The American Association of Immunologists
Oral History Project

Transcript

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Williams: This is an interview with Dr. Jonathan Uhr for the American Association of Immunologists Centennial Oral History Project. Dr. Uhr is professor emeritus at the Cancer Immunobiology Center at the University of Texas Southwestern Medical Center. Dr. Uhr was president of the American Association of Immunologists from 1983 to 1984 and served as an AAI Council member from '78 to '83.

We are in Dr. Uhr's home in Dallas. Today is Tuesday, April 16, 2013, and I'm Brien Williams.

Dr. Uhr, thank you for doing this. We appreciate it.

Uhr: Pleasure.

Williams: Let's start with your family background. Tell me about where you come from.

Uhr: On the maternal side, which played the most important role in my life, my grandfather came from Russia to a tiny little town in Iowa, less than a thousand people, with his new bride, my grandmother. They moved from town to town because they were very poor and had to work very hard. My mother was born in Ottumwa, Iowa. They finally settled down in Oskaloosa, Iowa. My grandmother brought over her 11 brothers and sisters, and from them there emerged, as of twenty-five years ago, 250 progeny. We know that because I have a cousin, Devita Handler [phonetic], who came from Dublin, Ireland, and who was a genealogist. I'm not sure whether it was her father or grandfather who was the first Jewish mayor in Dublin, but in any event, she was a genealogist and we got appropriate information. Most of the Handlers were in the middle west, where they would meet their spouses at school, but some got to the coasts on either side.

Well, my grandfather became a pioneer in the movie business, and after about a couple of decades became very wealthy and moved the family to Detroit, and that's where they still are. I have five cousins there who are really like brothers and a sister to me. We spent every summer together on Lake Huron, where we had a tiny little cottage, but it just had everyone in it.

Well, my mother's father felt that women could do anything, so my mother was sent to law school, Detroit Law School, to keep her brother company. He was a wild brother, and there's no way he would have gotten through without my mother. My mother thought it was a second-rate law school. It was called, I think, Detroit Law School. So she went to the University of Chicago, where they let her take all of the exams, and she got her law degree, a second law degree, in something like seven or eight months.

She was very bright. When she was sixty-seven, just for the hell of it, she decided to take the bar a second time. She studied a little during the summer and breezed through the bar. Anyway, my mother was considered brilliant by everyone and

extraordinary. I never thought she was brilliant. I thought she was very bright, but had very good judgment.

Now, my father's family came from Hungary, and my grandfather came to New York City, and my father grew up there with his brother. Both families were the same, two boys and four girls in each. The youngest girl died in each family, so they each ended up with two boys and three girls that grew up together.

My father went to medical school and graduated, and so did his brother, who was eight years younger. My father became a pediatrician, but he was extremely talented in terms of music, so he thought he might become a conductor, but then decided it was more practical to be a physician. But his love of music persisted throughout his life, and he basically was a critic for our local newspaper that covered the concerts in New Jersey when we moved there when I was, I think, four years old.

He also had studied microbiology at the Rockefeller Institute, and when I was a small child, he encouraged me to pick up food from the floor and eat it. He said, "You'll have a good immunity that way." We would tease about "Pass the vermin, Herman." But in any event, it succeeded. I hardly have ever been ill for any significant length of time in terms of infectious diseases. In my forty years here at the medical school, I didn't miss one day's work because of sickness.

I became rather disinterested in problems. When I had a fractured skull, I just went to school the next day. I had blood in my chest once from tennis, very painful, and I just kept on going. So I think probably between my time in New York University and here in Southwestern, over half a century, I don't think I missed a single day's work. I don't have any allergies. Once or twice a year, I'll get a fever for a day or something like that, pay no attention to it. So I have to thank my father for that.

Williams: He's the one that said to you, "You will be a doctor"?

Uhr: Oh, yes. I was probably seven or eight at the time, and he pointed at me and said, "Son, you want to be a doctor. It's so much fun." It was fun for him. He just loved babies and so forth. So I just did what he said.

Later on when I had further training in medicine, he said, "You must learn pathology. Pathology and microbiology are very important." During the First World War, he co-directed the pathology and microbiology laboratories at Fort Sam Houston in Texas. Basically I just followed everything that he said, including coming down to Texas. He was sent to Siberia where we had an American army, where he also took charge of the microbiology and pathology laboratories.

So after my first internship, I did a year of pathology residency before I went on and did all the residencies in medicine. Of course I eventually chaired a microbiology department, so I certainly have fulfilled his wishes for me.

Williams: I want to go back and ask you a couple of family questions. When did your grandparents immigrate to this country, about what time frame?

Uhr: I think it was 1882.

Williams: And why Iowa?

Uhr: Well, my grandfather was a strapping six-one, very strong man, and in those days if you were Jewish, you had to serve in the army for something like twenty-five years, because all the Jews in Russia were in a certain area in the Ukraine. So they gave him a hernia to keep him from going in, but he was too strong; they took him anyhow.

When he came back after his first stint in the army, of course, his head was shaved, which is not good, if you remember with Samson. We don't approve of that. So it was decided that he would escape on his next leave.

So the next time he came, he was married to my grandmother. That had been, I guess, arranged. Then off they flew in the winter by sleigh to cross the border and then to come to America. He had a little paper in his hand with the name of someone in Iowa who would vouch for him and make sure he was not a problem for the country.

When they got to the station in Iowa, this little town whose name escapes me, there was no one there, and the stationmaster felt so sorry for these two young people with their four suitcases, that he let them stay within his house for several days till they could find one of their own. So basically they came there, I guess you could say, by mistake, and they had to just lead their lives there.

It's interesting that my grandmother had a first cousin who was raised with her as brother and sister by a grandparent. They both were orphaned. He went down to Buenos Aires, and he wrote to my grandfather and grandmother and said, "Should I come up to Iowa?"

And my grandfather wrote and said, "If you have enough to eat, stay where you are."

Well, he stayed where he was, and about twenty-five years later he had an enormous ranch and was extremely well off, and he had built little towns on his own railroad to basically take care of other refugees who came in. They're all named after his children. He had done very, very well. Well, that's it in terms of

that. We've been friendly with that family. We've gone down to Buenos Aires. They've come up here many times and so forth.

Williams: So just to review for me, you were born where?

Uhr: New York City. At the age of four, we moved to New Brunswick, New Jersey. I really left for college when I was sixteen—Cornell—and then I joined the navy when I was seventeen. In March 1945, we were expecting to have two or three years of war with Japan, but then as you know, it ended soon after that.

It's interesting that I was an only child, and I had a disease when I was thirteen, called sarcoidosis. No one quite understood what it is. They still don't know. It basically made me 4-F, but I had a very patriotic family and I lied about my health, and they also signed their permission because I was only seventeen. So I went in the navy when I was seventeen. I was discharged sixteen months later. All I did was really protect Long Island, but I became a boatswain's mate third class, and I got $100 extra every month, which was a lot of money in those days, because I was a sharpshooter. Went back to college and worked very hard.

Williams: So what made you choose NYU then as your medical school?

Uhr: Well, in those days—this goes back to 1948—if you were a premedical student and you were Jewish, they had very tight quotas, and it was very important at the college to be well rounded. So I was on the track team. I produced a radio program. I wrote lyrics for a musical, etc., to indicate that I was well rounded. I actually was quite immature.

But in any event, I got no responses from Yale or Harvard or even University of Chicago, where my mother was an alumnus, not even a response that said, "We've gotten your information. We will let you know," and so forth. Nothing.

The only reason I got into New York University was my father's co-director during the First World War was named [William C.] Von Glahn, and he became chairman of the pathology department at New York University School of Medicine. So in desperation when I'd gotten into nothing, my father called him, and two weeks later I was enrolled or admitted to New York University School of Medicine.

It's interesting that four years later when I went for an internship and residency, if you were Jewish, you could never become a chief resident in medicine or surgery. You can never become a chief of medicine or surgery. You could become a chief resident and a chief in all the other departments, pediatrics, psychiatry, etc., but not the two major ones. It was an unwritten rule. Now, to this day I don't understand how everyone knew this at every university medical school.

Now, that changed dramatically in the decade or two after I went into academia. That all was just thrown to the side and just changed completely. I'm also interested in how quickly it changed, but at that time it was part of the culture. Most people don't know about that. So that's why I got into New York University School of Medicine, and that's why I went to Mt. Sinai Hospital for my clinical training. I could become chief resident in medicine there, and I did.

Williams: This is a naïve question, but how did the schools know your background? Did you have to declare that you were Jewish on an application form?

Uhr: Yes, you did, and my reasoning for my mother getting into the University of Chicago was that my maternal side of the family looked more like Vikings than they do have Semitic traits. I mean, they're very blue-eyed and tall and thin and so forth. I once said to my mother, I said, "There's been mixing, Mother, of the genes."

"No," she said, "you're talking nonsense." And my mother's name was Mary. They didn't know what names to give them, just Mary Westman [phonetic]. She was a very good-looking young woman and blue-eyed and so forth. And how they all knew this just fascinates me. I don't know how this was understood. It certainly wasn't written down.

Williams: Amazing. Would you have had a different career had you gone to Harvard or Yale or Columbia or something?

Uhr: No. I think I'm very pleased with what I've been able to do.

Williams: I've heard that at that time NYU was becoming quite prominent in certain fields of medicine. Is that true?

Uhr: That's true.

Williams: Talk about that a little bit.

Uhr: They had a superb kidney group, and the head of it, I think his name was Homer Smith, was very, very well known. They had another five or six faculty who were quite famous, but they were not in the league with Harvard or Yale at that point in time, but they were a good solid B-plus, maybe not an A. The students there, of course, came from a much broader range of backgrounds and schools than at Harvard.

Williams: So as you pursued the M.D., did you begin to coalesce your thinking around certain specialties, or was it general at that point?

Uhr: I think it was just plain general. I had no interest whatsoever in science. I literally can't remember ever opening a scientific journal and reading an article.

My ambition was to become a very good clinician, a hands-on clinician. Mt. Sinai was kind of the last of the great clinical centers. It wasn't an academic center yet, so it just emphasized all aspects of history taking and physical examination. It was just superb.

So basically I was very happy there, but I had to wait six months for my chief residency. I didn't know what to do. So I talked a man by the name of A.M. Pappenheimer, Jr., a relatively famous name in immunology, into taking me for a six-months fellowship. That was very naïve on my part, but I needed some time. I had to do something.

So I went into the laboratory there, and basically I stumbled upon something that everyone thought was very important. I had no idea what I was doing, but I suddenly became the center of attention, and it was bizarre, to say the least. I mean, I was having lunch with Jacques Monod, who later became a Nobel laureate. I remember having contact with a famous pediatrician from Harvard, whose name escapes me now, who basically won the Nobel Prize for cultivating polio virus in a test tube, and he was telling me how exciting he thought my work was, and I remember I was now a chief resident coming back to just keep track of things in the laboratory, and I remember grabbing his arm and saying, "I'm sure you're doing interesting things too," not having any idea who he was.

Basically, it was nothing that was that important. It just was a flash in the pan, but it brought me incredible notoriety, so when I returned to New York University, where they begged me, "Come down and you can see if you like academia," I went and did come back.

Well, three months after I was back, I was offered the head of allergy at Mass General Hospital in Boston. I was terrified. I mean, it was three times my salary. I was getting $6,000 a year. This would have been 18,000. But I had no idea what to do there, so I just stayed in New York University and starved, so to speak.

But that got me hooked. Before that time, I just had no intentions. In fact, during my six months in Pappenheimer's lab, I flew down to Florida two times to take the preclinical boards and the clinical boards to get a license in Florida to practice medicine. I thought it would be an attractive place. I loved water. I could see I could have a nice boat there and so forth. Science was kind of—had been in the past, but, no, they insisted I should give it a try, and I did, and I got hooked at that point.

Williams: What was the thing that you were working on that brought this early notoriety to you?

Uhr: Well, I think basically what I did—I can't even remember, to tell you the truth. I just know I was working hard. My first child had been born. My wife was in

New Jersey. I went out there every night, I guess. It was a long trek going there and coming back in the morning.

But from that, although that wasn't important, I then did two things during the next fifteen years at NYU Medical School which I think were important. The first one which came from that, I'll call antibody feedback mechanism. I showed that antibody that is formed feeds back on the immune system and keeps it from making more antibody, an antibody feedback mechanism. I used to talk to a former medical student colleague of mine who was doing work with prisoners at Sing Sing on the Rh-incompatibility problem, and I think I had a modest influence on them. In any event, they basically solved the Rh problem of disease. When a father is Rh-positive, the woman is Rh-negative, with succeeding pregnancies, she begins to make antibody and therefore can react against the fetus and kill it. They showed that if you gave antibody to such women passively, it would feed back, and they wouldn't make new antibody of their own. So it was a major accomplishment. In fact, they got a major prize for it, which I wasn't included. [laughs] So that, I think, was a worthwhile piece of work.

The second one occurred just a year or two before I left New York University when I for the first time began to do biochemical studies, or I should say just chemical studies, of the cell surface of lymphocytes and looked at how antibodies are traversed in a plasma cell and secreted. It was rather basic studies and it opened up an area. I kind of rate myself as an A-minus for that fifteen-year period. It was some very good work, but not outstanding.

Williams: I notice you'd spent a little bit of time in Australia.

Uhr: Yes. I think in 1961, for three years after I started at New York University School of Medicine, I had a fellowship, and I went to Melbourne to work with Sir Macfarlane Burnet. Just before I left for Australia, he won the Nobel Prize with Medawar. They shared the Nobel Prize. I claimed my stock had split, and I had an excellent time. I loved Australia.

I learned a lot from being with Sir Mac, and it was really very interesting. Basically, he was rather a loner in his institute and didn't talk to any of the others, and I could see he wasn't going to spend any time with me. So every time the bell rang for tea in the morning and afternoon, I would dash in and sit at the head right next to him and pepper him with questions. So the staff really enjoyed my time there. We became quite friendly, so when he would come to New York, my mother would take his wife around, Lady Burnet, and I would host him and so forth. We had a good friendship.

Williams: Were you married when you went to Australia?

Uhr: Yes, I was married and had two small children.

Williams: Did they all go with you?

Uhr: Yes.

Williams: How long were you there?

Uhr: I was just there for six months.

Williams: What about your service with the Irvington House Institute? What was that about?

Uhr: Irvington House Institute, prior to my becoming head of it, had worked out the natural history of rheumatic fever. It was an incredible feat. I didn't participate in this. It had been done already. Basically, I realized that the important thing was to do research. They already had determined the natural history of disease. I thought to myself, rheumatic fever is probably an autoimmune disease, but it's the only one where we know the etiological agent. It comes after a beta-hemolytic streptococcal infection.

Well, I managed to get a hold of a huge primate colony, twelve chimpanzees and twenty-four baboons. When the chimps came, they were just babies and they all had colds, and we just held them in our arms and just tried to cuddle them and make them feel better. Then over the next several years, after I got a very large grant to do the study, we began to give both the chimps and the baboons, particularly the chimps, streptococcal infections, and we had to give them different types. It's something I wouldn't do now that I've become an animal lover, but in those days we just weren't that sensitive to those things.

So this went on for three years and nothing had happened, and it was very discouraging. I had a huge grant and obviously couldn't put it in again without any results, when one night I got a phone call from the primate colony that one of the chimpanzees had a heart murmur. Well, that was incredibly exciting. I mean, if we really were going to produce rheumatic fever in an animal, which had never been accomplished in all the normal mammals, guinea pigs, mice, etc., this would have been a huge boon because we could have worked out the details and the mechanisms. The problem was how would we determine whether this was a real organic murmur from rheumatic fever or just a trivial sound, which is very common. We all have little sounds here and there from our hearts which aren't indicative of an organic condition.

So we decided we had to have the chimpanzee catheterized. So we dressed the chimp in children's clothes. And this time the chimps were big and strong. We had to be rather careful. In fact, even when they were babies, one of them tapped one of the physicians in my group on the sternum and broke his sternum. They're very, very strong. They can bench press a thousand pounds as soon as they're adults.

So we tranquilized the chimp and dressed it in baby's clothes, got it in a convertible, drove it into Bellevue Hospital where we had forewarned the appropriate groups, and the chimp was signed in as a patient, and was on a stretcher going through the halls to the cardiac catheterization lab, and it was rather surprising that no one seemed to notice that this was a rather hairy face with the green hat on and so forth.

It was catheterized and brought back, where it signed out against advice, was retranquilized, put into the car, and drove home again. Now, I don't think that would have gone over very well if we had been discovered, but there was too much at stake to not do this. This was a critical thing. Now, unfortunately, it turned out to be a functional murmur, not an organic one. We had failed, and that kind of ended my attempts to do something pertinent for rheumatic fever.

I think we did one other thing. It was Irvington House Institute for Rheumatic Fever and Allied Diseases. So we also had a group of patients, mainly young patients, who had arthritis, which we thought might possibly be due to a slow-acting virus or something like that. So we took material from them, I don't remember where, and we injected that into chimps to see if we could get rheumatoid arthritis, and that didn't succeed either.

Williams: So with this bounty of animals at your disposal, did anything come of working with them?

Uhr: Not a thing. Not a thing. We also looked carefully to see if they got post-streptococcal glomerulonephritis, which is another human thing. We did very clever things with them. I mean, for example, you had to change the type of streptococci every time we challenged them, because you get type-specific immunity. Well, there are many different types. For example, somewhere, I think the Netherlands or Belgium, a whole town had become sick with rheumatic fever after the milk got contaminated with streptococci. Well, we reproduced that. We did a lot of clever things, but they all went to naught.

Williams: Any other highlights of your time while you were at the—

Uhr: Well, I will say this. At one point, and probably the reason I left New York University, was the Irvington House Institute did not have enough money to continue this famous clinic. This clinic, basically they worked out the life history of rheumatic fever, found that it was just critical to get follow-ups of ten, fifteen, twenty years. Well, that's very hard to do with the population that they serve, many of whom were from impoverished and minority groups, etc. So they basically would do everything. They'd go to the home. They would bail the father of a rheumatic fever individual out of jail so they could keep this family in close touch. And that's how they managed to get this really excellent idea of

what happens with patients with rheumatic fever. You needed a very high follow-up rate, like 90 percent, not 30 or 40 percent.

Well, it was very expensive, and I felt that the research we were doing in basic immunology was the future, and the clinical studies had been completed, so we closed the clinic. Immediately those physicians who were involved in the clinic protested and began to do things. This is basically in 1960s, when we were having problems in Vietnam and people were protesting everywhere. They began to picket and close up the clinic, etc., and soon Bella Abzug, the communist leader, New York, joined in, and other minority groups, and basically began to picket and to do physical things to keep us from going into the laboratories and the clinics and so forth.

I remember sitting at home with my two children, who now were old enough to watch TV, and they were looking at the television and at the signs of the pickets saying, "Dr. Uhr. Dr. Uhr hates our children. Why does Dr. Uhr hate our children?"

And they said, "Well, Daddy, that's you there. Why are they saying that about you?"

And I had to explain that I really was an innocent individual in this, that this was a judgment and the right judgment that had to be done. The clinic was now in Bellevue Hospital, but it wasn't the same old clinic where you could just do everything for the families to keep them literally attached to you and not moving away.

So, anyway, it really was a very unpleasant time, and very few of my medical colleagues stood up and said this was wrong. So, basically, I decided at that point after a year of that, that I didn't have a future, that the Institute would not have a future there, and I decided to leave. That's when I began to pursue other opportunities, which were all over the place.

Williams: Did the Irvington Institute continue? This was just a clinic within it, is that correct or not?

Uhr: I think they decided to do something different and kind of phased out over the years their place in New York University, and they went and made an arrangement with some other medical school to do something, but I'm afraid that kind of started them on a downward course in terms of financing.

Williams: So talk about the process of eventually selecting to come here.

Uhr: Well, I was looked at for chairmanships in pathology and microbiology, and at this point in time also at New York University, I had to make ward rounds three to six months a year at Bellevue Hospital, and for seven years I was assigned to

the psychiatric and prison wards, and it was exhausting. After making rounds, you'd come back to the lab, and you'd kind of been depleted of energy for your basic research. And I was on all these committees, up and down the line. I realized that I might as well take a chairmanship in a basic science department, but I definitely wanted to continue to make ward rounds at some level.

I can't remember all the places I visited. Anyway, when I came down to Dallas, they had just put up a new beautiful building, and I was allowed to have twelve new appointments to the Department of Microbiology. It was just an excellent offer. I'm very athletic and I'm an outdoor person, so as soon as I accepted, I immediately—I didn't come down for a year, but I enrolled in the tennis club, you know, and tried to get seats for the Dallas Cowboys football game.

I just love Texas. It has very good manners and very friendly, and it's a very attractive place for me to be. I was brought up with very good manners, and New York City and I didn't get along very well. I can remember getting on a bus and seeing the bus driver yelling at some older woman one morning, looking at my watch and deciding, well, should I begin my first fight of the day now or should I wait till I get to Bellevue where I have trouble with the elevator guy, we always are fighting, and so forth. New York City was not my cup of tea, and Texas was.

Williams: What kind of bones of contention can you have with an elevator operator?

Uhr: Who knows. I don't remember. He just was rude to people and so forth, and I don't like rudeness. Other colleagues there and so forth would have arguments. It just wasn't the kind of culture at the medical school here. People just behave very nicely to each other.

I can just tell you one place where I went. The dean met me at LaGuardia and went—it was a southern school. As we went down there, in addition to discussing the position, the chairmanship of microbiology, he was complaining about his son, who just was a bum and so forth and so on. I said, "Well, you know, let me talk to him. I can be quite convincing."

So when we got there, had breakfast, I went into the room and here was this young man, lying there in jeans, torn jeans, music around, and looked drugged to me and so forth. I gave him a thirty-minute discussion of why he should think of becoming a doctor, how good it was, how many different things he could do. That was my one and only meeting with—I'll have to fill in his name. It's a very famous name, but it escapes me for the minute. His father's name was Ezra.

Williams: That's all right. You can fill it in in the transcript. That's no problem.

Uhr: Okay.

Williams: But you turned him around?

Uhr: Not at all. He just continued on his ways and became one of the most famous singers in our country. [laughs]

Williams: I jumped to the conclusion that you got him onto science.

Uhr: Nothing.

Williams: Not at all.

Uhr: Nothing.

Williams: Interesting.

Uhr: First name is James.

Williams: Taylor?

Uhr: James Taylor. My one and only interaction with James Taylor. His father was dean of the University of North Carolina Medical School. There he was with his music, a bum, and I didn't convince him, fortunately.

Williams: You came here to a microbiology department? That's what it was called at the time?

Uhr: Right.

Williams: Talk a little about what it was like when you got here.

Uhr: Well, I was really very lucky because there were just three people left in the department. One of them was an excellent microbiologist, and one of them was an elderly but good microbiologist, a good teacher, and then there was a third person who taught parasitology, who wasn't a scientist and he did nothing in terms of science. The dean at that time was Charles Sprague, a wonderful, wonderful person. He was a hematologist from Tulane, also an All-American football player with a big heart. Oh, just a wonderful person.

Basically, I was able to make twelve new appointments in this beautiful new building. The clinicians were still in old buildings, but basically they felt it was important to have a new basic science importance in the school, and therefore they stayed in their own little quarters, and this new building was all for basic science. It was a very unselfish attitude.

The medical school has an incredible history. Donald Seldin, who was an assistant professor who came down here when it was nothing—I mean, there were a few Quonset huts near a garbage disposal place. It didn't even look like a

medical school. And he basically built this up and probably became the most important physician, scientist leader, or certainly one of the most important in the twenty-first century, twentieth century. He just built the whole school up. I mean, the school has five Nobel laureates now, just unheard of, and had four working ones, which was unique in terms of medical schools. In fact, *Science* magazine once published an article about this incredible new school that came from nothing because of Donald Seldin and what he did.

So, anyway, I was able to be an effective chairman. I considered myself a kind of benevolent dictator. I didn't want committees. I wanted to make all the major decisions. So I wasn't a popular chairman. The faculty, I kept them sullen but not rebellious, and I made wonderful appointments in immunology. We had as strong an immunology group as any other medical school in the country. We just could get the students and fellows we wanted. The only one that was comparable was University of California in San Francisco. They also had an excellent immunology group. You don't want to hear the details of that. Just very, very strong.

Basically, I had a good group in microbiology and some good virologists. It was a very strong department. I feel it really was a big cause of the basic science growth of the medical school. We were really the first department that were that strong. But basically I made all the decisions and so forth, and I stayed on past twenty years because the new president begged me not to leave after twenty years because they had other two vacancies in basic science. They didn't want to have three; it wouldn't look good. So I stayed for the twenty-five years. I think that my work in immunology, I'd give it a high mark, but I don't consider it breathtaking, just an A-minus.

About twenty years ago, or twenty-five, I began to slip into immunologic approaches to help patients with cancer, and fifteen years ago, I started a field. The work I've done in that field, that I'll give an A-plus to. I'm not fully recognized at this stage. I'm too old. I don't go to meetings and talk about it enough. But I became interested in trying to find a method to capture cancer cells in the blood. With the help of a firm called Immunocon, we developed such a method. I published and patented it in 1998. There was no field of circulating tumor cells at that time. I couldn't even get a grant. People said, "What are these cells? We don't know anything about them." The usual response to something new, rather than, "Wow! Let's find out what we can do with these."

Well, Johnson & Johnson developed an instrument called CellSearch, which counted the number of cancer cells in a whole bunch of cancers, and found that they could prognosticate on the basis of the number of those cells. Above a certain number, you had to treat very aggressively. Below a certain number, no, the prognosis was quite different. Moreover, you could see if the various drug regimens were working, because if they did, the number of cancer cells in the blood went down. If they didn't, you had to try something new.

So in 2009 it was voted the most important medical invention by the U.S. Pharmaceutical Industries, and the French gave it a similar rating, some special name, a very high prize, and Johnson & Johnson nominated me as Inventor of the Year. Now, mind you, I'm pretty old at this point in time. I tried to get Johnson & Johnson to give me a little money so that I could push things more quickly. They had made literally hundreds of millions of dollars from this invention, and I wanted another couple of hundred thousand to press ahead a little more quickly. That would be nickels and dimes in our pockets. They came down and visited me three times with groups and never gave me a penny. I'm not surprised.

I had grants for other things in oncology from Abbott and from Eli Lilly, who did things that I wouldn't believe, except that I saw them, just ridiculous things. Pharmaceutical companies, they're not any brighter than our federal government, let me put it that way. They do very bizarre things.

In any event, I'm still working on this, unpaid. I think if I can do the things I want and they would work out, it would be a step towards using this to diagnose all the cancers earlier, or let's say most of the cancers. I mean, that would just change things in a major way. In addition, I found that patients seven to twenty-two years after mastectomy for breast cancer, who are perfectly well, 90 percent of them will live out a normal life without a recurrence, have tumor cells that I could detect unambiguously. Well, our test isn't that sensitive. I'm sure if it was more sensitive, it would be 80 percent, maybe everyone, maybe every patient with breast cancer.

Here they are, these cells only last an hour to two hours in the blood, so they're being replenished at the same rate as they're dying. One patient was twenty-two years after mastectomy with this sustained low-level of circulating tumor cells. So there's a precise balance between replication and cell death. Now, this shocked everyone. Everyone thinks that a small percentage of these patients have dormant cancer which isn't dividing, and then this small number, 5 to 10 percent have a relapse. Well, that's not the way it's going. So people refer to this, but they don't know what to do with it. They're still hanging on to the dormant philosophy. Most in the field think this is due to an immune response or a lack of vascularization.

But the only part of the mammalian body, not only mammals but invertebrates, that is absolutely precisely regulated is organ size. I mean, we have a certain half-life for red blood cells, for platelets, for different kinds of leukocytes, and if I take out half of your liver, which never divides, it suddenly begins to divide. It goes right down to the costal margin where it should be and stops. Or if I take out one of your kidneys, the other kidney which normally doesn't divide suddenly begins to divide, grows larger, and then stops. So if you think about it, we need this mechanism.

Well, the mechanisms in the fruit fly, the genes that are involved in the size of the wing, in the cells in the wing, etc., organ-size control mechanisms are all conserved in the human. We have those same genes. My hypothesis is that it's that system that's contributing in a major way to this precise regulation of the tumor cells, that the body is looking at them and saying there's enough breast cells. They don't care whether they're neoplastic or not. They're not looking at that. They're saying, well, this is enough and this is how we control the number of breast cells. In this case, they're in the blood. Well, I shouldn't say that. They're in the tissues, and they're being regulated, and they're spilling the dead cells off into the blood.

So if I were younger, I would be working on this fifteen hours a day. There's no reason to think I would succeed, but if I did and managed to get a more sensitive assay to routinely pick up cancer in healthy people and begin to treat some cancers like chronic diseases like breast cancer, basically knowing the signaling pathways and giving just like you give insulin to a diabetic, keeping them on a good balance, I'd expect to be giving a talk in Sweden.

Williams: A couple of questions related to this. When you identify the cancer cells in the bloodstream, do you also identify the site of the actual cancer? Is it tagged or not?

Uhr: No. Basically, we've never gotten enough cells from these patients with breast cancer to really interrogate them. We have developed a hyper-spectral microscopic platform, which looks at colors in much more broad and profound ways than ordinary microscopes. I mean, I didn't invent this, but I've basically overseen its development. It can now quantify eleven different colors, eleven different fluorochromes. So by attaching the fluorochrome to an antibody to what you'd expect to see in a tumor cell, we can look at the nucleus and ten different tumor markers.

So if I could get enough of these tumor cells to do that, I could begin to answer the question that you raise. I could begin to show that basically, for example, they express mammaglobin, which isn't done by many other carcinomas, but it is by breast and so forth. And I could begin to look and see other qualities of it, and one can do genetic studies.

Right now I'm struggling with the following problem. The field of circulating tumor cells has now exploded. It's a huge field, many pharmaceutical companies, many different investigators, and basically they are doing various things with them. But the whole push from the scientific point of view in terms of these patients who have, let's say, breast cancer are—let me put it another way. Everyone is trying to increase the sensitivity of this by physical magnetic thrust, let's say. In other words, they're changing the particle. Because the way we do it is we put an antibody on a particle to the tumor cell, and then it captures the tumor

cell, and we have a magnet that pulls it to the side of the tube, and that's how we isolate these cells.

But no one has paid much attention to the immunological aspects, and there are many very fancy things one can do now in terms of the antibody to make it stronger, to make it grab harder, to make it stay on, and then you can have it to more than one particular aspect of the tumor cell. The tumor cell has a number of different proteins on it that can be used as attachment for the metal particles with their antibodies.

I'm struggling to get these done, and I don't have enough money, basically, to pay pharmaceutical companies to develop just what I want. I'm struggling with the medical school now. I have an endowed professorship in my name, and I want to use it to hire a young scientist and have the school commit themselves to continue this work which all took place here. I think it would be very helpful to the school academically in recruiting good oncologists to the school, and it would help them in terms of the local community.

Williams: Your guess is that this balance that you describe between short-lived blood cancer cells and so on, that an immune factor isn't involved there, they're not being attacked?

Uhr: Pardon?

Williams: They're not being attacked. The cells die after two hours because of what?

Uhr: Well, because we don't allow epithelial cells. All the carcinomas are epithelial cell neoplasms. Now, we can't afford to let an epithelial cell stay alive in the circulation. I mean, if you should have your intestine injured or, let's say, operated upon, or you have an accident and you have an intestinal cell or anything in your blood, you can't afford to have it go to your toe and become an intestine in your toe. So they go into a program called apoptosis, which is a planned program of death, and you need that to basically keep, again, your tissues intact. So, basically that's why, whether it's a neoplastic epithelial cell or a normal epithelial cell, it basically is programmed to die. Now, if it's a neoplastic one, it may be able to evade that, because neoplasms keep on mutating and so forth, and that's how you get metastases.

Williams: How far along was the science?

Uhr: Can I just explain why I don't think it's an immune response? If you immunize twenty people to diphtheria toxoid, for example, you'll get antibody responses all over the place. Some of them will have none to the first injection. Some will have a high level. Some will have a certain kind of antibody to toxoid, and others will have another kind of antibody. So the immune response is all over the place. As you get older, your immune response gets weaker. We know that. I just don't

think it's strong. I don't think there's good enough evidence to suggest that that plays a role.

And the same thing from neovascularization. Judah Folkman speculated a long time ago, and it's a very popular concept, that basically you don't have enough blood supply to a group of tumor cells, and they'll be dormant. Then you'll increase the blood supply, and off they'll go. Well, again, I don't see that as being so precisely regulated that someone fifteen, twenty, twenty-two years later can have this turnover in the tissues and be that precise. That's why I'm interested in following the hypothesis of an organ-control mechanism, which are poorly understood in the human. They haven't been worked out yet. I don't know why. It's a very exciting area.

Williams: I'm curious about how far along you developed the science before you gave over this to Johnson & Johnson. In other words, did they also do some research and refinement, or did you hand them the finished product?

Uhr: No, the firm that I worked with was headed by a brilliant man whose name is Dr. Paul Liberti, and we developed this. Immunocon was on the patent, as well as our medical school and myself. They made a contract with Johnson & Johnson to retail this particular instrument. They had developed the instrument CellSearch. Well, Johnson & Johnson simply did nothing for several years until Immunocon went bankrupt, and then they could just take it all over for a song and a dance. Then they proceeded to sell this machine on many parts of the world, and very expensive to run as well. It's not quite that popular now, but it was extremely popular for many years.

Williams: So let's get back to the department here. It grew under your direction, and then were there structural changes that occurred? For example, in '98, I guess, after you were no longer chairman, the Department of Immunology was developed, right? There was a split.

Uhr: That's right.

Williams: That was after your time, is that correct?

Uhr: Well, yes. I'm trying to think back just how that went. The head of the Department of Immunology was a postdoctoral fellow with one of my faculty, Ward Wakeland. He's done a very good job. The microbiology department took one of my microbiologists, Michael Norgard, who is chairman of this, has done a superb job, better than I did. I mean, he really consults with the faculty, very popular, and has made some excellent appointments.

Williams: I've heard high praise of you as a mentor, and I'm surprised, then, to hear that you ran your department like a dictator. Aren't those two concepts a little bit in conflict?

Uhr: No. I basically was a good mentor and I'm really a sweet guy. I have to just be tough to get what I wanted to do, but, you know, I don't really enjoy confrontations particularly and so forth. I think I was a very supportive chairman, but I don't go in for a lot of frills, etc., and so forth. So when it came down to the bottom line, I was very direct, and it would get me in trouble scientifically as well. When I didn't think someone was presenting something that wasn't solid and so forth, I would get right up and say so.

Amusingly, one of my faculty who just tortured me became head of an institute of his own, and six months later he came up to me, apologized all over the place. He said, "I didn't realize what I was doing to you until it's done to me. He said, "They're just torturing me for every kind of complaint, decision, want me to rewrite the menu in the cafeteria in the institute I'm head of."

Well, this person started, as a tradition, to come in the fall—he's from another state—and have two other full professors formerly of the department get together with me and have dinner together and go to a Cowboy football game the next day. Throughout these several years when this is developed, their attitude towards me just is this remarkable change. They now have forgotten about how bad I was, and now they say, "You were the most wonderful, supportive chairman." I mean, you just this and this and that. That's not how they treated me when I was chairman. I guess their own children and grandchildren have made them aware of all the problems in life and they've become extremely empathetic, and I find it rather amusing how they tell me how sweet I am and so forth. That's not how they were when I was the chairman.

Williams: What's it like to be a professor emeritus?

Uhr: Well, I mean, basically it really is a problem. For example, from this patent that I have, significant funds would come from it to the school, but I'm not allowed to utilize them. They go straight to the department, in this case the Cancer Immunobiology Center. They would have helped a great deal, and I can't get them and I can't do anything.

Williams: Let's turn to your time at the American Association of Immunologists. You were president in '83, '84. Do you have any outstanding memories of your presidency or other associations with the organization?

Uhr: Not at the scientific level completely. I mean, it just was part of your interactions with other scientists, and you'd see them, if not at the immunology meetings, you'd see them at other meetings, or meetings all over the place. I used to go to those once a month, frequently abroad, where you'd see the same people.

While I was a tough chairman, we played hard as well, in addition to working very hard. So we had our own band, and our own band would frequently play at

the annual immunology meetings, and we would put on skits for them. We had a lot of fun as well.

Williams: Do you recall what your band's name was? I think Dr. [Ellen S.] Vitetta—

Uhr: Dr. Vitetta probably told you.

Williams: Was it the Microbes?

Uhr: Yes, the Microbes. Yes, and they actually were quite good. They would play once a week at one of the pubs in Dallas. They were quite good.

Williams: Were you a member of the band?

Uhr: No.

Williams: Any particular recollections from being president of the organization? What words would you use to describe that experience?

Uhr: I'm afraid I can't. We were so involved in the research, and I'm very picky at that level. I like to have solid data and I like to have hands on. I used to do all the assays myself when I was younger. I just can't remember. I remember having to give a talk, and I spent quite a while on that talk.

The only thing I remember, I think I spoke about Ricin. I may not have. But I remember I wanted a last line that was a little erudite, and I was very friendly with [Gerald] Gerry Edelman, who won a Nobel Prize for his work. I think he must have been a chairman at one point, wasn't he? Or no?

Williams: I don't think so.

Uhr: He worked for Henry Kunkel and did the amino acid sequencing of antibodies. But, anyway, he gave me a particular line that I liked. It wasn't exactly what I wanted. It was close enough. And I remember getting that from him to finalize things. I gave a good talk, a very good talk, but, again, not a knockout.

Williams: Were there any particular issues that you dealt with? This was in Ronald Reagan's first term, when tax cuts were—

Uhr: I'm sure there were. I mean, I've had a lot of committee appointments that have been very interesting and brought up a lot of issues. For example, I became deputy director of the Commission on Immunization of the Armed Forces Epidemiological Board. This was a group that gave the armed forces recommendations for immunization during the Vietnam, Cambodia campaign, etc., and I served on it from—I think it was '59 to '71 or '72 when, for legal

reasons, it was stopped. It was an excellent group. It was my first exposure to making very critical judgments when you didn't have all the information.

I was just dumbfounded at what had to be decided. For example, when I was deputy director, unfortunately, one day the director was out of the country and he couldn't be contacted when I got a call. The Marines who were getting their typhoid immunizations were dropping like flies. Something had to be done. There was no time to get some new vaccine or anything like that. A decision had to be made. Did you cut the vaccine down? Did you give it in two different, three different days, or something like that, etc.?

Now, that was a critical decision. The vaccine was only 70 percent effective. Well, let's say you cut it down and it's only 45 percent effective. Is that platoon going to be out of commission because of that and overwhelmed and have everyone killed? A decision had to be made. That's all I remember is that I was very concerned and basically called all over the country to talk to everyone I could think of.

Then I reached a point where I thought this was the best decision I can make. I can't even remember what it was. But that was a whole different ballgame than science, where you can make a judgment about whether you're right or wrong, what you're going to say, and so forth, and there are no great consequences unless you do something ridiculous. This had great consequences.

I was very impressed also by the people on it. There were some excellent scientists, and everyone thought long and hard about our recommendations, which were carried out usually to the extreme. So that was a very interesting experience.

Then I was a Howard Hughes Institute reviewer with Henry Kunkel and Bill Paul and myself for many years, and when we went down to the Howard Hughes Institute in Florida, where all of Howard Hughes' Mormon guys who had taken care of him, no one was quite sure whether they had really been good or not been good to him, but I can tell you it was very clear to me he was the boss even until he died. I remember talking to his lawyer and saying, "How could you let Howard Hughes die without a will? Look at all that's happened."

He says, "You don't understand. I asked Mr. Hughes thirty-five years ago, 'Can I make a will for you?' and he said no. You never ask Mr. Hughes again, if he said no, the same question." They all were very respectful in terms of that. So it was very interesting.

Then I ended up on their medical board, which is a brilliant group of scientists. Every one of them was brighter than I was, as far as I was concerned. They were very smart. So I spent close to twenty years, and it was a very interesting experience because it illustrated how tradition is carried on unconsciously by all of us. I mean, Howard Hughes was very out of the spotlight, and while the new

Howard Hughes Institute there, you can hardly find it. The number is so small on the thing, and when you go there, when I was there, you had to be in by twelve o'clock and you couldn't bring your wife. I mean, all these odd things that Howard Hughes represented are being carried on by new generations that don't even understand it.

Williams: So how does Howard Hughes Institute figure in the overall mix of—

Uhr: Oh, I think at the point when I left, which is about, I'm guessing, a decade or more ago, I'm not even sure, probably fifteen, twenty years ago, maybe, it was supplying 20 percent of basic research funding. Huge, very important, and it is at most good schools, the Howard Hughes investigators.

Williams: And that continues to this day?

Uhr: Yes. Very interesting.

Williams: You also have held positions in the AAI with the International Union of Immunological Societies.

Uhr: Yes.

Williams: Talk a little bit about U.S. versus the global scene.

Uhr: Well, there hardly is a country that doesn't have some excellent immunologists in them these days. I mean, certainly the western countries, but, you know, everywhere. There's excellent ones in almost every country in the world. I have to say that I had made good friends of two very good scientists who were immunologists, and they both came to work with me when I was in New York University School of Medicine. One was Gus Nossal, who's been knighted and is very famous in Australia now, has played a major role in immunology. The other one is Sir James Gowans, who basically came very close to getting a Nobel Prize for his work on the life of the lymphocyte. He first described how lymphocytes, where they go and come from, and their circulation. I'm very, very fond of Jim Gowans. He's several years older than I am. He now is pretty much confined to his home because of a neuropathy.

I would go and see him every time I went to Europe, and we would meet. I just saw him on my way to Egypt last November. We went to Egypt. I do like adventuring abroad, have taken my poor wife to every strange place you can think of, Africa seven times. I love animals, fascinated by lions, tigers, elephants. Anyway, we went there, and I stopped and spent a night with him at his home.

I'm taking my wife on a European trip, haven't done that for a long time. It's always been, you know, Tibet or Antarctica and so forth. So I'm going to see Jim again this following September when we go on some normal cruise, Venice, etc.

He's a wonderful person, very humble and very interesting. I will talk with him about his interactions with Thatcher. He was head of our equivalent of NIH for many years, so he had to meet with the prime minister several times a year. He has very funny stories.

But in terms of science, I mean, Sweden is excellent. Norway, we go there a lot. I've been to Sweden at least fifteen times for various meetings, Nobel-type meetings or immunology things or otherwise. I'm very friendly with the Möllers, who were major figures in immunology. Immunology is just exploding, and it's very exciting. I'm not up on all the things in immunology now. I have to just focus at this stage on my circulating tumor cells. But it's an incredible field.

Williams: If you had to do your life over again, would you have made major other decisions?

Uhr: No. Very pleased, very pleased. I mean, it was difficult being poor for so long. To send my children to camp, I had to be the camp doctor. I had a difficult family decision, family life. My chief residency in medicine changed my life for the better. I mean, I needed eight hours sleep every night, and being chief resident, I was up till three a.m. every other night, and I would have to come home because my wife had fallen into a deep postpartum depression. I had to take care of a one-month-old baby and a seventeen-month-old baby, and when they cried during the night, I was the one would have to take care of them. I'd have to give them breakfast, and then on the next night when I would come home at six, I would have to bathe them and feed them and so forth and so on.

I also had to give my first scientific presentation on the antibody feedback mechanism, and basically I can remember being in the bathroom at four-thirty in the morning practicing my talk and saying I was determined not to be defeated. I just had a great year as a chief resident. I became a very well-trained physician, and at the end of that year I had enormous confidence, the confidence that comes with overcoming adversity.

It was a very adverse year. My wife really never recovered from that depression, and I had to be a full-time father, a half-time mother, a full-time chief resident. When I gave that talk, I can tell you I knew every word to such an extent that I couldn't wait to get up in front. This is my first scientific talk to about three thousand people. It was a transplantation-type meeting. And I literally pretended to search for a word here or there, having it right on my tongue, and I just played it like an orchestra, a symphony conductor. I remember all the comments I got afterwards.

But at the end of that year, I had enormous confidence I didn't have before, even despite all that I had done at college, because you had to do everything if you were Jewish. I was on the track team, I was on this, and so forth, all that stuff. Then the navy, which toughens you up, but nothing like that chief residency.

When it was finished, I felt I could anything, and I wouldn't change anything in my life in terms of that.

Williams: I notice that you are a member of the National Academy of Science.

Uhr: Yes.

Williams: How does being a member of that make you feel?

Uhr: That makes me feel good. I really have not been as ego-involved as most scientists. I mean, after I got in the National Academy, I just put the plaque up somewhere. I didn't put it up; I just put it away until I got into a new office, and then I was told I should put it up, and I do. I'm very proud of my army plaque from the Commission on Immunization, and then I have a couple of others I put up there. But I think if I had not gotten in, I would be significantly disappointed, because I think that I deserve that.

Now, in contrast, when I was thirty-six years old, I got a letter that I won the Newcomb Cleveland Prize for the best paper in the journal of *Science* in all disciplines, had my picture in *Time* magazine, and when I got it, I just laughed. I mean, I've never been cursed or blessed, how you see it, with looking at myself or my children or my grandchildren through rosy glasses. I just look at them and myself rather objectively. And this was ridiculous. I mean, it was a fancy paper. I was immunizing with a bacteriophage and finding some new things, but the best paper in every discipline? It was ridiculous. There must be a hundred that are more important than that. So I don't know why I really got that. They were just overly impressed. I mean, it was a good—it was an A-minus paper again, but an A paper, no. An A-plus-plus-plus paper in every field? That was just silly. So therefore when I say that I think my circulating tumor cells are A-plus, I think I'm being relatively objective.

Williams: Did you write a letter to the editor of *Time* to that effect?

Uhr: Oh, no, not at all. [laughter] I do. Oh, I write letters all the time to people. They frequently are published. I wrote Carl Sagan when he wrote a foreword to a book and said how clear it was, and I chastised him and got an answer from him and so forth. I sent seven letters, airmail letters or something, to Dukakis, demanding an apology for how he had run his campaign. I did that for two weeks, but he never answered. Yes, I write letters all the time.

Williams: I know the question I wanted to ask you when we took a break. You were talking about being the attending physician in New York and what a profound year that was. Did you continue to be attending through your career or not?

Uhr: No. When I came down here, I planned to, but it just wasn't possible. Being the chairman of a basic science department and building it from scratch just was overwhelming, so I couldn't pursue that.

But I can tell you it was quite an experience being on the psychiatric and prison wards back in 1960. There weren't medicines around. You couldn't find any guards around. Everything was barred. It was not like it is nowadays. People threatened me. The first five minutes I was there, I was threatened with death by at least three or four of the inmates who were absolutely hallucinating all over the place. I went to the nurse and I said, "I'm going to be killed here. I mean, I don't see any guards around. These are fairly big guys, and they think they're George Washington, Napoleon, or Hitler, this and that."

She says, "Don't worry. They won't touch you. If they touch a nurse or a doctor, they go right up to the violent wards." Well, the violent ward was horrible. People would be tied together. One would get loose and kill the other. It was horrible.

I said, "Well, they don't know that. They don't even know who they are."

She said, "Oh, yes, down deep, they do."

I was never touched in seven years, and in the prison ward sometimes I would be taking care of someone who shot it out with the police, was on the front page of the *Daily News* the night before. I was never touched once, and it's why I'm a firm believer in if punishment is swift and sure, it's a deterrent, not the way we carry it out in our legal system. It's the one area where I am not a liberal.

Williams: How come you had that post?

Uhr: Just mere chance, that's all.

Williams: You were assigned?

Uhr: Yes.

Williams: You didn't volunteer.

Uhr: Right.

Williams: Specifically you were there to do what?

Uhr: Take care of their medical problems. In New York City then, if some Park Avenue woman would try and, quote, "kill herself" by taking a few pills so her husband would find her and he was foolish enough to call the police, she would

end up in the Bellevue psychiatric ward, where she had to stay for as long as thirty days with her fur coats. It didn't help.

One night, I changed places with the other physician when Norman Mailer was admitted, and that became a big fight. I'm very glad I changed places with him. He had stabbed his wife, and the other physician thought that he should be—he was hurt as well, so that's why he was seen by a medical person. We would change places every other night. He got him into the mental ward, where Mailer protested and so forth. It was a big thing in the newspapers. I'm glad I wasn't part of that.

Williams: You wanted to tell me about your time in medical school.

Uhr: Well, yes. I'd worked very, very hard to get into medical school, as I told you, and I was Phi Beta Kappa and all this and that. So I decided when I got in that I didn't have to work that hard. I'd been working all my life, and I decided I would just become a good clinician. I bought very few textbooks, but I took very careful notes.

My father had an office in New York as well as in New Brunswick, New Jersey, a little one-room office, and I basically stayed there, and I would study my notes carefully from six to eleven p.m. At eleven p.m. I would close the book, if I was in the middle of a sentence, and I went down to Ryan's Bar and I would have one or two drinks. I would walk around the block, and I'd go to sleep by—you know, and get eight hours' sleep. I would very carefully, if there was a test, I wouldn't come till the last minute, because everyone else was studying and changing notes, and I wouldn't do that.

At the end of two years, you could find out what part of the class you were in, which quarter. Well, I was afraid if I found out I was in the third quarter it would depress me. I hoped I was in the second quarter. I wasn't sure. The one time they gave results of an exam is when we first started in the first year, and I failed it. So I wasn't very optimistic. So I'm probably the only person in the history of NYU who didn't find out what quarter they were in.

Well, in the last quarter, I suddenly got some note that I'd won some academic prize. I was way in up in top, in the top 10 percent, so I was an Alpha Omega Alpha, which is their equivalent of Phi Beta Kappa, and therefore I was able to go to Mt. Sinai. But, I mean, I never suffered like all the other medical students. I just studied hard, but had a good time, very relaxed. So I'm rather pleased that I had that attitude, puzzled that I did so well, but who cares?

Williams: Looking ahead, what do you see as the future of immunology? I know that's a big question, but—

Uhr: You know, I haven't kept up with immunology for the last two decades. I'm way behind in terms of that. To keep up with it, by the way, I'd have to be full-time. I mean, there's so much going on there. I have kept up to a certain extent because I was one of the editors of *Advances in Immunology*, which is a rather prestigious collection of reviews, but the reviews have new things in them. So I have a certain familiarity, not in great depth.

One of the things that I think is very exciting is gastrointestinal flora and the role they play in terms of many, many things. I suggested several years ago—I think I resigned just this fall. There was a chief editor, Fred Alt. There were five of us from different—mainly from this country, but not completely. We'd publish four volumes a year. I suggested there be a thematic one on the gastrointestinal flora, and that is just exploding. You've heard about the feces transplants. Who would think that that would change things so well? I think that's a big area.

I think basically in cancer they're going to learn finally how to immunize against cancer cells. It hasn't been that successful, and I think that's going to be a big one. Then in terms of immunity, etc., things happen that are not insignificant. There's a new pneumococcal vaccine, which is, again, seven different strains. Well, you know how many older people die of pneumococci.

So I think that in terms of resistance to infectious agents and cancer immunity, those are going to be big areas, and autoimmunity is all over the place. Incidentally, I ran the first lupus erythematosus clinic, which was at Mt. Sinai Hospital, for seven years with another physician who was at Columbia, and I was at New York University. He was my best friend, so we decided we'd run the clinic and see each other there, and we did that for seven years. We then decided it wasn't that interesting. We just gave cortisone to these poor patients, and we weren't that skillful at it. So we decided to resign and meet at a bar for the next eight years and just have a drink together.

But I think that's going to be a big area. Autoimmunity is all over the place, and maybe we'll find out what we can do in terms of neurologic diseases from the immune standpoint. Maybe we can do something that would help with Alzheimer's and prevent the deposition of that protein.

Williams: What do you tell trainees, young students about their futures? You're talking about the wealth of scientific investigation, but in an age of sequestering and whatnot, where are they to go?

Uhr: I think they have to realize how tenuous it is and how unattractive it is nowadays. I mean, no one can survive definitively on 5 or 4 percent funding from the NIH. I'm not surprised that most of our students and most of new scientists are going to be from the Orient or from India. I mean, they are used to not having much and working very, very hard, particularly as they're first generations, and it shows. It also shows in medical school. Most of our interns and residents now are not

going to be Caucasian males as it used to be. They're going to be from India and mainly from China, and then quite a few Caucasian women. It's just changed completely in terms of the ethnicity and races.

Williams: Taking India as an example, are the strides ahead going to occur in India, or will those people come here to do their work?

Uhr: Well, as of this day, I mean, I get at least three letters a week of people who want to work with me, and they are mainly from India, now mainly from India. They used to be mainly from China. Secondly from China and thirdly or recently, for some reason, from Turkey, etc. Yes, I think they want to come here and stay. This is a better place for them.

Williams: So in that sense America is still ahead of the field.

Uhr: Yes. I certainly would hate to see us not giving Green Cards to all of these youngsters, because we need them desperately, particularly with such a poor public education system. It's just very disheartening. We just have such a poorly educated high school and college-age generation, particularly in math and science, the two things that are so important.

Williams: I've been asking everyone this question at the end, and you've already answered a good part of it, but I'll ask you to sort of restate it. What does a scientist do for fun?

Uhr: For fun? I mean, I've always had a lot of fun. Basically, I'm still playing singles tennis. I'm in very good shape. The physical therapist that my wife just went to, I go to twice a week to muscle-up. For some reason, I like to be not just physically strong, I want to be muscular, and it's emotional, and why should I fight it? So I'm still doing that.

I started tap dancing lessons over a year ago, and I'm just taking a little rest now because it's rather exhausting. I don't have good short-term memory. I have a neuropathy in my lower leg, so my balance isn't 100 percent. But I came from an era where tap dancing was very attractive, so I'm doing that.

I go to the gym twice a week to do weight things, etc. We go to the symphony regularly. I love country music as well as classical music. I just love country music. Took me many years to appreciate it. When I first came down here, I thought it was rather nothing, but I began to see nuances and depth in it that makes me very attracted to that. We went dancing the other night.

We travel a lot, and I'm fascinated by—I have a technician who raised eight orphaned tigers in Texas. Texas, next to India, has the most tigers of any country in the world. Tigers are very popular in Texas. Ranchers have them, and other

people, as pets, and dope dealers in the rural areas just bury their dope in the ground, put a stake, and attach a lion or a tiger to it.

Well, I've investigated this in depth, and, again, if I were young, I might not go after the circulating tumor cells. I would go after brain research in these animals, particularly those raised by humans. Tigers and lions raised by humans, rarely, if ever, rarely, probably never hurt their parents, the human parents. Everyone points to this thing in Las Vegas. I won't go into it. There was an ambiguous situation there, and I don't know where when you have those two—you know the one I'm talking about. I'll leave that out. But by and large, they're like kittens.

So here are these two—I went out with my wife to play with them. They're huge. They're bigger than lions. They're 450-pound Bengal tigers, just kissing me and licking and so forth, and lying around with each other. I've gone into this in some detail, and in contrast to humans and chimpanzees, these wild animals where everyone says, oh, they can always turn on you, it's nonsense. They're just extremely well behaved and so forth.

I'm fascinated by this and I'm trying to get videos together to give talks to high school students, getting them interested in science by showing these huge, wonderful animals hugging and kissing their parents, their human parents, and basically talking about genetics versus environment, nature versus nurture. I haven't been successful. I've gotten no help from anyone in making the videos, but they're incredible.

Not only that, if you take wild animals from birth and you have them nurtured together, they're wonderful with each other, in contrast to human siblings. So, for example, a huge black bear, lion, and tiger in the zoo in South Africa were orphaned in the sense their mothers weren't taking care of them, so they spent several weeks trying to find out where they could take them. So they shipped them together. Well, they arrived at this reserve. They were doing so well together, they kept them together.

It's now three years later. The black bear is 1,000 pounds, the lion and tiger are 350. It shows them when they get up. The lion sleeps longer, so the black bear gets up with the tiger and puts its arm around the tiger, kisses it. They go around and they play together. The lion then joins them, and they play and they play and eat and sleep together. They're unaware of the fact that these differences mean anything, and that's not true of humans. So that's what I would like to study, and I, again, follow this very eagerly.

So I've always had a lot of fun. You can work hard and still have fun if you're adventuresome, and I've always been adventuresome. When I got out of the navy, I worked as a traveling salesman each summer, door to door, to see the country first before I went abroad, and I had a wonderful time going out on one

route one year and the next route another year, and I saw all the national parks and met people from every state, and I had a good time.

Williams: What were you selling?

Uhr: I sold folding chairs at first, most successful in Iowa where I had relatives, and the second year we sold feathers for women's hats. The first year, I slept outside every night unless it rained. We would sleep and we'd wake up with the cattle browsing near us, wondering who we were. The second year, I had a tent. We had a tent, and we used that at times, but mainly slept in the car.

Williams: You had a partner?

Uhr: Yes. My partner the first year, I thought he was going to end up in prison. He was a friend of mine from Sunday School, and he was a real nerd. He was very fat and interested in becoming a geologist and so forth. But he'd been in the war and, in contrast to me, he'd been shot a couple times and he'd gotten real tough, and he carried a gun all the time. He just would pull the gun out. This was back in—you know. Wow, this is like sixty-five years ago. And we ended up on very bad terms.

I opened *The New York Times* once in New York, and he'd become a professor at Caltech and was in the NASA program big time as an expert in dating radiocarbon and other kinds of dating. I wrote to him and said, "I thought you'd be in jail at this point in time."

"No, no," he said, "I'm now a professor. I have a wife, I have children," and so forth. So I went out to see him, and he'd calmed down somewhat, but not entirely.

Williams: Have we left anything out unsaid here? We've covered a lot of ground.

Uhr: No, I don't think so.

Williams: Good. Thank you very much.

[End of interview]