The American Association of Immunologists
Oral History Project

Transcript

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Williams: This is an interview with Dr. Paul W. Kincade for the American Association of Immunologists Oral History Project. Dr. Kincade is Vice President of Research, member of the Immunobiology and Cancer Research Program, and the William H. and Rita Bell Chair in Biomedical Research at the Oklahoma Medical Research Foundation (OMRF). He's also the Scientific Director of the Oklahoma Center for Adult Stem Cell Research and an adjunct professor in the Department of Microbiology and Immunology at the University of Oklahoma Health Sciences Center. Dr. Kincade was President of the American Association of Immunologists from 2002 to '03 and served on the AAI Council from 1997 to 2003. He was awarded the AAI Distinguished Service Award in 1998. We are at IMMUNOLOGY 2014™, in Pittsburgh, PA. Today is Saturday, May 3, 2014, and I'm Brien Williams.

Thank you for doing this, Dr. Kincade.

Kincade: My pleasure.

Williams: Let's start with a little bit of family background.

Kincade: Okay. I grew up in a very small town in Mississippi, a place where you could see cotton fields for miles and miles, where there are no mountains and no oceans. It was a very small town with, including the junior college that was across the street from my house, there were about 1,500 people, so very small town, very conservative.

My mother was a music teacher. She had a college degree and taught piano. My father did not have a college degree, but was a pharmacist, managed to get his pharmacy license by being an apprentice and taking a state exam. So he managed to buy the drugstore that he clerked in from the retiring person, a very smart individual, and ran the drugstore. And he always wanted to have a farm, so he bought a little broke-down farm out of town and had that as well. So we had a drugstore and a farm. So that's our background.

Williams: When did the Kincades arrive in Moorhead?

Kincade: I'm not sure. My father arrived there to be a clerk in this drugstore, but I'm not sure of the date. We don't know very much about his family. His parents had passed on before I was born, so I never met his parents. I think they were reasonably poor. I know they were in a poorhouse at one time. And don't know a whole lot more about them, except that they were Irish on that side of the family.

On the other side, my maternal grandparents I knew. My grandfather was the last telegraph operator for the Illinois Central Railroad, so he sat there all day operating a hand telegraph until he was the last one they replaced with more modern communications.

So that's my background.

Williams: Where did your parents meet?

Kincade: They met in this small town. My mother got a teaching job, when she graduated from college, in this small town. She got the job as the music teacher, the piano teacher, and that went well until she met my father and they got married. When I say it didn't go well, they had then a rule that teachers couldn't be married, certainly not pregnant, but not married. So she had to give up her job until she had my brother and I, and then they changed the rules and allowed her to go back to work. They managed on his salary of fifty dollars a month when they were married.

Williams: So what was it like as a young person growing up in Moorhead?

Kincade: Well, it's a small environment, very conservative, different from most of the people you will have talked to about their environment. There were good things about it, rich things about it, things to explore. Horseback riding, for example, is something you could do. I could do things on the farm and I did things in the drugstore, eventually worked in the drugstore, which was important for me. My brother liked the farm and I liked the drugstore, and that was important because I had quite a bit of experience working in the drugstore when my father died. May the 18th, 1960 my father died, and I was fifteen. But I had worked in the drugstore and I operated the drugstore for a time, trying to get someone to buy it, which eventually did happen. We managed to sell the drugstore.

But that was an important event in my life because, among other things, I became an adult at the age fifteen. "Emancipated" is what the legal term for it is. My minority was removed, and so I had all legal rights of an adult at age fifteen, went into a courtroom and the judge pronounced me an adult. That was because my father didn't leave a will and we needed to probate the estate.

So suddenly I was an adult, and I considered myself adult in terms of responsibilities and also freedom to do pretty much what I wanted to do. So in high school, tried a little sports, but that wasn't the main thing, taught myself some amateur radio things and built radios, and had always wanted to go away to a proper school. It wasn't a one-room schoolhouse, but it was close to it. There were only twenty-five in my high school graduating class, and our classes were taught by football coaches for the most part, some of them really terrific people but not particularly good teachers, and my background was pretty restricted.

And I might add that I was a poor student. My report cards said that I daydreamed, which was true. I had a hard time focusing because everybody in the class was on the same page, on the same line at the same time, and it was lethally boring. Before my dad died, in fact, not long before my dad died, I asked to be sent away to school. In Mississippi, if you went to a prep school, it was

military school, and I wanted to go to military school. I thought I could get a better education, it would be interesting, maybe I'd get some discipline, which was probably needed as well. But we couldn't afford it, was the bottom line, so that was out for me.

So my education was missing a few things, I would say, relative to other people who went to college, and finances meant that my only option was to go to a state university, where I was admitted deficient in math and probably some other things, academically not prepared for college and, again, not a good student.

Williams: What career path did your brother take?

Kincade: My brother liked the farm, as I said, eventually decided to go back to school himself after he finished, and he got a Ph.D. in entomology and ran an agricultural experiment station testing pesticides and herbicides, things like that, his whole career.

Williams: So science was sort of in the air for both of you.

Kincade: Sort of. I really didn't know what science was. In small-town Mississippi, the only scientist is the local doctor, and so I was interested in that, but I really wasn't cut out to be a physician, didn't have the personality and discipline to do that. So I really didn't know much about what a scientist was. In fact, I was well into college, probably a junior in college, before I first met an investigator and really understood was science was.

All the beginning courses in biology, the survey courses that freshmen take, were pretty boring. I had a parasitology course that was quite interesting, and we went on field expeditions to collect worms at slaughterhouses and in the ground, and that was quite an interesting thing, one of the first interesting things.

But really had no idea what science was about, no idea about what investigation was like until probably I was a junior in college and I had the great fortune to take a class under Bruce Glick. Bruce Glick you will not know. In fact, a lot of immunologists now don't know him. But he was the first person I ever met who was—he was two things. He was a master teacher, and so suddenly I had a really captivating person to listen to, who did all kinds of innovations in teaching, and the second thing was he had made discoveries. He had, as part of a Ph.D. project, discovered that if you remove a small gland called the bursa of Fabricius from chickens, that those chickens are defective in making antibodies, which to me was just the most exciting thing in the world because it was serendipitous. I liked the sound of that because it seemed to me that immunology was different from any other discipline and that anybody could contribute if you tried hard and you paid attention, you had observational skills. Anybody could discover something pretty exciting. And his description of that primitive discovery was just so amazing to me. He was the first real experimentalist, somebody who asked questions. He

always said that doing research made you a better teacher, and vice versa, teaching made you a better researcher, and he's absolutely correct about that.

So his laboratory in immunology, we went into his research laboratory and we used chickens because he was in a poultry science department, and we did physiology experiments, and we immunized chickens and we measured antibodies and things in a real working lab. It was so much different from any other lab I'd ever been involved in because those people were not—I guess they were teachers, but they were pretty boring and they certainly weren't discovering something. They were just teaching you things to memorize. But this was different. This was exciting. The field was exciting. The teacher was exciting. So it was a real pivotal thing for me.

Williams: How do you figure teaching makes for a better researcher?

Kincade: I think it draws out questions from trainees, for one thing. When those hands fly up in the air, you're going to get something interesting. Somebody's going to make you think about things just a little different. You're going to get outside your immediate field, so it's going to broaden your scope a little bit. Teaching makes you think about what's new in a different way than from writing a grant. It's different, and I think it enriches in both directions.

Williams: When you came into Glick's lab, had he already made the bursa discovery?

Kincade: Yes, he had. He, in fact, made that when he was a graduate student, but he made other discoveries. He was interested in everything. He was interested in various endocrine aspects of bird life, and actually made another discovery which I was to return to many years later, was that if you dipped a fertilized egg in a testosterone solution, that that would also have the effect of bursectomizing the chicken. It would cause involution of that organ, and thence they wouldn't be able to make antibodies.

So he explored a lot of things, was interested in the physiology of how a bird lays an egg, what are the endocrine and metabolic steps involved in that, mobilizing calcium and depositing it in an egg, things like that. He was the first real scientist that I met that had a real inquisitive mind and would ask questions and seek answers for them. It was amazing.

Williams: So did he actually actively steer you in the direction of science?

Kincade: I think by example. I think by example. We probably didn't discuss it in those terms. He wasn't really a career advisor for me there. But an opportunity presented itself as I was about to graduate college, and again I remind you I wasn't a great student and I didn't have crisp career goals in mind as I was approaching graduation from college. An opportunity opened in a vet science program for a graduate student to do a master's, so two-years' program was to be

had, and I could work in a lab and have my own project. And I jumped at it. So this was my first opportunity to actually do my own experiments, my own projects.

My mentor for that halfway through my program got a job in a big pharmaceutical company and left, so I was left to complete my project on my own. So it again gave me an opportunity to be very independent and invent things. When I arrived in the lab, there were boxes with germ-free isolators in the boxes purchased, but nobody knew how to put them together or how to use them, and I assembled them and taught myself how to raise chickens germ-free and tested the precise pathological processes associated with mycoplasma, *Mycoplasma gallisepticum*, which was a big pathogen for poultry, but it was always complicated with secondary infections. So I had an opportunity to study this pathogen in germ-free chickens where there was no other organism, and had great fun doing it. It was just like playing around, and it was a good experience for me.

Williams: Did you have lab assistants, or were you pretty much on your own?

Kincade: I had one. There was one lady, a wonderful hyperthyroid lady, who was just wonderful. Her background was med tech. She had been a med tech. And that was it. There was one other guy who came in the lab for a time. He was a more experienced guy that came back into the lab for some reason and who was studying a different organism in cows, quite a colorful character. But that was about it. It was just a small group.

Williams: And did you publish?

Kincade: I can't remember if I even ever published anything from that, because my mentor left. I got my master's degree, and I moved on, and I submitted the papers myself with no help from anybody else, and they came back. I found out later they were actually accepted. I sent in two papers, but I was horrified when I saw the writing all over with red ink written on it. I had a serious problem throughout school, writing. This was a big problem for me in college. And I just put that reviewed paper in a drawer, and I don't think I ever published any of it.

But when I was in college, I wrote my mother a letter at least once a week from college, and my mother kept all of my letters in a shoebox. So when she died and we were cleaning out her things, I found this shoebox full of my letters, which was kind of like a diary or something, telling her what was going on every week. One of these letters, must have been a freshman in freshman English, but I said to my mother, "I'm not sure what I'm going to do for a living, but it's not going to involve writing," because I was tremendously frustrated. They didn't teach writing. They scored your papers, and gave me a "C" or something on the papers, but they didn't teach writing, and I had never been around writing before. The very thought that I could ever write was just out of reach. Of course, that's all we do now is write.

Williams: So you got your master's and then you went on to Notre Dame, is that correct?

Kincade: I went to Notre Dame to be a technician, and that was influenced by the Vietnam War, because I wanted to be deferred from that. So my small-town draft board, one advantage of knowing the people on that was that I explained the job opportunity that I had, which was to go to—Notre Dame had been the birthplace of germ-free technology, the so-called gnotobiology technology. It's where all the methods were developed for raising an animal in a germ-free condition. So that fit well with what I had done, and it seemed like a really good opportunity, and it was medical research related.

I explained this to my draft board, and they said, "Okay, you can take that job." So this was terrific for me. So I had a job doing an extension of what I did. It would be the first time to be out of Mississippi. I must tell you that I'd never seen the ocean or mountains until I graduated from high school, and so everything was new and cosmopolitan and interesting to me. Even South Bend, Indiana, was a step up.

But I discovered I had to teach myself to do things at Notre Dame, because they didn't know how to do the projects that we were going to do, which involved making antibodies and purifying them and detecting proteins and immunoassays, and they had nobody who knew how to do that, and I didn't either, but I taught myself to do it, which were skills that helped me for years to come. So I spent a year doing that at Notre Dame, discovered that I was not a good technician because I didn't like to be part of somebody else's project. I liked to do my own thing. I liked to be independent.

So before the year was out, I was looking for something else, some other opportunity, and that is a story in itself. When I was doing my master's project, which I didn't publish, I discovered that when I gave germ-free chickens mycoplasma infections, that they would make antibodies to their own proteins; they would make autoantibodies. Their red blood cells became what's called Coombs positive. They became coated with their own antibodies. So I thought that was interesting. And their joints swelled up with inflammation, so they basically had arthritic conditions. So I thought, "This would be a good project to continue." I thought it was interesting, and, of course, I didn't work on that at Notre Dame; I worked on germ-free mice.

But I was intrigued about the little things that I'd managed to find by myself, so I decided, "I'm not such a great technician. I need a Ph.D. to really get some freedom in this business. That's clear. So I've got to find someplace that will let me take my project with me to their lab." So I called up Bruce Glick and asked him about it and talked to him, and he was advising my career at that time. I said, "I want to go to somebody's lab that knows how to work with chickens and who will give me some freedom to do what I want to do."

He said, “Well, you could go to the University of Hawaii,” where there was a very good immunogeneticist who worked with chickens. I met him some years after that, and he’s a very impressive guy. Would have been a rigorous academic environment, but there would have been the problem of getting to Hawaii with a wife and small child, and we had no money.

So the other possibility was a guy named Max Cooper, who had just moved to the University of Alabama. Bruce said, “He’s fantastic, and, by the way, he’s extending studies that I discovered with the bursa of Fabricius, and he’s a terrific guy. I don’t think he has anybody in his lab yet. You might give him a try.”

So I couldn’t get my courage screwed up to write a letter. I didn’t trust my writing anyway, so I called him at home. The only telephone number I had was his home telephone number that I could get from the directory, and it was Thanksgiving Day. Timing; I knew he would be at home. But in retrospect, this was crazy, disturbing somebody on Thanksgiving Day at home to say, “Hey, Dr. Cooper, you don’t know me, but I’d like to come work with you, and by the way, I have my own project.”

So he listened politely, and I thought, “He’s going to hang up any minute now,” and should have. It’s just ridiculous. People don’t recruit people to their labs with their own projects. You go to a lab and you take the project that you have a grant for. That’s how it’s done.

But Max said, “Why don’t you get on a plane and come down here. We’ll talk about it.” And I did, and he said to me that I could work on my project, “But I don’t have any money for that, and I really don’t have any money to pay for your Ph.D. either right now. Maybe we’ll get it later, but we don’t have it right now. I have enough for maybe six months, but you’re going to need some kind of grant support for me to let you come here.”

And I said, “How do you do that?”

He said, “Well, you have to apply, and the NIH has some things available. You could try that.”

So I said, “When’s the deadline?”

And he asked somebody to check on it, and it was the following week. So he says, “Okay,” and he got the forms, and he said, “This is what you do. You write an application. You write a proposal for a predoctoral fellowship.”

“So, okay. So I’ll go home and I’ll do that.”

He said, "No, you don't go home. You go back to the hotel and write your fellowship." So I did in a day or two. It had to be pretty crude. Called up people asking them to write hurried letters of recommendation for me; Bruce Glick was one of them.

So I went to Birmingham with only the security I had was that maybe I'd have a position for six months, and got the fellowship, fortunately, so things worked out really well.

Williams: And you were there for, I think, only a couple of years.

Kincade: It only took a total of five years to get my Ph.D., and I stayed on a little time doing postdoctoral work. We had a lot of success, we had a lot of papers to finish up, and I was just in a perfect situation where I had, again, a lot of independence. As my program progressed, I had my own technicians in Max's lab. I had a tremendous amount of freedom to do things. We would argue like crazy about what should be done, but at the end of the day, I made all the decisions about whether to do this experiment or that experiment, and he let me have that freedom. So it was a wonderful place to be, wonderful time.

So I took my time finding a postdoctoral position, narrowed it down to two places. One was on the NIH campus. I went there and interviewed and looked at that, and it would have been a good opportunity, but a better opportunity presented itself when two Australian scientists came through and did seminars, and I was intrigued with this. One of the medical students that had worked with Max went to this institute [Walter and Eliza Hall Institute of Medical Research] in Australia and worked and came back, telling me about it, and that did it. I had to go to Australia. It was an amazing institute, a very famous little place with a small budget. It had been headed by one of the Nobel laureates in immunology. Sir Macfarlane Burnet was the first director of that institute, followed by Sir Gustav Nossal. It was just an amazing place to be.

Of course, the problem was I had no money to get there, of course, so I applied for every fellowship there was, got turned down on all of the ones, except for one. And I still remember I was at home looking out the front room, just looking out at the street when the mailman came and brought me a letter that said that I got my fellowship from the Arthritis Foundation, a postdoctoral fellowship. So that was it.

We had a small house that we had a very small amount of equity in, sold that, bought one-way plane tickets to Australia. It was just fantastic. And you can imagine the step up each time in sophistication of the cities I lived in. Birmingham was very cosmopolitan relative to South Bend, Indiana, and Melbourne, Australia was just amazing. It was multinational, it was interesting, a lot of fresh things.

The institute was a very exciting one, and it just exactly suited my disposition. The fellows that went there had to get their own money. They didn't have any salary money for us postdoctoral fellows, but postdoctoral fellows and sometimes people on sabbatical came from all over the world to go there, and every six months or so, there'd be new people coming in and people leaving. They had a small core of really terrific investigators that were sort of like permanent faculty, but a lot of turnover of people. And all of them had to bring their own money, so the Germans had money from the Volkswagen Foundation, and people had different kinds of awards from the United States to allow them to go there and study, and mine was the Arthritis Foundation, as I mentioned.

Williams: So did that obligate you to work on arthritis-related issues?

Kincade: Not at all. I can tell you later about my work I did as a graduate student with Max. But it set a path for me to learn more about how cells of the immune system were made. They had a guy in the Walter and Eliza Hall Institute who assumed that I would come there and work with him on chickens, because he had a chicken project, and I said no. I wanted to start working with mammals, and I wanted to learn how to do experiments with mice, and I wanted to learn procedures for studying stem and progenitor cells. Don Metcalf headed a unit that did that. He went on to win the Lasker Award, which is the highest prize you can get, short of a Nobel Prize.

So I had an opportunity to work in that program, but with, again, a lot of freedom. People could pretty much do what they wanted to do. They gave you very, very little lab space that was yours. It was usually shared with someone else. You worked in very crowded circumstances in a place with no air-conditioning, where we used open burners for sterile technique and where two or three people would be working in a little tissue-culture room, side by side, reaching over each other to get pipettes. It was just fabulous, a wonderful experience at the Walter and Eliza Hall Institute.

Williams: So you went through an application process to go there?

Kincade: I wrote them and got accepted to go there, but they said, of course, "You have to bring your own money. We have no travel money. We have no salary money, no stipends. We provide supplies and things for you to work with when you get here. It's limited, but we have some." And I found out it was very limited. They had lots of mice, hundreds and hundreds and hundreds of mice. I could use four hundred mice in a week for my experiments. But if I wanted to buy some instrument or something like that, that was another thing entirely.

But, yes, so I knew I would be accepted there or I could go to the NIH campus and work in a lab there. I knew I had a place to go. But there was just no comparison, to me, because this was an extension of what I was really passionate about learning and just sounded like a great place, and it was.

Williams: Be precise about what you were looking for and passionate about learning.

Kincade: I was passionate about extending the work that I did with Max Cooper, which was an extension of the work that I basically heard Bruce Glick talk about so many years before.

Just to explain that, if you'd like now, I can tell you what that was about. So I told you that Bruce Glick removed an organ and selectively—that's an important word, "selectively"—inhibited the ability of the chickens to make antibodies. So Max Cooper built his career on extending that observation by showing there were other kinds of lymphocytes. So we knew there was something supported by the bursa, but Max determined that the cells that are involved in, for example, skin graft rejection, what we now call T lymphocytes, are made in the thymus, and Max was able to independently remove one source of cells or another source of cells and show that there were at least two highly specialized components, cellular components of the immune system.

So I had the privilege of working on one of those, which was what we now call B cells, bursa-derived or bone marrow-derived, whether you're talking about chickens or mammals. And my project was about learning how the immunoglobulins are first expressed by these cells. Cells that we call B cells are professional producers of antibodies. They give rise to plasma cells, and they can make a thousand antibody molecules per second.

They come from B cells, and my questions have been where do B cells come from and how do they get assembled, how do they get made? Because if you don't have them, you have immune deficiency disease, for example. So I had enough knowledge of purifying proteins and making antibodies that I got when I was working pretty much by myself at Notre Dame to start right away to make good reagents for detecting the immunoglobulins in chickens. And I made these, by the way, with the understanding that I was going to use them to study mycoplasma infection in these chickens. So Max carried me along and said I could do my studies, and my studies began with making all these nice reagents, hooking them up with tags so I could look at them in fluorescent microscopes and so forth.

Then little by little, I lost interest in the mycoplasma project and went with Max's core interest, which is how does the immune system develop, not how does it work and not how does it relate to arthritis in chickens, but where does the immune system come from, how is it assembled, what kind of assembly line is there for making these vital cells that we need.

So I had the reagents made, and Max gently nudged me in the right direction, never said I had to do anything, but excited me about his ideas. Since I was the first and worst student he had, we spent a lot of time together. We worked side by side sometimes till four o'clock in the morning. It was just fabulous.

So the project was learning about how these immunoglobulin molecules first get switched on, and we knew there were different kinds. There's one that protects the mucosal system, for example, that's IgA, and there's one called IgM, which is a bigger molecule than the others, and a principal one in immune responses called IgG. Well, we didn't even know that the chickens had those particular molecules, but I was able to separately identify that there were at least three kinds, had specific antibodies for them, and so I could track them, and I could see that the IgM came up first, and then the IgG and then the IgA.

Then we discovered that if I took fertilized eggs and learned how to intravenously inject the embryos in fertilized eggs, I could deposit antibodies to IgM in those embryos, and then when they hatched out, they would produce no B cells, not IgG or not IgA. So it was the first evidence that what's called immunoglobulin isotype switching occurs during development in a programmed way, and this event happens before this event, that event, which is pretty exciting.

We were able to make a lot of related discoveries pretty easily once we had these reagents. Ben Pernis describes spots of immunoglobulin on some lymphocytes. By looking with frozen microscope, he saw these little spots of immunoglobulin on the surface of some lymphocytes, and in an afternoon we found that the chickens that were bursectomized that had no B cells had none of those cells with spots on them, so the first demonstration that the B arm of the immune system displayed immunoglobulin on the surface that you could see. It was the first discovery of that. Very quickly, a number of people found it other ways, and eventually the T cell receptor was described. At that time, we had no idea what it was. We just knew that it wasn't an immunoglobulin.

Williams: So you're describing work that you did in Birmingham?

Kincade: I did that in Birmingham, yes, which led naturally to questions about, well, "Where do the B cells come from?" Here's the sequence of development of immunoglobulin-producing cells. What comes before that? So I went to a program where people were studying hematopoietic cells. These are blood-forming cells, including hematopoietic stem cells, which we knew existed, but we had no way to see, no way to purify, we just knew that they had to be there. So I learned a lot of techniques for handling mice, for doing procedures with mice, and for doing procedures with these cells. I did some crude cell separations with cells, using a big glass apparatus for settling cells according to their size and density. We did separations like that. We had no cell sorters, of course. They hadn't been invented by that time.

Williams: You mentioned the Arthritis Foundation. They got you to Australia but they also supported your work there, is that right?

Kincade: No, they didn't support my work. They gave me a stipend, and the stipend didn't change when I got to Australia. It was the same stipend I would have had here, except the second week I was there, the Australian dollar was revalued relative to the U.S. dollar by 30 percent. So if not for my then-wife getting a job right away teaching in a teachers college, we would have been in serious trouble, because they weren't going to give me more money in U.S. dollars because the Australian dollar changed. But it was a 30 percent change with no notice.

Williams: Let's move on to some of the other sites, then. You come back to Australia after two years, right? And was that you were limited to that term, or did you choose to return to the States?

Kincade: Well, I had always planned to go back to Birmingham, and something like an instructor position was waiting for me. So with Max, it was always the understanding I was going to go to Australia for a couple years and then come back to Birmingham and start my career there, eventually having an independent lab. That was the plan.

I was really having a great time as a postdoc. My graduate experience with Max Cooper was as good as it could be, but my postdoctoral experience was as good as it could be, and so I was having a wonderful time, just great, and complete freedom to do what I wanted to do. So I was under no particular deadlines, except I started writing grant proposals to sustain me when I went back to Birmingham.

My wife decided she wanted to get an advanced degree in teaching and the most famous teachers college in the world is Columbia Teachers College. So she wanted to go to New York. A person that I was working with in Australia, Malcolm Moore, got a position in New York at Sloan-Kettering, and the then-director of Sloan-Kettering [Robert A. Good] had been Max Cooper's mentor. So he was kind of like a scientific grandfather to me. So it was just automatic, if I wanted a job at Sloan-Kettering, I could have one. And I was given a job, given a lab, and it was very easy to decide to go to New York, but it was going to be for a couple of years while my wife got her master's degree. Then, of course, she went on to get a Ph.D., so we were there eight years, as it turned out.

Worked out well for me and probably, career-wise, was an important step to not go back to Birmingham, to not go back to doing the same things. I was in a very different environment. Relative to today, it was an easy thing to do. I wrote four grants, all four were funded the first time, and I negotiated to keep a half of one of those, because there were two grants, duplicated. I've always just, for unbelievable reasons, had complete success with getting grants.

So I had my grants, I had my lab, and could do what I wanted to do now. Didn't have very good plans for my research. Nowadays, my grants would be so laughed out of there. I had no good hypotheses about what I was trying to investigate, just general ideas about what I wanted to do and what questions I wanted to ask.

But it was a good experience. We had a nice lab. It was kind of an adjunct to Sloan-Kettering's downtown operation. It was in Rye, on a beautiful site near the Long Island Sound, and I lived one mile away from the lab, so it was very pleasant to walk to work. I could go into the city any time I wanted to visit with labs there and go to seminars. We had a nice group of people, had my first postdoctoral fellow who came there and my first graduate student came there, and it was really a good career move for me.

I got a Career Development Award, which was nice, and from the Arthritis Foundation I got an Advanced Career Award that following my postdoctoral fellowship, which was really a very good thing. It gave me solid security.

Williams: Then what motivated you moving from there to Oklahoma?

Kincade: Well, it was certainly not something I planned. I'd started looking at a few jobs that came around. I think I only ever applied for one job in my life, and I didn't know how to apply for a job. But I saw an announcement that there was a job in Baltimore, and I wrote them, and they said they had interest in me. So my next letter was, to save a lot of time, to just outline all the things I wanted, a terrible thing to do. I had no idea, and I completely put them off, because I made a long list of I have to have this and I have to have that, because I really did have a lot at Sloan-Kettering. I wasn't sure how much of that I would be able to move. So that did that.

And the next thing was I interviewed for a job at Scripps in California and one or two other things around that just came up. Someone would ask me to take a look at this, take a look at that, and none of those really looked that good relative to where I was. The then-president of Sloan-Kettering called me up and said he was moving to Oklahoma and said, "I want you to come and look at it. Whatever you're making now, we'll double your salary. We'll give you whatever you want to move to Oklahoma." I don't even know this place. I never heard of it. Nobody in my lab would want to go there. It would mean starting all over. He said, "Just ask everybody in your lab to come with you and visit." The three people who were most valuable in my lab liked Oklahoma City and decided on that first visit that they wanted to move there, for three different reasons.

It turned out that the director, the president of OMRF, Oklahoma Medical Research Foundation, [William V. Thurman] had been chief resident for Max Cooper, so he knew all about me through Max. And it was an amazing interview that I had with him. It's the first time—I'd talked to some deans and people like that about jobs, where you feel like you're being interrogated, but with this guy, I liked him instantly, trusted him instantly, and he said, "We want you to come here, and we'll give you what you need. Just tell us what you need. And if we can't get it for you, we'll tell you." But it was really just carte blanche. "We want you to be successful. We want you to come here."

The core of my lab wanted to move there, and we did. The second week we were there, we had cultures in the incubator growing. I think Sloan-Kettering might have tried to get rid of me, or wanted to get rid of me, because they said I could take everything except a typewriter. All of my equipment, my laminar flow hoods and microscopes and everything I just took, and cells and biologicals. There was no signing away your rights to your discoveries. So cell lines that we'd made and antibodies and things that we'd purified, we just took everything, and it was painless. And I've never been sorry.

Williams: What was the impact on your wife's career with this move?

Kincade: Difficult, but it was difficult to get a job in New York too. So with someone with a Ph.D. in education and two master's degrees, you might think there'd be a lot of job prospects, but there weren't. At that time, this was '82, New York was broke, Boston was broke. A thousand people a week were moving into the Oklahoma, Texas, Louisiana area. The oil boom was on. There was a lot of money there. You could go to a dinner party in Oklahoma and ask who's from Oklahoma, and maybe nobody would put their hand up. Just it was a lot of people moving in.

Financially, New York was difficult. We couldn't afford a second car, we couldn't afford much furniture, and we had credit card debt in New York, and we certainly didn't have that in Oklahoma. She managed to get a job, but—

Williams: Did you have kids?

Kincade: We had one daughter, who had two years left to finishing high school, and it was probably difficult for her, particularly difficult for her, but we got her into an expensive private school that's like a small college—it's a very, very good school—that gave her poor career advice about where to go to college. That's a long story. But in the end, she did well. She's a full professor, tenured professor at Indiana State and teaches English Lit. She's done well. She's published a lot. So she wasn't permanently damaged by her move in high school.

Williams: I guess there is some writing skill in your gene pool there.

Kincade: Well, maybe. Mine had to be pulled out and nurtured and helped a lot, and Max Cooper was the first to do that for me. When we first started working together, I would write something and put it on his desk, and he wouldn't get back to me for a week or two. He would put off dealing with me because it was so bad.

Finally, he came upon the idea—I think it might have been suggested by one of his co-workers—to write with me. So he invited me over to his house, a wonderful house, beautiful Tudor house. Got used to going over there a lot at night just to talk. But we'd go over there, and he'd get a yellow pad and put it on his kitchen table with a stack of sharpened pencils, and we would write together.

We would talk about how do you write the first sentence of the results. What should it say? What should it be about? Okay, here's the data. This will make a story, but what kind of story? How's the story going to begin? It's not the order in which you did the experiments. You've got to tell the story, and how can you lay these results out and tell that story? And so he would teach me the art of writing a paper. His skills in writing also were so different from anything I imagined that science writing would be like. I thought you had to use third-person passive voice, and you had to sound official and sound sophisticated, but, in fact, what you need to do is be clear. Simple declarative statements will get you a long ways. I learned that from Max.

So it's a technique, a teaching technique that I've used throughout my career. Of course, in recent years what it amounts to is we have a big computer screen, and a fellow comes into my office with their data and maybe they have a draft, maybe they've written part of the introduction or just part of the results section, and then we write together. We talk about what's a good first sentence. How do we get into this? "Well, that's a little strong. Take that out. That little turn of phrase changes everything."

I hope they've learned as much as I've learned about it, about writing from that, but it's different from you writing something when I was in college and it got graded and given back to me with hardly any comments. When my daughter teaches writing in her university, they go through iterations, and she teaches them how to construct sentences and what's wrong with it and what you should do. It's an iterative process that you learn, and I think if somebody like me can learn to write, anybody can.

Williams: So you've spent thirty-two years, about, in Oklahoma, up to here.

Kincade: That's it.

Williams: What major structural changes happened there in terms of departments and in terms of emphasis on scientific research?

Kincade: Well, it changed a lot. Science changed a lot and my institution changed a lot with it over the years. We had a very famous group, research group there that had studied lipoproteins, apolipoprotiens, and identified them all and separated them all and named them all. There was about sixty people in that group. They were very famous. They had a kind of European-style structure to the lab, where there was one real big professor and then everybody else worked on program grants.

We had some investigators that were famous for other things, but not immunology. This was new. So at the same time that I went to Oklahoma, Moe Reichlin went there, who is a very famous, very, very good investigator, physician scientist, who studied autoimmune diseases. He was an expert in lupus and rheumatoid arthritis and had developed a lot of serological methods for detecting

autoantibodies. So we went at the same time, so he was building a program that specialized in autoimmunity, and that program is, by the way, one of our most successful programs now. It's our largest and most successful in many ways. We have a number of other very successful programs, six in total.

But we've seen a lot of changes in our physical plant. I never really thought that it mattered too much whether the air-conditioning worked or whether you were crowded into a space. What really mattered was the people in the space. But when it comes to recruiting somebody to Oklahoma, it helps a whole lot when you can show them brand-spanking new lab space with the latest greatest technology and lab design, and we have that now. So it never mattered to me very much about whether the benches could be rearranged easily on a short notice, because I didn't care that much about that. But we have really evolved our infrastructure over the years, and our science has evolved over the years, have a lot of good people there.

Williams: Talk about the William and Rita Bell Endowed Chair. What is that about?

Kincade: Well, William Bell was one of these amazing people. In Oklahoma you have a lot of people who get interested in medical research who happen to be successful. He was successful and he was also head of a big trust, which, when it was first transferred to OMRF, I think it was $100 million, which was big money back then. It's used to support all of our administrative activities, so that when people donate money to our institute, it all goes to research. We can tell them it's not going to be spent on administration. It's not overhead. That's covered. So he was a very impressive guy, a lawyer who was himself successful, but was the, I guess, senior trustee or whatever it is for this big trust, which became attached to my institution.

We have wonderful people in Oklahoma that are different from the people who donate to Sloan-Kettering, I can tell you. When I went to fundraisers at Sloan-Kettering, there were people who wanted to be there with the Rockefellers, and it was a social event, and they were interested in getting Uncle Harry the best prostate surgeon at Sloan-Kettering and things like that. But they weren't terribly interested in what you did, especially if you do, as scientists generally do, start talking about the nitty-gritty of B cells and stem cells and things like that.

So in Oklahoma, what happens right from the start and continues today that if somebody asks you what you do, they really care, and there's a good chance they're pretty smart and they can understand it, and so it's a whole different dialogue. They regard the scientists as their local team. Like we have a very successful NBA basketball team now, but we're a successful scientific team, and people take great pride in it and a lot of interest in what actually you're trying to do.

Williams: What does it mean to be now Vice President of Research at the foundation?

Kincade: Well, it's a different role to play than being investigator, of course. Scientists—I don't know how to put this—don't necessarily hold administrators in the highest regard. You tend to respect experimentalists, people like you who are doing the same kind of things, who are writing grants like you, and you may or may not have the highest regard for people who send you memos about what kind of procedures you have to follow to get rid of your radioactive waste or how you handle your animals or how you report this. Bureaucracy is not something you're fond of.

But there are administrators who are absolutely required to keep an institution running topnotch, and we have excellent supporting administrators of all kinds, from human resource to purchasing, you name it. We have excellent people. But the Vice President of Research job, which I kind of wrote my own job description for, is about trying to make it easier for scientists to do what they need to do, give them support that they need, make sure they have the space and the equipment that they need, and making sure that their work is held to high standards.

So we've instituted a rigorous annual performance review for all of the scientists, coupled that to an award system and a reward system for channeling institutional resources to somebody who earns it. Someone might be in trouble, may have lost their grant or had trouble getting started with grants, and I have some resources, I can give them money for some period of time. And I just love it when it works out, when somebody takes fire and gets an NIH grant, and we can worry about somebody else. So that's what the job is.

Williams: So does that take you entirely away from working with postdocs and whatnot?

Kincade: Yes, and I made the deliberate decision to do that, to phase that out. Started making that decision maybe five years ago. I first started looking for a replacement to head the program that I started thirty years ago, and it took five years to find somebody I really was satisfied with, and he's fantastic. He's been on the job for most of a year now, and he's just fantastic.

Williams: So in very broad terms, describe what that program was, is, and has become.

Kincade: The Immunobiology and Cancer Program. It was called Immunobiology, which was one of our favorite terms in immunology you'll hear a lot, which sort of encompasses clinical and basic and everything else. We like that term. There's one of the review groups at the NIH was called the Immunobiology Study Section. Charlie Janeway's famous textbook series is called "Immunobiology." So that was my preferred term when I was told to start my own program.

But the president of OMRF, still the same guy who hired me, said, "We need something with the word 'cancer' in it," so we call it "Immunobiology and Cancer." Helps with fundraising a little bit, I suppose. But all the people that I

hired, and they were all wonderful people and good colleagues, great success, worked on things related to how the immune system is built, which was my life's work, and also relationships between the normal process and cancer, immunodeficiency diseases as well, but cancer. So we use a lot of models that are cancer cells, and we study a sequence of events that happens in normal cells also happens in cancer cells. So briefly, that's what the goal was.

Williams: Just before we leave OMRF, I was impressed that you have five hundred patents, I read somewhere.

Kincade: A good number, yes.

Williams: And have produced eleven biotech companies.

Kincade: Yes.

Williams: That's a good record, isn't it?

Kincade: Yes, it's pretty good. I've never wanted to do it, but we have the wherewithal to help people to do it. We have a research park that's about a mile away that's full of spiffy buildings, and companies can be started there, can be launched. We have a new system in place to help somebody when you have no clue as to how to start a company, they can do it for you and handle all the administrative aspects of it and business plan and things like that that you have to do. I've never really been interested in doing it, for a number of reasons, but a lot of my colleagues have, and some of them have been very successful at it.

Williams: We don't have time to get every detail of your scientific career, but I do want to ask you what the highlights of that career have been.

Kincade: Well, the highlights of the career, aside from being able to watch trainees grow and see them take fire and do well, which is certainly a highlight of my career, and things to do with training, it's been a privilege to dissect the steps in building an immune system and see how you have basically an assembly line where things happen. This happens before this thing, happens before that thing. And then to have an opportunity to study things in the environment of that assembly line that control it all, so what slows it down, what speeds it up. It could be what we call a cytokine, which is a kind of growth factor, or it could be an inhibitory factor. We studied those. It could be small molecules, big molecules.

We study now the popular thing is stem cells, and we've always been interested in how these rare cells, which are vital cells in the body. Extremely rare, buried in 30,000 other cells around, there's a stem cell, and it lives in a special environment, and we contribute a little bit to understanding what that environment looks like and where it is.

In my presidential address for AAI, I tried to draw parallels between this assembly line for making blood and building the immune system and the assembly line for making scientists, because that was another passion of mine. But we were lucky on many occasions. We have always had a small lab, and with my limited background in a number of areas and deficiencies in areas, I had to depend on really top-quality students and postdoctoral fellows who came into our lab, and they were given a lot of independence like I did, and they made some spectacular discoveries. And lab managers that I work with have made spectacular discoveries, and we've gone where that led us and just had a lot of fun.

Williams: Major disappointments?

Kincade: I can't really recount one. As I mentioned before, there's something very strange about my career in that I got every NIH grant I applied for, and I just don't understand it. I was trying to study it and understand what accounted for that, besides pure luck, which, of course, was most of it. So that meant I went my whole career without ever being disappointed about getting a grant.

For thirty years, I got the grant the first time for the requested amount, so I didn't even have to revise the grant, and then I had to revise one. I had to revise it twice before I got a perfect score on the third submission. And then they stopped letting you submit three times, and I thought, "Well, I would have been dead." Then the very last grant that I wrote got funded by one point, so I could see it's getting harder. Eventually I'm going to have some disappointments, but so I wasn't disappointed there.

You're always disappointed when a trainee comes to the lab and doesn't thrive, but, fortunately, there were very few of those and made up for it by ones that really, really did well.

Williams: Let's turn to AAI for a bit here. You joined in 1975. What was your motivation?

Kincade: Let me start by saying that AAI was my home. It was my first place. My first scientific meeting was AAI as connected with FASEB [Federation of American Societies for Experimental Biology] in Atlantic City, New Jersey. That was my first scientific meeting, also my first opportunity to give a talk, a scientific talk. And there are thousands of people who would tell you the same thing, AAI is the first place they went to a scientific meeting and the first opportunity to present their work, thousands. There's no telling how many people would tell you the same thing.

So it's open. Anybody can submit an abstract, which will be put up somewhere, at least in a poster. So it's open to everybody. So it was my first experience with a scientific society. It was probably terribly inefficient in those days, but it had a wonderful character to it, and it was, of course, the first place I wanted to be a

member, first society I wanted to be a member of. I joined a lot of other scientific societies and nonscientific societies as well, but AAI was special in being the first one, and I think you'll find hundreds, if not thousands, of people that would tell you the same thing. It's sort of a stepping stone. It's an introduction into an academic career or another kind of career in immunology. It's been very important.

Williams: I was struck by your sort of fast track into positions of authority and whatnot in the organization, first with the *Journal* [*The Journal of Immunology*], I guess, and then the advanced course. You were teaching that after just a few years.

Kincade: Yes, I taught that.

Williams: Then you were a member of the Trainee Affairs Committee for a while, and I know that was an important—

Kincade: Yes, I was on the Program Committee. That was my most important service to AAI, probably. Yes, I think it's that kind of organization. I hope it always is that kind of organization where anybody who wants to can volunteer. When I was program chair, and later president of AAI, it would always please me that a lot of people would come up to you that know you and say, "What can I do? How can I get involved in this organization? I want to do something. What can I help you do? You need any help anywhere? I'll work on the Program Committee; I'll work in the *Journal*; or anything that needs to. I'll seal envelopes." People would come up and volunteer their time. They cared about the organization. They wanted to be involved in it.

I was thinking this morning, two of those people that distinctly stand out in my mind as having come up and volunteered went on to be presidents of AAI. So it's an organization that you can participate in, you can belong to it, and just go to the meetings and sit and listen to talks, or you can get involved.

So it was very open to me. My first important appointment was to the Program Committee, and then I became program chair. Soon after I became program chair, the then-executive director was let go, so I had to go for—I don't know how long it was. It seemed like a year, year and a half, I don't know if it was that long, but a long time as program chair with no executive director running the organization. So I micromanaged, and I did a whole lot of things that hopefully, thankfully, program chairs don't have to do now. But I was involved in minor details like what size room should this particular session be in and everything. The process of inviting speakers and everything was done by me.

Williams: Program there refers to the annual meeting.

Kincade: Yes, annual meeting, planning the program. We always had a terrific Program Committee and, of course, they did the lion's share of the work, figuring out what

the program should be about, what's the theme this year, what's important, what will attract attendance.

Williams: Then you were on the council for a number of years. Were there some big issues you were dealing with there?

Kincade: Every person who serves on the council and goes on to be a president is an amateur as a president and an amateur in this kind of operation. Or at least I think they all are, or most of are, but we all have agendas. We have things we care about. For me, the top thing on my agenda was training issues, trainee issues. I was always disturbed by the fact that I would meet somebody who had a bad training experience, and mine was just spectacular. And I would meet somebody who had an abusive mentor, and it just would go completely through me.

Then I would talk to people who weren't excited about what they were doing, about their projects, or as time got on, there was more concern about people being trained in areas that there were no jobs. People were starting to say, "We've overtrained." And I just thought, "Is this true? Are we really attracting people into a field that has no possibility of them getting the positions they want?"

So that was my top agenda, and I studied up a lot and went on to serve as AAI's representative on the FASEB board and then become FASEB president, where I had at my disposal a lot of policy wonks who could ferret out a lot of information. So I became, for a time, well informed about training issues and career issues. I'm still very interested in it, but I don't study it anymore and certainly don't go to meetings about it.

Williams: Were you able to effect some real change in terms of training behaviors?

Kincade: I hope so. We wrote articles trying to make people acquainted with the facts, because if you throw your hands up in the air and say, "Oh, my god, we should cut back on our graduate programs, it's not fair to our students," what facts are you basing that on? What is the situation? What is the demand for immunologists? If you don't know that, then you can't make informed decisions.

So I worked to try to get answers to questions like that and disseminate them. I went to some NIH-sponsored meetings on career issues. I was present at the founding of the National Postdoc Association [ed. National Postdoctoral Association]. I spoke in the first meeting they had, so was involved in that for a time. That was one of my main issues.

Another issue that I had that I cared about, I didn't do much for. I cared about international issues. So it didn't escape my notice that more than half of the papers published in *The Journal of Immunology* came from outside the United States and that every year people who paid their dues to AAI would make donations, and most of them were from outside the United States, or at least a

huge portion of those just free donations to the cause for AAI were from outside the country. So, clearly there was a desire of people in Japan and China and South America and everywhere else and Canada to be part of what AAI was. So it's the American Association of Immunologists, but it really belongs to the world, and they want to be part of it.

So when I was program chair, I worked hard to invite guest societies to come to this meeting here and put on a workshop, a session that they sponsored, so they could pick the speakers. It just really enriches the program, so you get involvement of other people. I also got the first person, I believe, outside the United States appointed to one of the positions on the AAI program, and I've forgotten what it was. It was membership in one of our committees or something like that. It wasn't a huge thing, but I think before then, nobody outside the United States had ever been invited, and this individual is a very distinguished scientist in Canada. But it was just an effort to incorporate that into the same goals.

I was AAI's representative to IUIS, International Union of Immunology Societies [ed. International Union of Immunological Societies], and tried to understand what the issues were for immunologists in other countries. I wanted to bring some of their bright trainees here and let them be more involved in our training programs, and I failed at that, but that was another agenda item that I had.

Williams: How do you compare IUIS and AAI in terms of representing immunology?

Kincade: Completely different organizations, completely different. IUIS's main role is to have an international congress every year and hopefully to make money for the immunology societies that host it and for IUIS to have some money left over to give some grant programs and some educational programs. They've had courses in immunology around the world. But I think it's a completely different agenda. It's kind of a society of societies; it's a union of immunology societies, rather than a single one. It's not really comparable to AAI.

Williams: Did they have a meeting every year?

Kincade: The international congress, I believe, is every three years, and it's a big event.

Williams: So talk about some other recollections of your year as president of AAI. For one thing, it was the ninetieth anniversary, right?

Kincade: It was the ninetieth anniversary, and I thought a big deal should be made about that, but I remember contacting the editor, one of the editors at *Nature* or someplace, or *Nature Immunology*, probably, and saying, "Hey, this is a big deal, ninety years. It's a nice round number."

And they said, "Well, I think a hundred would be more important than ninety."

So I thought it was an important milestone, and it was, for me anyway, and for the organization.

Williams: Other vivid recollections of your presidency?

Kincade: No. I enjoyed many aspects of it. It's a brief thing, as I told you. You're amateurs, and amateurs that come in bursting with good ideas about what the society should do could wreck it if you started a different agenda every year. If you're lucky, you get one of your agenda items addressed, and, as I said, training was most important for me.

Williams: What about the political aspects of being the president, in the sense of going up on the Hill and things of that sort?

Kincade: Tell you the truth, it was pretty minimal. I went to Washington almost every week as FASEB president. FASEB is an umbrella organization that now represent over 100,000 scientists, including members of AAI. So it's a bigger operation, it's a bigger effort, a more professional public affairs effort than AAI could mount.

AAI does what it can and has, I'm sure, had some wins over the years, but any organization has very limited power over Congress. You have to get lucky and find champions now and again for the research, but I don't think probably, hopefully, any member of AAI believes that the AAI is effecting great change in Washington, is going to get great amounts of grant money liberated to support research. It's just not the way it happens. But I became involved in a lot more congressional activity as FASEB president than AAI president.

Williams: You mentioned that AAI was lagging on women and minority members.

Kincade: Well, they caught up, and when as part of the Training Committees that I chaired and groups that I worked with, we talked a lot about women in science, and I concluded that there was no traction that I could get on that issue. I wanted to understand it better, and it's an interesting statistic that women surpassed men in graduate school some time ago, and all the growth in Ph.D.s and biomedical scientists is female. Male is flat. Dropout rate continues to be high. I don't know how high, but it's certainly higher than for males, because of all of the stresses and constraints there are on female scientists.

But I talked to people who were knowledgeable about women in science. I became very close to the Women's Committee [ed. Committee on the Status of Women] that AAI had at the time. I talked to them a lot about what could be done to improve opportunities for women scientists, and there were a lot of interesting things I learned. I can't say that I contributed to anything, but I learned some interesting things. I learned, for example, that you could kill

women and minorities in science, especially in the beginning when there were very few. Just imagine if you happen to be a black woman Ph.D. and you graduate and you get an academic appointment. You'd think life would be grand, you'd be allowed to do your work and that. But I had quite a number of people tell me that those people just get killed because they get drafted into every committee there is. The committees that want to have balance for male-female and race and all that would insist on them doing that, insist on them being on panels, insist on them doing things way before they would normally do it in their careers, and not leave them any time to develop their careers before they should.

So I had a number of people tell me that, and I thought, "Never thought about that." We need to protect people, not only to attract them to science, but give them a chance to breathe a little bit. And things have gotten better because we have a much better ratio of female scientists now. So it's not as big an issue.

Williams: Let's do some big questions here at the end here. Where do you see immunology going these days? You described it while you were president as unrivaled prosperity in the United States.

Kincade: Yes.

Williams: Is that still the case?

Kincade: I think so, and it's because it's so unfocused or multifocused. It encompasses such a wide range of science. Immunologic techniques are used by every cell biologist there is. The methods that were developed for immunoassays and immunologic procedures have just propelled the science across the board in every field. And you can be interested in endocrinology and hormones and study aspects of that in immunology, or you can be interested in how the immune system develops, like I was, am.

There's so many different ways to say that you're an immunologist. In fact, sometimes when I've gone to some of the meetings, people have asked what I do, and I'd say, "Well, I'm an experimental hematologist," because when cells leave the bone marrow, I don't study them anymore. I don't immunize animals and look at communication between effector cells and the immune system. But other people do, and there's so many facets to it.

What's amazing about immunology, to me, is it's still fresh. It's still a fresh field. Just when you think you know everything, there's a new cell type discovered, and, my goodness, there's another specialized cell type. We don't know how many components there are to the immune system to this day. People are still discovering important regulatory molecules, cytokines that just break the field open, and, wow, you look at everything different. And it happens every year. So I don't know for sure, but from my experience in college, biochemistry is not that

interesting. I mean, it's just not that fresh. Probably it is from their perspective, but not from mine.

Williams: Where do you see the field moving in the future?

Kincade: Well, I think it will go where discoveries lead it, and this is an important thing to me. There are people you could ask that question of, and they would give you learned, thoughtful answers to it. I think they don't know. Perhaps they know. There are some very smart people who know where things are going. But over and over and over again, advances in medical research, and immunology in particular, have been based on serendipitous findings, unexpected findings. And when you build the framework of knowledge to a certain point and you develop technologies that allow you to look at a certain level, you're going to find things that you never intended to find. It's going to be on different topics. You thought you were studying breast cancer, but you were studying osteoporosis. You didn't realize that, but that—and your work goes that way.

Again and again I see biotech companies changing focus and individual labs change focus, and you have be sometimes facile to take advantage of it. That's what I did through my whole career. Whenever we got something hot and new and nobody else was working on it, we went after it until very soon we had competitors who were better than us, and we found something else to work on. It's a long answer to your question about where's it going, and I think it will be driven by discovery.

Williams: How important are technological discoveries for the field, or do you pretty much have the equipment that you need at this stage?

Kincade: You can look at that two ways. You could say that technology drives what we do. Where would we be without high-speed cell-sorting capabilities and now a lot of new technologies for working with individual cells, some really important technology that we'll use. So you could say that enabling technology is crucial, and it is, that's true. It's expensive. It definitely is. But the other side of it is there are people who think that you get a method and then you look for a question, and it's driven by this half-million-dollar or million-dollar instrument that you have.

There's an old saying attributed to many different famous scientists, so I don't know where it came from, but the phrase is, "If it's not worth doing, it's not worth doing well." Again and again you see students and trainees who will dive into collecting a lot of data with some high-throughput instrument, and now you can get reams and reams and reams of information with no question. They do it because they can, and you shouldn't do it well if it's not worth doing. So that's my two answers.

Williams: What's your position on recommending the field to students and trainees today, and what areas of the field do you steer them into?

Kincade: Let me tell you broadly what I tell students, because I'm really passionately interested in students and trainees and attracting people to this and sharing the joy that I've experienced. The first thing I ask them is, "Can you do anything else? Is there anything else that you like? Do you know what this is?" and that sort of early discussion. And if they're really interested and persist with an interest in stem cells and hematopoiesis and the things that I know about, then the next question is, "Do you know what the unemployment level is for scientists?" Nobody knows. You can ask anybody, and they can't tell you. But the truth is, it's almost zero. The unemployment for Ph.D. s in the biomedical sciences is almost zero. So there's a job to be had there. So if you get a Ph.D. in the biomedical sciences, you're going to have a job.

And the satisfaction surveys that I've seen carried out five years after people got their degrees, most of them say they're doing what they wanted to do, I think probably more than shoe salesmen would say and more than physicians might say. But it ain't too bad. So the second thing is there's a job here.

So then you have to decide what kind of person you are. Someone told me years ago, you can divide students into two categories broadly. The ones that are interested in what's known, and those do really well on standardized tests, they can learn massive amounts of stuff. Then there's this other category of people who are interested in what's not known, and particularly I'm in that category—people who are interested in what's not known that I could answer. What is something I could address? What intrigues me? So it helps a lot if you're in that second category.

And then you have to decide what kind of career you really want to have and how willing you are to follow somebody else's lead or what level of independence do you need. If nothing else will satisfy you but discovery, I tell students you have to decide if you have a receptor for it. If you have the receptor, the opium is discovery, and all you need to have is just a little whiff of that opium to get you started, and you want to come back for more and more and more, and you'll develop a need that nothing else can satisfy. You have to have your own independent way of doing experiments, you have to have a chance of doing that, and you'll put up with all kinds of money problems and trainee problems and institutional problems just so that you can get that high.

So for the rest of the people, there's a job to be had, important jobs to be had, good jobs to be had, wonderful jobs in pharmaceutical companies. I've had a lot of friends who've had just wonderful careers. Many of my trainees have. Administration and many other ways, but there's a job to be had, and I think it's important to tell them that.

Aside from all that, your original question, I think, was how does immunology strike your fancy, and I think it depends a lot of times on the teacher. If they had Bruce Glick to teach them, they'd be attracted to immunology. Max Cooper, if that was the professor, they'd be into it. A lot has to do with who introduces you to it.

Williams: What do you do outside of science for fun and recreation and so forth?

Kincade: Well, I've tried a few things. Always like to read. When I was fifty years old, I bought my first Harley-Davidson. When I was sixty years old, I got my private pilot's license. I'm not sure what seventy's going to bring. Maybe try some different things like creative writing is something I've been thinking about maybe someday I might do a little bit of. Just dabble at other things, did scuba diving, did a hundred scuba dives and got that over with. But I like to read, I like to garden, I like to plant things. That's pretty much it.

Williams: I know we haven't talked a lot about particularly scientific things today, as much as you may have wanted to, but I think from my standpoint we're pretty well done today.

Kincade: Well, good. It's been a pleasure.

Williams: Do you have anything left you want to say?

Kincade: No. I think it's a great organization, and I thank you for this interview and hope you will continue to do this and capture the essence of immunology and immortalize it. It's a great thing. Good idea whoever's idea it was.

Williams: And you've certainly added a great story to the collection. Great. Thank you.

Kincade: Thank you.

[End of interview]