was elected to be the first President of what was the first international Society of Virology. Now most developed countries have one. There is a German Society for Virology and a French Society for Virology, an Australian one, a Brazilian one, and so on. I have been to several of them. And also we founded a journal, called *Virology*. George Hirst was the first Editor-in-Chief of *Virology* for about six years, but then for about twenty-three years, I was Editor-in-Chief of *Virology*, which was, for many years, the only journal for virology. Now of course there are others as well.

ROSEBERRY: When did women start coming into the field of basic biological sciences or—?

JOKLIK: Well, that was about forty years ago. When poor President [Lawrence H.] Summers in Harvard said that perhaps the reason why there are not more women in science is that they are not as well suited for careers in science, that was about the most stupid thing for anybody to say that I've ever heard. It's completely, completely wrong!! It's just that in the old days, one appointed men and not women. Women were *at home*! And nowadays whenever one sees a man retire from a top position not only in academics but also in business, the job is taken over by a woman. You remember what happened at Hewlett Packard the other day when one woman was accused of—do you remember the case? Well, in that big case at Hewlett Packard, something was leaked by a board member that they didn't want leaked. And the woman who was head of Hewlett Packard was very annoyed about it and got private detectives to investigate members of the board and who they might have talked to, and apparently used some, well, underhanded procedures—well, she used whatever means she had to. But some lawsuits were started against Hewlett Packard and her. I don't think anything happened, but, on the other hand, it created a lot of ill will and so on. But then she resigned, and her place was taken by whom?

Another woman! You just have to look at the paper and there are constantly women pictured in top positions.

ROSEBERRY: Did you receive any flak for having women in your Department?

JOKLIK: No, no, no. In fact, my Department was always highly regarded for having more women as full professors than any other.

ROSEBERRY: In general, do you mind kind of talking about your Department's relationship with other Departments, what—?

JOKLIK: That's interesting. There are joint or associate appointments. I was always very keen to appoint faculty members with primary appointments in other Departments as joint appointments in mine. And associate faculty members, either they, in the Department in which they have a joint appointment, choose to play no role, which is fine, or they choose to play a role; and then one can call on them for participating in teaching medical students or graduate students, and teaming up with faculty members for research, writing joint grant proposals and so on. And I think that's very good. That's what ought to be done. I'm not going to mention any names, but there is a faculty member in the Department of Radiation Oncology who is an outstanding biochemist, who has been interested for a long time in DNA silencing. If one changes cytosine molecules in the DNA by methylating them, it inactivates the DNA: it no longer transfers the information stored in it into messenger RNA which can then be translated into protein. And in this DNA methylation and silencing field, he is one of the top world leaders. And yet when he is not in a basic science Department; he is in Radiation Oncology. And when he came to my attention just after I stepped down in '93 as Chairman of my Department, I was keen that we should give him a joint appointment, but faculty voted against giving him a joint appointment. Nor has any other basic science Department; and he is one of the top leaders in a crucially

important field! If he had come to my attention when I was still Chairman, I would have been ever so keen to give him a joint appointment. Wherever he went he would have said that he was a member of the Departments of Radiology Oncology and Microbiology & Immunology at Duke. So some joint appointments work very well and others don't. And in some cases there might even be resistance. Of course, there are cases of faculty members wanting joint appointment for their own purposes that would not really fit. One has to make a judgment in such cases. But in most cases, I mean, I always interpreted the fit to be very wide. There are also scientists in the Research Triangle Park; at Burroughs Welcome; Glaxo, Smith, Kline; and so on who had joint appointments in my Department and were very proud of it

As for the question of relationships with other Departments, I never had any problems. In my experience, I found that Departments always try to help each other.

ROSEBERRY: Does much of the funding for basic research come from grants, or is it from—?

JOKLIK: Only from grants. In a place like Duke, solely from grants, yes. Personal funds, like Joe Beard's that I described earlier, are exceptional at Duke. If one doesn't have research grant support, forget it. The medical school has no sources of funds derived from its clinical activities. In clinical departments income can be used for research on clinical problems, but only within the Department.

ROSEBERRY: Are they grants from the NIH program?

JOKLIK: Yes, oh yes. I had NIH grants continuously from 1962 to 1996, constantly. There are personal research grants, and there are grants to research units. And then there are grants for the support of graduate students and postdoctoral fellows and so on. There are even grants for buildings, as for our Cancer Center. For these the NIH usually does not fund 100 percent of the total cost so as to ensure that there is input from the institution as well.

ROSEBERRY: What does it take to be a good researcher? What is important?

JOKLIK: Having a broad knowledge of the field so as to be able to choose appropriate avenues of research and obtain crucial information. What does one need to know? I was once in an unfortunate, so to speak, situation. It was a ticklish situation. Some Universities, when making major appointments like full professors, send applications to several outside reviewers. Some years ago a university was about to appoint as a professor someone who had been there as an assistant professor, then left for a few years, and now wanted to come back. I knew him because he was in the unit of a very good friend of mine who had in fact been a postdoctoral fellow in my unit. But looking through the applicant's bibliography, I really thought that there wasn't a single publication that described research that one really wanted to know the results of. I mean, one must have the ability to identify crucial new information. What Joe Beard did and what Cathy Wilfert has done was to supply critically important new information on which to build further research. But this guy—no. So I was tempted to write a negative note but didn't. But here was a person who had made a reputation doing research that didn't really advance knowledge significantly.

ROSEBERRY: Well, thank you, Dr. Joklik. I appreciate you taking the time this afternoon. It's been wonderful. Thank you.

(end of interview)