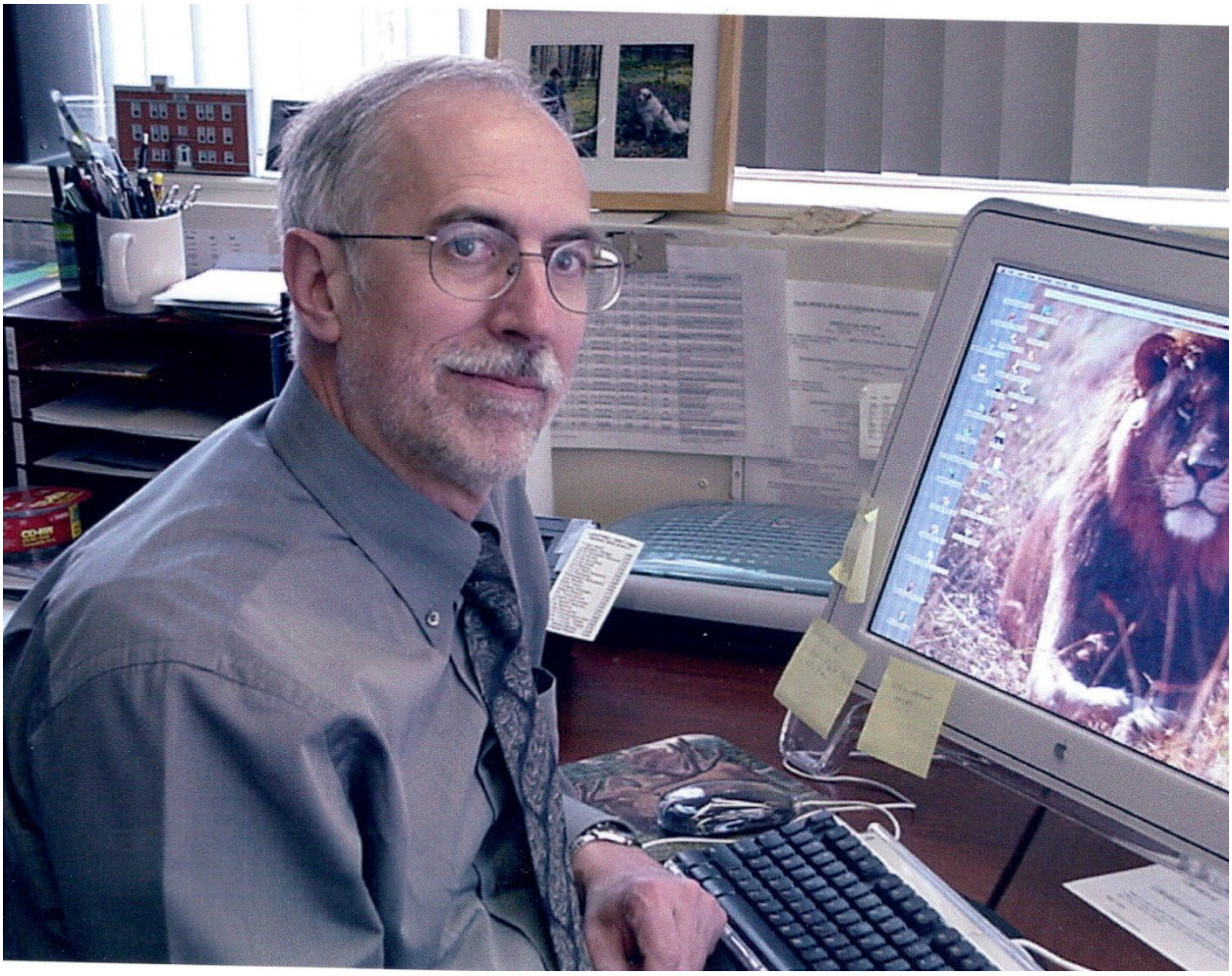
Dr. Thomas J. Kindt Oral History

This is an oral history with Dr. Thomas James Kindt on July 26th, 2023, and August 2, 2023, about his career at the National Institute of Allergy and Infectious Diseases (NIAID) at the National Institutes of Health (NIH). The interview is being done over Zoom. The interviewer is Dr. Victoria Harden, Founding Director of the Office of NIH History and Stetten Museum (ONHM).



Dr. Thomas Kindt

Harden: Dr. Kindt, please state your full name that you know that this interview is being recorded and that you give permission for the recording.

Kindt: My name is Thomas J. Kindt. I give permission to record an interview with Dr. Victoria Harden.

Harden: Thank you. You were born on May 18th, 1939, in Cincinnati, Ohio, as the only son of James Michael Kindt, a bookkeeper, and Barbara Mayer Kindt, a homemaker. You had two older and one younger sisters, Barbara Joan Kindt Thomas, Jeanne Kindt Rogers, and Judy Kindt Naugle. Tell me about your life growing up, just through high school, and note especially if there were family members or teachers who nudged you towards a career in science.

Kindt: We were a working class family. My parents were bright enough, read the newspapers, read books, but no one in my entire family had gone to college. The first person to get any higher education was my oldest sister, who went to nursing school, so there wasn't a lot of family influence in my choice of careers. I went to Catholic school, and I have to say that the emphasis there was more on discipline than on science.

Harden: Did you go to Catholic schools all the way through high school?

Kindt: Yes, I went to the local grade school, and then I went to an all-boys high school, which took in a pretty large neighborhood in Cincinnati. It was called Elder High School.

Harden: And were there any teachers there who you remember as being particularly interesting?

Kindt: My freshman algebra teacher was interesting and gave me some interest in learning math and learning how to manipulate words and numbers. My chemistry teacher and physics teachers were—I would just say that they were boring.

Harden: In 1959, you enrolled at Thomas Moore College in Covington, Kentucky, where you studied chemistry.

Kindt: Between when I left high school and started college, I spent two years on active duty in the Navy.

Harden: Oh, wow. Tell me about those two years. They weren't listed on your CV.

Kindt: When I joined the Navy, I was stationed on a destroyer and traveled widely. We went to the Mediterranean for about nine months, stopping at many places in Italy, in Greece, in Spain. I saw a bit of the world in the Navy. I began on the deck force of the destroyer, where they started new recruits, but then I was taken into the sonar team, and there I got to stand sonar watches looking for submarines. And you may remember that in those days, the idea of Russian subs was not just theoretical. We actually did see and chase a few. I also learned at that time how to do some basic manipulations of electronic equipment, because we did maintenance of our own sonar gear. At the end of my tour, I decided not to stay in the Navy.

When I got out of the Navy, on the advice of my brother-in-law Donald C. Thomas [Dr. Donald C. Thomas], then a grad student and part-time biology teacher, I went to Thomas More College, which was then called Villa Madonna College. It was a downtown, put-together college in Covington, Kentucky, at that time. Now it's a university with a large campus in a Northern Kentucky suburb, but then it was just downtown. We had labs in old firehouses and buildings that they had taken over in downtown Covington. I majored in chemistry. Why did I take chemistry? I'm not a hundred percent sure. I liked the idea of some math with my science, and I had a chemistry set as a child, and well, don't ask my mother, but I had a lot of fun with it. And I thought maybe playing with chemicals could be a profession.

What switched me from chemistry to more biologic subjects was a summer job, which became a part-time job at a place called the Institutum Divi Thomae in Cincinnati. It was a little institute funded by a man named George Sperti [Dr. George S. Sperti], who had invented a couple of remedies and made some money on them, and also invented a process for making orange juice concentrate that tasted fresh. The institute had graduate students and postdoctoral fellows doing various projects, and I got a summer job there. My first job was in bacteriology, where I worked with *Staphylococcus aureus,* determining growth requirements for different strains that had different biologic effects. Some were pathogenic, some were not. At any rate, that got me much more interested on the biologic side, and I drifted toward biochemistry at that point.

Harden: In 1963, you graduated with a Bachelor of Arts degree, cum laude. Tell me first, why you decided to go to graduate school, and then why you choose University of Illinois in Urbana? You had an NIH predoctoral fellowship. How did all this play out?

Kindt: A chemistry professor at Thomas More College, Dr. James Cantrill, who had recently graduated from MIT [Massachusetts Institute of Technology], was a well reputed chemist. We discussed things, and he thought that I should go to graduate school and recommended a handful of them, most of them in the Midwestern area: Purdue, Indiana, Illinois. I applied, and Illinois made a fairly generous offer for graduate school, so I went and talked to him about it.

He said, "For chemistry, it's an excellent school, an outstanding school." So I said, "Okay. I'll go to Illinois." And the chemistry department had a biochemistry division, which I joined.

Harden: Tell me about your Ph.D. program there. Who was your mentor? Who were your student colleagues? How did you develop the focus for your dissertation?

Kindt: My fellow students at ILL [University of Illinois] were at first a bit overwhelming; many had degrees from Ivy-league schools, and several had research experience working with well-known scientists. There were Harvard, Radcliffe, Princeton, Amherst grads, and another who had worked with James Watson [Dr. James D. Watson] at Cold Spring Harbor for the summer. There was another military veteran from Oklahoma State University, and we became friends based on mutual backgrounds. After a semester, the group became easy to interact with and some social ties formed.

One of the things that interested me in college were carbohydrates, in which you had similar molecules put together different ways, different stereochemistry. Also varied were the polymers of the carbohydrates. Take something like glucose, and you could make cellulose, or you could make glycogen, which is just a blob of sugar and starch, et cetera. These are all polymers of carbohydrates. I interviewed several faculty members at Illinois, but I chose to do my research in the lab of a carbohydrate chemist named H. Edward Conrad [Dr. H. Edward Conrad], and because I had experience with handling bacteria, I did a research project concerning the synthesis of a glucose biopolymer in a strain of bacteria, *Aerobacter aerogenes.*

Harden: Before your Ph.D. was awarded in 1967, you had already, in 1966, published your first paper, which was also on the structure of this bacterium and was supported by an NIAID grant. Will you tell me about that?

Kindt: There were new techniques coming in for determining the structure of carbohydrates, and Illinois had a very diverse and rich set of chemists. I was able to do some work with NMR [nuclear magnetic resonance], for example, and did some radio labeling techniques, which were being done in another lab at Illinois. I applied these techniques to the structure of an oligosaccharide molecule, and it resulted in new data that was publishable. For my thesis research, I came up with some ideas for determining how the bacterial glycogen polymer I was studying was put together, how there were linear parts and branch parts, and I worked out some techniques that would determine which were branch, which were linear. My work turned out to be worth publication.

Harden: While you were in graduate school, on September 5th, 1964, you married Marie Robinson, who was also a biochemist. Did you meet her at the University of Illinois? Would you tell me about her?

Kindt: We were lab partners in the Advanced Biochemistry Department, and the story is that one of our projects involved growing a large carboy of *E. coli* to isolate mitochondria or something, I can't remember exactly what. But we inoculated our carboy, and the *E. coli* didn't grow very fast. We had a lot of time on our hands, so we went out and got something to eat and drink, and we formed a relationship. Before you know it, we were dating, and then we got married.

Harden: Did she continue to work in laboratory research?

Kindt: What she did was take a master's degree at the end of her first year and go to work in the laboratory of Lowell Hager [Dr. Lowell P. Hager], who was working there in protein chemistry. She worked as a technician for the next three years as well as later at City of Hope and Rockefeller before we came to the NIH.

Harden: After you got your Ph.D., you became a postdoctoral fellow for two years, from 1967 to 1969 in the Department of Immunology of the City of Hope National Medical Center in Duarte, California. And then, you were promoted to be an assistant research scientist for another year. Tell me about your research during this time.

Kindt: One of the professors of biochemistry at Illinois was a man named Charles Todd [Dr. Charles W. Todd]. He was an immunologist who had recently spent three years at the Institut Pasteur and came to Illinois as an instructor, just transitionally coming back to the states. I did a small project with Dr. Todd in my biochem course, and we became acquainted. He was moving to City of Hope in California, to start a new lab. He discussed with me the possibility of going with him to be a postdoctoral fellow in California. It was a very attractive idea to me for several reasons. One, it would move me to a more biologic type of research, working with animals, and secondly, to go from Illinois to California, I mean, hey, that was a good deal!

So when I graduated, my wife and I moved to California, and I assumed a postdoctoral position with Dr. Todd in a very exciting atmosphere at the City of Hope. Because of his interest in the genetics of immunoglobulins, Dr. Todd was a member of the Genetics Department at City of Hope. The department included members with a range of interest from molecular biologists, such as Arthur Riggs [Dr. Arthur D. Riggs], who cloned and expressed gene for insulin, to theoretical scientists such as Susumu Ohno [Dr. Susumu Ohno], who proposed gene duplication

as a means of gene evolution. The department offered many good learning opportunities and had a very good atmosphere. We were also in easy driving distance of Caltech [California Institute of Technology], where I found some collaborators in immunology and in allergy. I felt that Dr. Todd was a great mentor. It was a productive postdoc for me.

Harden: I was especially interested in one paper you published in 1969. This was "Amino acid sequence restriction in rabbit antibody light chains," in *PNAS* [*Proceedings of the National Academy of Sciences*]. You published it with Leroy Hood [Dr. Leroy E. Hood], Richard Krause [Dr. Richard M. Krause] and others, and it's a big deal to publish in *PNAS.* This was also the first time I could see that you had a personal scientific relationship with somebody at NIH, Dr. Krause. Furthermore, one of your colleagues had a Public Health Service international fellowship, and the study was funded by NIAID and the American Heart Association. It was sponsored by the Armed Forces Epidemiological Board and supported by the Office of the Surgeon General of the Department of the Army. What was so important about this particular paper?

Kindt: We were looking at antibodies that were raised in response to injection of streptococcal vaccines, and that was the link that got us to the Heart Association and probably to NIAID. Much of the information about antibody structure was obtained by study of proteins produced in patients with an immune-proliferative, leukemia-like disease called multiple myeloma. These monoclonal immunoglobulins were produced by a cancerous cell that grew unchecked and were a single molecular species, making them useful in structural studies such as those carried out by Edelman [Dr. Gerald M. Edelman] and coworkers. The limitations of using these disease products were that there was not a means to know the specificity of the immunoglobulin, and availability was dependent on random, unfortunate cancers. Krause and his group demonstrated that immunization of certain rabbit strains with streptococcal vaccines induced the formation of antibodies in high concentration that were a single molecular species. Studies in collaboration with the Todd lab showed that the genetic markers on these antibodies were consistent with a monoclonal origin.

Harden: I also note that you turned 30 in 1969, and this was the height of the Vietnam War. I presume that your time in the Navy satisfied your military obligation and allowed you to continue your work in the laboratory.

Kindt: The simple fact of the matter is that when I was 18, I received a draft notice to go in the Army, but I was already scheduled to go in the Navy, which I did. I served my time in the Navy, which then was sufficient to exempt me from further service. Now, I have to say, there was one point when the Berlin Wall went up [1961], when I was in college, that I was contacted to go back into the Navy. But the registrar at college was able to obtain a deferment. I had served my time and didn't have to go back.

Harden: In 1970, you switched coasts. You moved from California to New York to do another postdoc at the Rockefeller University in the Laboratory of Bacteriology and Immunology. Why did you decide to make this move and with whom did you work and what were you working on? Tell me about it all.

Kindt: The reason for the switch was that Dr. Krause, who was then at Rockefeller, had begun a collaboration with our laboratory in California. He had a system of making antibodies with streptococcal vaccines that were of limited heterogeneity, which we already discussed briefly. The thing is, he had a colony of rabbits that were bred to make these antibodies, and it was of interest to apply the knowledge we had made on the genetics of antibodies with Dr. Todd to the antibodies that were made by Dr. Krause's rabbits. So I sat down and wrote some grants, and one of them was funded by the American Heart Association for what they called an Established Investigatorship, which gave me five years of salary. That was just a beautiful thing to get. And so I went to Rockefeller, but there was no immediate position. I went as still a postdoc, but shortly I started to climb the academic ladder there.

But why I went there was strictly to gain access to this group of rabbits that Dr. Krause had. And it turned out to be a fruitful line of investigation because with a collaborator from Germany who was an assistant professor in the lab, Klaus Eichmann [Dr. Klaus Eichmann], we were able to show inheritance of antibodies in a rabbit family. It was the first time anybody clearly showed that a specific antibody was passed through a family.

Harden: As you said, from 1971 to 1977, you went up the ladder at Rockefeller in a newly renamed Laboratory of Immunology and Immunochemistry, and you ended up as acting head of this laboratory. Is there anything else that you should tell me about this time, including why the laboratory's name changed, who changed it, and what other research you worked on?

Kindt: The original laboratory was historically very interesting. It was headed by Maclyn McCarty [Dr. Maclyn McCarty], one of the co-discoverers of DNA, and Rebecca Lancefield [Dr. Rebecca C. Lancefield], who devised the typing system for the streptococcus, known as the Lancefield grouping, with clinical typing for streptococcal diseases. They were heads of the lab with Dr. Krause, who joined as a senior person. He later changed the direction of the research and renamed the lab. It became a new lab with myself, Dr. Krause, and a couple other people, because we were no longer doing the streptococcal research that Dr. McCarty and Dr. Lancefield had fostered. It was an evolution. It wasn't a revolution or anything negative. It was a positive evolution.

Harden: In July 1968, you and Marie welcomed your first child, Rachel, and in October 1972, your second child, James was born. Would you tell me how you and Marie managed what is today called work-life balance?

Kindt: Very simply, I worked, and she took care of the home, mainly. In California with the one child, it wasn't such a bad deal, but when we were in New York and had the second child, it became a financial issue. She worked part-time in New York for a while. I have to say that she was very generous in allowing me to pursue my research in New York, and I don't remember ever working harder than I did in New York. I would maybe take Sunday afternoon off, but that was about it. Writing grants, maintaining the lab, doing the type of research we were doing with the animals that had to be tended all the time—it was really hard work, and I loved it, but it didn't give me a lot of time to be at home and with the children. We did make some arrangements. I tried to be home at 7:00 pm every night for dinner, which I managed most of the time.

Harden: From 1973 to 1978, in addition to your work at Rockefeller, you were an adjunct associate professor in the Division of Human Genetics at Cornell University of Medical College. Will you tell me how your research in immunology related to what you were teaching about human genetics?

Kindt: The connection was that my research in immunology gravitated to a field called Immunogenetics. I became interested in the inheritance of the factors of the immune system, and I also had an interest in genetics in general. The head of the Department of Medicine at Cornell, Alick Bearn [Dr. Alexander G. Bearn], was a geneticist and also a friend of Dr. Krause's. We got to talking, and he thought that I might be interested and be able to add to lab meetings in his Division of Human Genetics. And it turned out that it was very interesting. I don't know if I added too much, but I certainly learned a lot by listening to the geneticists there who were into human disease, inheritance, amniocentesis, and typing of the fetus. It was quite an interesting jump for me.

In addition, it turned out there was what they called the Field of Genetics (an area of graduate study), which included faculty and students from Rockefeller, Cornell, and Sloan Kettering. As a member of this group, I was able to obtain graduate students and sometimes part-time workers in the lab. A couple of them were docs who were working residents or had some level of training in the hospital, and they wanted to learn some lab techniques, so they came over and joined us. It was very beneficial, but it didn't lighten my workload.

Harden: The mid-1970s marked a great expansion of knowledge about the body at a molecular level. It was an extremely fertile and productive period in general. What can you tell me about this?

Kindt: My laboratory always had a molecular focus. We did structures of proteins and had some rather inventive ways of labeling the proteins on cell surfaces. We were using radiochemical amino acids, growth in different amino acids, and then we sequenced these proteins. We were able to obtain information on very minute amounts of protein. Different people in the lab did different aspects of this, but the focus was always on the genetics of the immune system. How does it work? As you can probably see, one would naturally gravitate from antibodies to cells. And when you began looking at the cells, you got into the major histocompatibility complex (MHC).

For this work, we collaborated with different people, mainly Stan Nathanson [Dr. Stanley G. Nathenson], who was at Yeshiva University in the Bronx. Our own work with the rabbit MHC yielded some interesting results. We shifted from antibodies to cellular molecules and protein structure, and then of course to the DNA. Now, I was not a molecular biologist. I had not trained in DNA chemistry. That's very important. But what I did after I had moved to the NIH for a few years was get them to allow me to spend a year at the Institut Pasteur in Paris.

Harden: We're coming to that in detail. Don't go too far down that road yet.

Kindt: Okay. But that was the point of my personal shift to DNA biochemistry.

Harden: In 1975, you had received the Professional Achievement Award from your alma mater, Thomas Moore College, and two years later, 1977, you were recruited to NIAID as Chief of the Laboratory of Immunogenetics. Would you tell me about making this shift? Richard Krause, of course, was director of NIAID at this time. And Ken Sell [Dr. Kenneth W. Sell] came in as Scientific Director sometime in 1977, So I don't know if he was involved in recruiting you or if it was all Krause, but please tell me how it all played out.

Kindt: Well, my negotiations with NIAID did not involve Dr. Sell at the beginning. They were with Dr. John Seal [Dr. John R. Seal], who had been Scientific Director until 1975, when he was appointed Deputy Director of the entire institute. But since Dr. Sell had not assumed the job as Scientific Director when my negotiations were taking place, I dealt with Dr. Seal, who was also serving as Acting Scientific Director. At that time, I had several job offers and chose the NIH for several reasons. I had a good offer at the Scripps Institute in La Jolla, and sometimes I wonder, should I have taken it?

Harden: The weather would certainly have been better in La Jolla.

Kindt: Yes. It's interesting that some of my advisors, including Dr. Todd, with whom I still discussed things, and who, incidentally, would have taken me back at the City of Hope, convinced me that the grant world could be shaky, and that Scripps operated solely with grants. There were also some personalities that were hard to deal with at Scripps, so going to the NIAID seemed like a pretty good idea. So I chose NIAID and made the transition from Rockefeller to the government.

Harden: And never looked back.

Kindt: Spent 28 years in the institute.

Harden: Would you describe the intramural program when you arrived in Bethesda? How did things function? Who were your colleagues? Just build a picture for me.

NIAID Lab chiefs, 1979. Back row, left to right: Thomas Kindt, Robert Chanock, Malcolm Martin, William Paul, Norman Salzman, Richard Asofsky, Anthony Fauci.

Front row, left to right: Wallace Rowe, Michael Frank, Kenneth Sell (Scientific Director), Franklin Neva, Roger Cole.

Kindt: It was not an easy transition. There were some major differences in how you were treated, which I hadn't realized until I got there. At the university, I had a nice number of grants and a number of people who were ready to help me with hiring, ready to help me with getting equipment. After a couple months, I understood that the big difference was that at the university, if you had the money, you could do anything you really wanted to do. At the NIH, money didn't make any difference at all. You had to convince a bunch of bureaucrats that they weren't going to get in trouble helping you do what you needed to do. And for a lot of the equipment we wanted, we'd fight for months—writing, rewriting proposals. Space was also a problem because my lab was shoehorned into the institute and started off in the basement of, I think it was Building 5, if I'm not mistaken. At any rate, it was a difficult transition, but I had some people who came with me from Rockefeller, and we fought it out until we achieved a reasonable situation. My fellow lab chiefs were an eminent group, but not particularly

welcoming to new lab chiefs, because of resource limitations, and none had been friends previously.

Harden: Given all the bureaucratic and ethical restrictions associated with working for the federal government and the multiple other job offers you had, what was it that kept you at NIAID?

Kindt: That's an interesting question. I did interview at a number of places and had a number of job offers, some attractive, some just okay. I would say that when I was on a trajectory with my research, I didn't want to cut it off and go somewhere else and start over. If we had started to learn things, I wanted to keep moving along. At that point, to take another job would have cost the year, and I really didn't want to do that.

One positive feature was the ability to travel as a government employee and have access to areas not readily open to all. A good example of this was a trip in 1980 to the Peoples Republic of China (PRC). When the U.S. wished to establish relationships with the PRC at the end of their Cultural Revolution, science was one area of possible collaboration. Dr. Krause, then Director of NIAID, was charged with exploring means to do this. With a small group--myself, Dr. David Gordon from the CDC; Dr. Joseph Davies from Washington University; and Dr. Krause--we spent several weeks in Beijing and Shanghai talking with officials, visiting labs, and exploring potential areas for cooperation. We were taken to major tourist attractions--the Forbidden City, Tiananmen Square, the Great Wall, the Ming tombs, the Temple of Heaven, etc.--with informed guides and treated to excellent meals (the ability to use chopsticks required) and cultural events, including the Chinese opera. Lab visits included seminars in some cases, where translations were sentence by sentence, making them quite painful, especially when hosts could not agree on proper term in Mandarin.

Private vehicles were not allowed so traffic in Beijing was mainly bicycles, and our transport van moved about easily. The labs we visited were not up to standards, and the personnel were behind in technique. A recurring topic was contrast between "Western and Traditional" Medicine. Hard to find a counterpart to treatments that concentrated one's life essences. A notable exception to lab quality included labs dealing with agricultural topics which had continued work and maintained contact with outside groups during the Cultural Revolution. Several contacts with excellent scientists were made and proved useful in screening possible candidates for positions in U.S. labs. We left the PRC on a Japanese flight to Tokyo, and I was glad to have a drink with ice in it without worrying if all components had been boiled.

I revisited PRC in 1997 to find that Beijing now had three Beltway-like roads circling it, and these were usually jammed. Several of my previous contacts were there, and reunions were warm. Many scientists now spoke English and wanted information about our latest findings. So

that ability to travel as a government employee and have access to areas not readily open to all, among other advantages, prompted me to stay at NIAID.

Entrance to Forbidden City, 1980. Portrait of Chairman Mao.

Harden: And you would have had to write a grant proposal for new funding?

Kindt: That's right. And as you know, the review process at the NIH at that time was almost non-existent. It became more rigorous with the four-year review that meant writing up serious proposals to continue. But the NIH review process was a lot easier than trying to get enough money and enough people together to do something in a university. I should say, I also looked at a few jobs in industry, and there it was always a question about what they would allow you to do. You could say, "Can I continue this area?" And the answer would be, "Well, you can continue that, but the genetics, that's not going to make us a lot of money. Maybe you want to drop that promising project in T cell genetics . . . ." At any rate, I didn't go anywhere; I stayed at the NIH. And as I mentioned before, at this point, I applied for a sabbatical, which made a very big difference in my career.

Harden: Yes, indeed. But just before we get there, I want to note the two Rockefeller scientists that you brought with you to NIAID, Dr. John A. Sogn and Dr. John E. Coligan. They came with you to function as the core of the Laboratory of Immunogenetics that NIAID had created for you. So when you three arrived, what were your initial goals for the lab, and what did you undertake at the outset? I note that both those gentlemen moved off in other directions but talk about when you got there.

Kindt: When we arrived at the lab, we had several objectives. One was to get a functioning protein chemistry lab set up, and John Coligan did most of that. It took a bit of time and there was a little trouble with infrastructure. Some of the electricity didn't support some of the instruments, but no need to go into all of it. John Sogn was more interested in the genetics of the immunoglobulins. He had a few findings about new markers in that area.

And we set up a rabbit colony. Now, that was another adventure, trying to get a contract together to do this. I don't know how much time I spent on that effort, but we finally got the colony established at a place in Howard County in suburban Maryland on a farm. The owners had run a dog boarding kennel and had the space and personnel to bid on our rabbit contract. It turned out very well, and they were able to support our research as it evolved into virus infection models. The staff who managed the rabbits were dedicated —we always found that farm people handled the animals much better than city people, because it wasn't just a job. They cared for the animals, and they treated them very well. John Sogn did most of the work on the rabbit colony, and John Coligan started the protein chemistry lab.

Harden: In 1981, you published a paper that was first to report the complete primary structure of a major histocompatibility complex antigen. It was among the hundred most cited papers in 1982. Would you tell me about this work and why it was so important at this particular time?

Kindt: I think it was important because people said it couldn't be done. There was such a small amount of the histocompatibility antigen on the surface of cells that if you use conventional techniques, you'd grow trillions of cells and come up with almost nothing to work on. Cell surface proteins were particularly difficult because they always had a piece on them that helped them stick into the membrane of the cell, and that made them sticky, gooey, lipidy, whatever you want to call it. They were very difficult to work with.

But we had a couple of techniques, and we worked with Dr. Nathanson, who had the mice that were making the cells and had a lab with some people who were experts at cell culture. They would add individual amino acids labeled to the cell culture, grow the cells, isolate the molecules, and get the minute amount of protein that you could isolate. That’s what you could sequence. Of course, the sequencer wouldn't see anything, but the radio label would show up

in the readout. So you could define the major histocompatibility antigen by taking the peptides, sequencing them, finding the radio label, and painstakingly putting together the whole map of the protein. And with a couple of years work—and a couple of worn out postdocs—we were able to demonstrate the complete sequence of a cell surface protein.

Harden: Now, at long last, let's move to Paris. From 1982 to 1983, you were a visiting scientist in the Laboratory of Analytical Immunochemistry at Institut Pasteur. You said you had a sabbatical. Tell me about getting that, about whose lab you worked in Paris and what research you did, and whether you continued collaborations later? That's a lot. Just tell me about it all.

Kindt: The laboratory I went to was one to which I had historic ties. It was the French laboratory that Charles Todd had worked in before he came back to the United States. The head of the laboratory at that time was Pierre Cazenave [Dr. Pierre-André Cazenave], but the head of the lab historically was a man named Jacques Oudin [Dr. Jacques Oudin]. Jacques Oudin had discovered genetic markers (called allotypes) on rabbit antibodies and had used them to determine some of the basic facts of immunoglobulin inheritance, antibody inheritance. So that lab was familiar to me. I had had contact with Pierre-André Cazenave, and he invited me to come to his lab. The project I wrote up and had reviewed by the NIH was pretty rigorous. I wanted to make sure that NIH knew I wasn't just goofing off. The project was to look at some rabbits that had been isolated on an island off of Tunisia. Now, you may know this, but the Phoenicians domesticated rabbits.

Harden: I didn't know that.

Kindt: They spread them around the Mediterranean, but one of the places they came from was this little island called Zembra, which is off Tunis in the Mediterranean. And on this island were some rabbits that hadn't been touched or outbred or had research done on them for a long time. We wanted to identify the genetic types of immunoglobulin found in these feral rabbits. The Institut Pasteur in Paris had a connection to the Institut Pasteur in Tunis, and so I started looking at these rabbits as my primary project. However, in order to broaden myself, I wanted to learn techniques in molecular biology. I collaborated with other labs at Pasteur, learned new things, and set up some techniques in the lab that we could use to look at the immunoglobulin genes.

It was fun, but it was somewhat difficult, because I didn't speak French when I went there. I had to learn quickly because that's what they spoke in the lab. And after making some really dumb mistakes—embarrassing mistakes, some of them—I learned. By the end of the visit, I actually was able to instruct in French in a course on MHC molecules.

Harden: Did your whole family move to Paris?

Kindt: Yes, they did. And it was wonderful for them. The children went to a bilingual school, and my wife worked in the laboratory of an old friend of ours at the University of Paris, Donny Strosberg [Dr. A. Donny Strosberg] was his name. She did part-time work there, mostly helping them edit papers and prepare clean translations of their work in English.

Harden: While you were there, the disease we now call HIV/AIDS had recently been recognized by the medical community, and people at the Institut Pasteur were the first to isolate a retrovirus that eventually was demonstrated by Dr. Robert Gallo's [Dr. Robert C. Gallo] lab at the National Cancer Institute to be the cause of AIDS. Did you know about any of the work being done on AIDS at the Institut Pasteur?

Kindt: Vaguely. I knew about Luc Montagnier [Dr. Luc Montagnier] and his lab, but I didn't know him personally at that time. When I came back to the NIH, research on AIDS was a central part of NIAID, and I learned a lot about it then. Some of the people from my lab went to Tony Fauci's [Dr. Anthony S. Fauci] lab to work on it. But at Pasteur, SIDA [Le syndrome d'immunodéficience acquise], as they called AIDS, was thought of low-key. People were not excited. If I walked through the labs at Institut Pasteur and talked to people, they were primarily talking about tuberculosis and different neurologic diseases; they weren't talking about SIDA.

I think that if you look at the whole background, there's a separation between medicine and basic science in France that doesn't really exist in the United States, certainly not at the NIH. The people who studied medicine were different from the people who did the basic research in France. There wasn't this bench to bedside attitude, if you will. So AIDS was downplayed at the Institut Pasteur in 1981, but that changed rapidly. Now, my observation cuts off at 1983 when I left, and when I went back later, concern about SIDA was full-blown.

Harden: In 1984, you and Donald Capra [Dr. J. Donald Capra] published a book, *The Antibody Enigma*. It traced the scientific progress leading to an understanding of how antibody genes are able to encode almost unlimited potential to respond to foreign antigens. Would you tell me about why you chose that title and more about what you conveyed about antibodies in the book?

Kindt: At immunology meetings, there had been endless theoretical discussions about how the diversity of antibodies could be genetically encoded and maintained. I mean, if you think about

it, it is a real mystery. The title of the book was chosen by Don Capra. He had been reading Robert Ludlum or one of the other authors of mystery books, and he said, "They always use two-word titles." He said, "Let's try this one, *The Antibody Enigma*." That's the title he wanted. And I said that if I let him have that title, he would let me be first author of the book. I mean, that makes little difference on a two author book. But at any rate, that was the deal we struck.

What we wanted to do was summarize all of the outpouring of theory, to try to boil it down. What were they really saying? What are the answers? Is there a solution? And, of course, we had a solution in mind. It turned out that probably before the book hit print, the molecular biologists had come across a solution, which was not unlike the one we proposed. But at any rate, the book was a summary of us doing our work, going to meetings, interacting with other immunologists, and just putting it together in one place.

Harden: And it's still being used, I understand.

Kindt: One or two of my colleagues have told me that they still assign it to students.

Harden: In 1985, you and Mary Ann Robinson [Dr. Mary Ann Robinson] published a paper documenting the first demonstration of genetic variation in genes encoding the T cell antigen receptor in humans. Tell me about that work.

Kindt: We were very excited by that, I can tell you. At the NIH and in Canada at the same time, the T cell receptor, which had been an enigma, was discovered, sequenced, and characterized. This molecule, which is on the surface of T cells, figured prominently in immunity. Mary Ann had molecular biology experience, and she liked to look at human questions. She had trained in a laboratory with Bernard Amos [Dr. D. Bernard Amos] at Duke and had experience in histocompatibility genes and other human genetic phenomenon. She put together a group of cells that she made by viral transformation, and then she typed these cells for their T cell receptors. And what she found was a genetic variation that held up very nicely through inheritance. The reason we thought this was important is because a number of diseases might well be linked to the ability of T cells to react with certain pathogens or auto react with self components to cause autoimmune disease. And so this turned out to be important and well received work. It also led to collaboration with Stephen Hauser [Dr. Stephen L. Hauser], who then was at Harvard and now is in San Francisco at UCSF [University of California, San Francisco]. He is a neurologist studying MS [multiple sclerosis]. We looked at some family cohorts of MS to determine if there were a pattern of T cell receptor genetics correlating with the occurrence or severity of multiple sclerosis.

Harden: In 1988, you and Henrietta Kulaga [Dr. Henrietta Kulaga] and Tom Folks [Dr. Thomas M. Folks] applied for a patent entitled “Animal Model for Diagnosing and Testing Vaccines or Therapeutic Agents Against AIDS.” It took eight years to get this approved. Will you to tell me why it took so long to get approved? You have another patent that only took two years to get approved. Talk to me about this patent.

Kindt: There was controversy at the time at NIH about anything that was done with AIDS. Let's say you wanted to publish a paper on AIDS. It got reviewed and re-reviewed, rejected, revised and reviewed again, and back and forth. People were very cautious about what they wanted to go into the refereed publications.

Harden: Why?

Kindt: I must say, I don't know, but there was this cloud hanging over the field and tons of money being put into it. There were meetings all the time. I don't know how many meetings I went to, but for some reason it was difficult getting your work into a proper format and getting it published. And I'm sure our patent just bounced around because reviewers said, "Well, I'm not sure," or, "Have they proven it?"

Harden: Given the intense need for an animal model for AIDS research, I have a sense that politics might have been involved, but that may not be correct. Do you have any other insight as to why this patent application got bounced around within the bureaucracy?

Kindt: There were people who were downright hostile to the idea of using anything other than a monkey for the study, because they said, "What good will it do to cure a rabbit?" Some people just did not want to accept the fact that other animal models could translate to the human condition.

Harden: Was that outside of NIH or inside?

Kindt: Both. It may be worth adding here that several collaborating groups were positive about our model and helped with some improvements. A group at a small biotech company produced a strain of transgenic rabbits that expressed the human CD4 gene in a tissue specific manner. Another lab headed by Mark Goldsmith [Dr. Mark A. Goldsmith] transfected rabbit cells with the human CCR5 receptor gene and showed that they were highly susceptible to HIV-1 infection.

Harden: From the late 1980s onward, your lab turned its focus to HTLV-1. Why did you choose that retrovirus, and what might you tell me about the research you did on it?

Kindt: HTLV-1 is a virus that affects 20, 30 million people in the world. However, only a small percent of them get sick from the virus. There were two diseases that came from HTLV-1 infection. One was a T cell cancer, a very aggressive, terrible cancer. The other one was a neurologic disease, somewhat like MS, with paralysis and a gradual loss of function. Now, the good questions: One, why can most people be infected and never get sick? Two, in this huge amount of people, what's going on with this virus? And it turned out that the rabbit was the best model for the study of HTLV-1, for what reasons, I don't know. But this was discovered in Japan, right when the virus was discovered. We were able to come up with some interesting models of infection with HTLV-1 in the rabbit. We isolated viruses that caused rapid fatal disease and others that mediated asymptomatic infection. In fact, we were among the first labs to isolate a molecular clone of HTLV-1 that would cause infection. You could take the naked DNA and infect a rabbit with it, which at the time was such a big deal that Bob Gallo invited me to his summer meeting with a prime time slot to speak about it. At the time I thought, "I've arrived."

U.S. Public Health Service 1990 Superior Service Award for AIDS infection model, 1991. From left: Assistant Secretary for Health, DHHS, Dr. James O. Mason; Dr. Thomas J. Kindt; U.S. Surgeon General Dr. Antonia Novello; NIH Acting Director Dr. William Raub.

Harden: In 1995, you were named Director of the Division of Intramural Research for NIAID, the position that at NIH is commonly called Scientific Director. Would you tell me about the process of how you were recruited? Did you apply? Who interviewed you? I presume that Dr. Fauci made you the official job offer since he was the NIAID director. But tell me how this all went down.

Kindt: There was an advert for the Scientific Director position, and I ignored it, quite frankly. I mean, I wasn't interested. Then Tony called me in and said, "Apply."

Harden: I see.

Kindt: And you've dealt with Tony.

Harden: Yes.

Kindt: It's hard to tell him no. So I did apply, and my application was late, but they took it anyway. There was almost a year as the applications went through committees, some from NIAID originally, some from NIH. And then, Harold Varmus [Dr. Harold Varmus, Director of NIH, 1992-99] got involved, and he decided that NIH wasn't just going to let Institute Directors pick Scientific Directors. He suggested that there needed to be a more rigorous process. He made me give a seminar to all of the Scientific Directors and those Institute Directors who were interested, and to let them question me *ad nauseum* about taking this position. It was very, let's say, rigorous.

Harden: So did you present a scientific seminar or a seminar about how you would run the NIAID intramural program if you got the job?

Kindt: He wanted a scientific seminar because Harold felt that if you weren't a class A scientist, you weren't worth much.

Harden: But then they could ask you questions?

Kindt: Yes, such as, "What would you do, doctor, if?" And then I had to answer them. Some felt that the Director should be a physician, but this was not a major issue as there was an excellent clinical director who would work closely with me.

Harden: Were they also interviewing other candidates in this way, or had Dr. Fauci selected you, but Dr. Varmus decided you needed a further rigorous review?

Kindt: Tony decided that I needed to do it. There were other people who had applied for the job. I'd hear rumors that somebody had taken the job and other rumors that they didn't. But the point was, there was an eight-month period that they were just hammering on my application. But the next year, in the summer, I was appointed.

Harden: When the application ordeal was ended, you suddenly were the Scientific Director for all these intramural labs. What goals did you have to start with for the program?

Kindt: My major goal was to clean up the program to make sure that everyone who was a Principal Investigator (PI) was well reviewed and was appropriately resourced, based on their review. I got that underway pretty soon. I also knew from personal experience that the space that was allotted to some very fine scientists was inadequate, and so I started a crusade to get more space for the institute. That was one of my major goals.

Harden: Did that result in space in Rockville—the Twinbrook Buildings, maybe?

Kindt: That's right. We had Twinbrook-2 already, and I got that renovated so that the space was decent for our laboratories. I took over Twinbrook-1, which was a smaller building next to it. It had some contractors in it. I put a lab in there, and eventually put a malaria vaccine group in there so that they could have essentially GMP manufacturing--GMP means good manufacturing practice: facilities and all materials meet standard for use in humans. It was small building, so the appropriate ventilation and other necessary utilities could be installed. I eventually got a large building built down the street on Twinbrook Parkway for the entire parasitology group who moved from campus. That was a success in expanding the space available. Now on the Bethesda campus, simultaneously, NIH was constructing Building 50, which was to have many different institutes in it with the state-of-the-art, modern space, et cetera. I had a floor or half a floor in that building to expand into for NIAID. So we were moving along, but we also had some real duds as far as laboratory space went that we had to get rid of. Building 7, where Bob Chanock [Dr. Robert M. Chanock] and his group were, was a disaster. The building was heated in the winter by the freezers. I'm not kidding. The heating went out and

nobody noticed because the exhaust from all the refrigerators and freezers actually heated the building. It was not good space for such a wonderful group of people as Dr. Chanock and his boys. I mean, they were as good as it gets, and we had them in a sty. So we got space for them.

Harden: We have reached 90 minutes, Dr. Kindt, and I think we are both tired, so I am going to thank you and stop for today. We will resume with a second sitting next week.

NOTE: This is the second interview for the oral history with Dr. Thomas James Kindt on August 2nd, 2023, about his career at the National Institute of Allergy and Infectious Diseases. The interview is being done over Zoom and the interviewer is Dr. Victoria Harden.

Harden: When we stopped, Dr. Kindt, you had just talked me through how you were selected to become the Director of Intramural Research—the Scientific Director for NIAID—and what the situation was that you found upon assuming the job, especially the lack of space, which you aimed to solve as one of your first goals. Now would you describe the organization you found when you took over. What laboratories existed? Who was running those laboratories? What changes did you think should be made in terms of research focus and personnel?

Kindt: When I started, there had been an Acting Director in the position for almost a year, and my office had an inch of dust on it. My first surprise was about all of the interactions that the Director had to have that a Lab Chief literally knew nothing about. I had to interact with the Extramural Program Directors of NIAID, with the NIH Director of Intramural Research and with the Scientific Directors of the other institutes. It seemed like there were countless meetings with all of these people. Also, I had to play a role in the NIH Clinical Center, where NIAID had a fair slice of activity. We had an excellent Clinical Director named Clifford Lane [Dr. H. Clifford Lane]. I met with him once a week when I took over as NIAID Director of Intramural Research. That was another interaction, and it led to the necessity for me to get some additional education. I took a course in medical ethics. I went on rounds a few times with the group and would speak with Cliff every Friday morning. Every Thursday afternoon, I met with the Executive Committee of NIAID, which included all the Directors of extramural programs and all the branch heads; the heads of personnel, and finance; the NIAID Deputy Directors, et cetera. At that weekly meeting, I was expected to give them a picture of what was going in the Division of Intramural Research and sometimes answer questions from them.

On every Friday at 4:30 pm, I met one-on-one with Dr. Fauci. And believe me, these weren't just friendly chats. Dr. Fauci's incisive questions led me to prepare for these meetings and usually have a list in my hand of what might be of interest to him. I should say, he was an excellent boss. He always had my back if I did the right thing. He had told me when I first started, "Never

do anything unless you wouldn't worry about having it on the front page of the *Washington Post* the next day." That was very good advice from Dr. Fauci. Other people also gave me advice. Some of it was amusing, and some of it was useful. Dr. Krause had told me, "What you must learn is to point with pride and view with alarm when you're reporting about your activities." That was interesting, too.

I knew about the interactions with the labs because I'd been going to Lab Chief meetings for 20 some years. And I was aware of the review process for labs, having gone through it for my own laboratory maybe five times by then. However, the exact role that I had to play with the laboratories and with the review process wasn't completely clear as to how detailed it was. But having to review personally the written submissions for labs under review, having to choose reviewers for the Board of Scientific Counselors (the board of outside advisors that conducted the review), and having to keep them up to date and appoint people when appointments expired—all that was a lot of work. Eventually, after I'd been in the position almost a year, I chose a Deputy Director. One of her primary charges was to take over the work with the Board of Scientific Counselors.

Harden: Who was your Deputy?

Kindt: Dr. Karyl Barron [Dr. Karyl S. Barron]. Karyl was a scientist and a physician, mainly interested in rheumatoid diseases. She was not terribly active in the laboratory any longer. She had some clinical interest, but she primarily served as my Deputy for the entire rest of my time as Scientific Director. And her major responsibility was dealing with the review process and with the counselors.

Harden: All right. Tell me about the labs.

Kindt: We had 120 Principal Investigators spread out into maybe 16 laboratories. Each one of these investigators had to be reviewed every four years and their resources allotted according to the review. And not only were they allotted according to the review they received, but the allocation had to be reviewed also by Dr. Fauci as NIAID Director and also by Dr. Gottesman [Dr. Michael M. Gottesman], the NIH Deputy Director for Intramural Research. So this was a stepwise process, which included reviews from outside, then our response to the review. Dr. Baron and I would interview afterwards every single investigator who had been reviewed and get their take on it before we wrote up our final comments about what the reviewers said. It was an involved process and rather time-consuming.

Harden: Did you have any sense that certain lines of research should be supported more strongly and some not so much?

Kindt: Absolutely “yes” to that question. One of my first actions was to create a Laboratory of Allergic Diseases. The Institute of Allergy and Infectious Diseases had, maybe, one section of the clinical lab that studied allergy. This situation was criticized often by the reviewers and by others. We had an excellent allergist, Dean Metcalfe [Dr. Dean D. Metcalfe], whom I made a lab chief and gave additional resources to, in order to strengthen this program. That was one of the first things that I did. And I'm glad I did it, because shortly thereafter, I was called to a meeting with certain allergists from Hopkins [Johns Hopkins University School of Medicine] and from downtown G.W. [The George Washington University School of Medicine], and they wanted to know what was I going to do to strengthen allergy research at NIAID. And I very happily told them that I already had. “Yeah, I'm on top of it. I started a new lab.” That was a good move to begin my tenure with. I also wished to strengthen the clinical lab and appointed some new leaders, including Steve Holland [Dr. Steven M. Holland], with expanded programs.

Another thing that I did was creating the NIAID division that Dr. Hohman [Dr. Robert J. Hohman] took over, the Research Technologies Branch. In 1982, when I was preparing to spend a sabbatical year at the Institut Pasteur, Bob Hohman was preparing to do a postdoctoral year there. We met in an evening French class at the NIH. His comment was, "It's you and me and a bunch of nurses that are going on vacation and trying to learn a little bit of French.” We also both learned a little bit of French, which got us started in Paris. After we both returned to NIH, Bob did further postdoctoral work and then became the principal scientist in a little company in Gaithersburg, MD called Oncor, Inc., and based on our connections, Oncor hired me as a consultant. So Bob and I stayed in pretty close touch. And then in 1995, when I became NIAID Scientific Director, the facilities were kind of ragged, as I have already discussed. Some labs had one instrument, some had another. I finally got all the technologies consolidated into the Research Technology Branch for the entire NIAID intramural program. This worked much, much better to enable the laboratories to get the technology services they needed.

Harden: Were there any labs that you needed either to eliminate or downgrade?

Kindt: Unfortunately, I could not go through and eliminate people or labs. I had to concentrate on the review process to weed out by reducing resources. I certainly knew there were some people who were subpar.

Harden: Was there any concern about increasing the number of women or minorities in senior positions? There may be others, but I note that under your tenure, two women were promoted to senior positions. Dr. June Kwon-Chung was named a Section Chief. And you hired Dr. Susan

Pierce to take over as Chief of your former Laboratory of Immunogenetics once you were swamped with administrative duties. Tell me about women and minorities at this time at NIAID.

Kindt: I spent a large amount of time dealing with these questions. The question of hiring women, promoting women, was not always straightforward. Several of the women with whom I discussed this wanted no part of additional responsibilities. They would say, "Oh, no. No, I don't want to be a Lab Chief. I don't want to do that. I have my own obligations. I'm happy with what I'm doing." And as you know, there are very bright women scientists, Patti Rosa [Dr. Patricia A. Rosa], for example, at Rocky Mountain Laboratories (RML). She is a stellar example of a good scientist, and we promoted her as much as she wished. So let's say that promoting women was a little bit difficult. And as far as recruitment, we welcomed any woman, any female applicant, to any of the positions. And again, one has to be careful. We could only take the very best candidate among the applicants for the positions that opened.

Now as for minorities, we had started, in our institute, a yearly program, a several-day meeting with a group of young, minority scientists. “Introduction to Biomedical Research,” I believe we called it. Dr. Richard Asofsky [Dr. Richard M. Asofsky] had taken that over, and I made him an Associate Director for these programs. Unfortunately, not too long after he took over, Dick developed terminal cancer and died. I found another person, Wendy Fibison [Dr. Wendy J. Fibison], to take over the program. That program gave us some recruits, and we got fellows from that group, so it wasn't a complete bust.

However, trying to get more senior minority scientists was a battle. I will relate to you a meeting I had with the Dean of Howard University Medical School. Howard is the medical school in Washington with primarily minority students. I met with its Dean and a couple of his associates, and I told them I was willing to go all the way to recruit from their ranks, but they essentially told me, "We don't want our recruits to go into the basic science track because when they go to the next step, they won't get funded, they won't get good jobs, they'll get poor-paying academic jobs. This is not a good career path for minorities." They had much rather have them become practicing physicians. They will be M.D.s, and then their mothers could point to them and say, "There's my son, the doctor, serving the community." That really took a lot of the wind out of my sails when I realized they were right.

But we tried, never very successfully. We nurtured some minority scientists along until we could no longer do so. They couldn't pass the review processes. About 2000, I had to write up a very detailed report for the Blue Ribbon Panel that reviewed the Division of Intramural Research. In fact, this could almost go as my legacy book. It's incredibly detailed and answers so many of these questions. But part of it was showing how many minority graduates there were in the pools from which we recruited, and what percentage of people we had. And they just weren't there.

In addition, there were 25 pages of supplementary material requested by the Blue Ribbon Panel. Good God, this was a tremendous undertaking! But this is the supplementary material that they requested, and they wanted to know details on the space and recruitment efforts and reviews, and how we responded to the reviews. I found this in my old boxes just a couple days ago, and I thought that it could answer a lot of your questions in great detail. In more detail than you want, I'm sure.

Harden: My former office, the Office of NIH History and Stetten Museum, will be very interested in that document. They will contact you separately about obtaining your copy for their library. As the NIAID Scientific Director, you had less and less time to spend in a laboratory doing research and more and more administrative responsibilities. I see in your CV that you sat on all sorts of planning, oversight, and advisory committees. I would like to hear about other major challenges you had as a scientist-administrator.

Kindt: I kept my lab going as long as I could. Initially, I had my laboratory at Twinbrook. I'd go there early in the morning. I'd spend time till just a little before lunch, and then I would drive down to my office in Building 10 and conduct the administrative business from there. And I laugh recalling that I thought my lunch was the best "dashboard dinette" because I would gulp my lunch as I was driving down.

I finally got the office better organized, but this took some push-pull with the Administrative Branch of NIAID, because they considered themselves answerable only to the NIAID Deputy Director, to their Branch Chief, and so on. I had to explain to these people that they were serving the Division, not themselves. We eventually developed a group of people who were sincerely interested in helping us. Then I had a good finance person, a good personnel person, et cetera. Then we would meet once a week, this administrative group and myself, and hash out what was going on in the division and what was coming up that would have an impact on it. Even though they felt they were all tied in, they weren't, because they didn't hear what went on at Executive Committee meetings. Their Director was there, but they didn't meet with their Director often, as far as I knew. So at any rate, I got that under control, and it worked very well. I was able to have some of my choices appointed into the group—for example, Beth Schmidt [Beth Schmidt] as office assistant and Judy Quasney [Judy Quasney] as a consulting architect. We would sit around the table weekly and hash issues out, each one stating their piece of the activity. Pat Stewart [Pat Stewart] and Marshal Bloom [Dr. Marshall E. Bloom] from RML were included by teleconference.

Harden: What about the NIH-wide committees set up when the Scientific Directors from all the institutes met with Mike Gottesman?

Kindt: The Scientific Directors meetings, which took place monthly, were quite involved. There was a lot of information and a lot of discussion about personnel matters. A lot of it was based strictly on history, as in, "We've always done it that way. We don't need to change." And some of that was pretty shortsighted. I remember fighting with the group about salaries for postdoctoral fellows. I said, "Why don't we just increase them with inflation like Civil Service salaries?" Oh, no, they couldn't do that. They had been fixed at a sum, and that number stayed the same until somebody went crazy at us and showed what the poverty level was in Montgomery County, Maryland, compared to what we were giving our fellows.

I was head of one of the larger intramural programs and so had a little more voice in these discussions than some who headed up the smaller programs. Based on this, I was appointed to a number of review committees for the program in general. There was one, when Arthur Andersen & Co. came in 1997 to look at the administrative practices that had an impact on the intramural program. This was before Arthur Andersen & Co. collapsed in 2002 as a result of its activities during the Enron scandal. Before that, there were a number of smaller committees that I sat on and tried to give rational input. Sometimes this worked; sometimes it didn't. I can't remember all of them. We are looking back too many years. I spent a lot of time on these committees, and on some of them, I think we made some positive changes. On some others, business as usual triumphed.

Harden: Even before the terrorist attacks of September 11th, 2001, with the collapse of the Soviet Union and the 1990-01 war in the Persian Gulf, NIAID and the rest of NIH, the CDC and the FDA [Food and Drug Administration] as well as the military and the White House, were all worried about the possibility that biological or chemical weapons might be used once again. And then, when the 9/11 attacks were followed closely by the anthrax letters, NIAID was tasked with research in this area to protect U.S. citizens. You were a member of the Blue Ribbon Panel on bioterrorism and its implications for biomedical research. You were also closely involved with the planning and construction of Building 33, the C.W. Bill Young Center for Biodefense and Emerging Diseases, as the award on your wall shows. Would you tell me about this time at NIAID and in the broader government?

Kindt: Yes. Let's back you up one second. As a historian, you'll remember the 1995 attack on a government building in Oklahoma City.

Harden: Yes, indeed. Those of us who worked in Building 31 were horrified because Building 31 looked just like that building [the Alfred P. Murrah Federal Building]. A car bomb would have taken us all out.

Kindt: That incident, I think, was a bigger turning point for NIH than we had seen previously. We changed our security protocols drastically after the Oklahoma City bombing. We had to have identification on people. Some of our buildings were more restricted, where we used agents that might be considered dangerous. Then, with the cooperation of Dr. John LaMontagne [Dr. John R. LaMontagne], who was NIAID Deputy Director and a microbiologist of national and international repute, we started thinking about increasing the ability of the NIAID to deal with emerging diseases like Ebola or re-emerging diseases like tuberculosis or select agents of possible bioterrorism. Very quickly, Dr. Fauci reasoned: "Wait a minute, guys. The recurring diseases and the bioterrorism agents, they're the same. One is mother nature throwing something at us, the other comes from the terrorist. Let's think about combining our efforts to think of these as one challenge." That was, I think, a very astute thought on his part. I think it was brilliant.

What we did was to start looking for possibilities in each of our buildings. Now, I personally had always worked at the BSL-3 level [Biosafety Level 3] on my HIV and HTLV-1 animal models. Working at BSL-3 did not require a separate facility, but it did require a laboratory facility that was above and beyond the normal, with appropriate air changes, et cetera. But we didn't have in the institute even one adequate BSL-3 facility that would handle waste disposal and air handling for lethal airborne pathogens. I remember Dr. Varmus asking us, "Well, we've got a bunch of them, don't we?" He was mistaking the BSL-3 practice labs with BSL-3 structural physical labs. There was maybe one place in the institute, up in the Clinical Center, where you could work on drug resistant tuberculosis. Only one.

So we then worked pretty hard at getting more facilities. The BSL-3 lab at Rocky Mountain Laboratories, which was a building that had been on construction lists for many years, was finally built. There was some fighting about that, but they let us do it. That building was built before the anthrax scare and before our full court press on select agents. But after the anthrax letters, the President and others decided that we should have facilities at the BSL-4 level, and we start designing and building one at RML at that higher level. That was very nice because we could piggyback it onto our existing BSL-3 facility at RML with an added BSL-4 facility. There was also to be one at our labs at Fort Detrick in Frederick, Maryland, in conjunction with the Army. We would have BSL-4 facilities at those two places. Not on the Bethesda campus. We had one little BSL-4 facility in Bethesda—it was a little building off to the side of the campus. Deb Wilson [Dr. Deborah E. Wilson], who was the head of Biosafety at NIH then, worked with me to redesign that little facility so that someone could go in with a protective suit on, and use the small lab area available for doing research with dangerous agents.

Harden: Where was it located on the campus?

Kindt: It was toward Battery Lane on that side of the campus. It looked like a little carwash—it was a little building.

Harden: Do you know if it's still there?

Kindt: I imagine it is. I don't think anybody tore it down.

Harden: Please go ahead. I just didn't know anything about that building.

Kindt: Well, with Deborah Wilson and a small group from the Safety Branch, we got BSL-4 suits. And we got that building set up so that if someone came to campus with a potentially dangerous package, we could bring it into that facility, either through a dunk tank or some way, to isolate it in that facility. Then someone could put the suit on, work on it there, and then shower out.

Harden: But it was not used as a general lab?

Kindt: No, no. One of the interesting things that happened about the building is that people from the FBI came to see us and decided to put that building on their list so that if they got something they couldn't deal with, they could bring it out to us. And of course, we thanked them profusely for this. It was probably used for some tuberculosis samples. But there was never active research in it.

Harden: Do you know if the Army had any other BSL-4 facility besides at Frederick?

Kindt: I do not know if they did. The Army Medical Research and Development Command was at Frederick. They had facilities, and they had stretchers. You could take patients with dangerous infections and transport them safely. And they had a little hospital up there, where if you got infected, they'd send you up there to their BSL-4 clinical beds. We shared some research space with them when we were building our BSL-4 lab at Frederick. That was part of the whole program.

Another part of the program involved convincing the local communities that it was okay to have BSL-3 or BSL-4 labs in their midst, which Dr. Fauci gave me as my charge. I had several meetings with people in Bethesda. And there would be people in the back of the meeting shouting, "Send it to Plum Island. We don't have to do it here." And things like that. They thought that they were knowledgeable about research and about these agents. Meetings with the community at

RML was a whole ‘nother story. We had several meetings with the community, some of them positive, some of them a little acrimonious. But I guess that's to be understood.

Harden: In his oral history, Marshall Bloom talked about those meetings. But at least out there, they had more physical space with fewer humans in the area.

Kindt: Yes, but the local community didn't want to hear that. I would say that my interactions with the Rocky Mountain Laboratories were much more frequent than those of other NIAID Division Directors. I tried to go there every couple of months and spend a number of days. I made sure that all the reviews of the labs were done there, and I was there for most of them. Dr. Fauci was also there. We tried to make RML seamlessly a part of the intramural program. With respect to the community interaction necessary when we built the BSL-3 and BSL-4 facilities out there, it turned out that Dr. Marshall Bloom was such a respected member of the community and a notable fly fisherman that they accepted him very readily. He became the NIAID Associate Director for the Rocky Mountain Laboratories—de facto serving as a Director but not given the title “Director, Rocky Mountain Laboratories.” That was because we never made Rocky Mountain independently budgeted, independently reviewed as it had been early in its history. When there was a separate Director, they didn't want much to do with Bethesda and wanted to set their own course. But Ken Sell and Richard Krause wanted to change that, and they did. They worked toward it, and I continued to integrate it into the NIAID as a whole. I think it was much stronger for doing that.

Harden: What else can you tell me about Building 33?

Kindt: Building 33 started off as a piece of paper in my files. I wanted a facility where you could work at least at true BSL-3 on tuberculosis, on anthrax, and on a number of other agents that people were already working on but couldn't work at the higher level. With Dr. LaMontagne's support, we got this project moved up the list of priorities for NIH. And when President George W. Bush gave us a bundle of money—I think I got $190 million to do this—we sat down and started planning.

It was an exciting planning process because instead of going step by step, they brought in people from each construction division, HVAC people, plumbing, structural people, et cetera. I can't remember all the different people. And the police were involved. We had NIH Security people—they were very important. We had to harden the building so that if somebody came with a backpack full of explosives, the building could withstand a hundred pounds of explosive. And we had to build it so you couldn't drive a car up to it and blow it up. It was a very exciting time. We put this together in frequent meetings, and before you knew it, we were breaking ground and moving right along.

Harden: It was not quite finished and ready for labs to move in when you retired and turned things over to Kathy Zoon [Dr. Kathryn C. Zoon]. In her oral history, she talked about where things stood when she took over. Would you compare where the intramural program stood in 2005 when you retired with where it stood when you became Scientific Director in 1995?

Kindt: Well, let's start with the budget. I believe I began with a budget of around $120 million. And as I left, the budget was $300 million. That's a fair hunk of change when you start moving it around. Over those 10 years, we acquired quite a bit more space. More importantly, we had space to do all of the infectious disease research we wanted to do. And as you probably know, the facilities have been well-used.

Now, why did I retire in 2005 right in the middle of this huge project? I was to have a beautiful office in Building 33 and so on. Two reasons. One, family. My wife's mother was in New Mexico and not doing too well, and my wife was spending more time there than she was in Bethesda. Secondly, I was getting fed up with dealing with the Department [Department of Health and Human Services, DHHS] and with other people on top security issues. I can't tell you the pressure and the things I had to do. I would have to go to a meeting at a non-disclosed location for example. I had a top secret clearance for seven years. And the reason I had to get the clearance was not for in the NIH, but for dealing with the Army and for others.

In addition, I went to South Africa, to the facility there where they had the other smallpox virus. I went to Rome, to their facility where they had a hospital for BSL-4 patients. I traveled all over and met with all kinds of people, which wasn't too terrible. What was bad about it all was dealing within the Department and having to go downtown and speak with persons who frankly didn't know what the hell they were talking about. It just got to me. It was not my job, and not what I intended my job to be. I am not a bureaucrat. I did not identify as part of this group. And that's really the reason I retired. On top of it, I'd been to New Mexico a couple times, and it really looked nice. I bought a lot there and started to build a house.

Harden: That answers my next question, because I know after you retired, you were on the faculty at the University of New Mexico in the Department of Biology. And now you're in Scottsdale and I was trying to put all this together as to why New Mexico when you seem to have been in Scottsdale a long time. So why don't you give me an overview of what you have done in retirement. I know you became the Chief Scientific Officer for two different biotechnology companies, InNexus and Diomics, while also writing a textbook. You were a busy retiree. Tell me about it.

Kindt: I was a very busy, and at first a very happy retiree. I worked in New Mexico. We moved to a house that my wife and I designed, in the foothills of the Sandia Mountains, with incredible views and a nice place to be. What I did there first was to write a complete new edition of our immunology textbook, [Kindt, T.J., Goldsby, R.A. and Osborne, B..A. *Immunology,* 6th Edition*.* New York: W.H. Freeman and Co., 2007] Among the authors, it was my turn to do a revised edition. I would spend mornings mostly at my desk. Afternoons, I would go to my workshop. I built a number of pieces of furniture, inside and outside, for my house. And I did a bit of photography. In fact, I had done quite a bit of photography, although I didn't talk about it earlier. When I was at NIAID, I made three trips to Africa that included safaris. So I have 5,000 animal pictures that someday I'll organize. There are some on the wall over there. Just some of my favorites.

Harden: Wow!

Kindt: I registered something like 55 species at one point, that I had photographed. At any rate, so those were hobbies, and I could continue them in New Mexico pretty well.

I also started consulting for a law firm in Palo Alto. I was an expert witness on a trial that went on for almost a year, between two big drug companies, about intravenous immunoglobulin, an area with which I was familiar. I have to say, it was enjoyable and different to work in the law firm. They treated you like a special person. I mean, "How do you like your coffee, sir?"

Harden: Unlike the government.

Kindt: Unlike the government. “The driver will pick you up from the airport and bring you down.” There were a lot of nice things about consulting, and the salaries weren't bad either. But at any rate, I commuted, New Mexico to Palo Alto, for almost a year. And then I was on a scientific advisory board for the company, InNexus.

What happened around that time, and very unfortunately, was that my heart valve started to give out, and I had trouble doing almost anything at the high altitude of my New Mexico house. This was a tragedy of my life when I finally realized that I shouldn't stay there. I had trouble taking my routine walks. I was someone who had been hiking all over the mountains there and really loved it. But then I'd be short of breath just walking down my driveway and up the hill. So we got a house in Arizona, near the company InNexus, which was on the same grounds as a Mayo Clinic facility. I went back and forth for a while, but after a while, I decided that I couldn’t keep this up. Being in New Mexico was a burden.

They offered, in New Mexico, to cut me open and do a valve replacement. And after reading up on it, I realized I would have to be on immunosuppressives the rest of my life. And I didn't want to do that, knowing too much about it. So I blew that off and moved to Arizona where I've had about 10 procedures, ablations and valve repairs, but not replacement. So I'm happy enough now with it. It's not good. But it's okay, even if leaving New Mexico hurt.

So I went to work for the company InNexus, which was developing cancer drugs. This was interesting work. I learned that we went to the point of developing a pre-IND [Investigational New Drug, an FDA category] for a drug. And, again, working in a company was so different from working in a government. I have to tell you, I fired three people who just were incompetent. And I could just fire them. I'd just go the CEO [Chief Executive Officer] and say, "This person is incompetent," and he was gone. It was wonderful that way. And I built a little team there of very good people. Unfortunately, that company got caught in the Great Recession. The people who were funding us were backed by an Irish trust, and Ireland got hit harder than the United States in the 2007-2009 and beyond worldwide recession. And so the company folded. That was too bad.

Harden: What about your work with Diomics?

Kindt: Diomics came up right behind the work with InNexus because one of the people that I had dealt with was a professor at ASU [Arizona State University], in material science. He had developed a material that was better than cotton for picking up DNA samples. We would do experiments where we'd put a microliter of blood on a slide, pick it up with a cotton swab, or pick it up with a Diomic swab, extract the DNA, and we'd get much larger amounts of DNA out of the Diomic swab because cotton didn't give it up very easily.

At any rate, I stayed with Diomics for a few years, and then there was a double tragedy. The CEO of the company, who became a close friend of mine, was an amateur pilot. One of the things I liked about the job was that we would fly to different sites together in his little plane. I really like to fly on little planes, especially flying over the Four Corners region here and flying down south. We went to San Diego a number of times; we also went to Colorado and Texas. Unfortunately, on one of his flying trips, the plane crashed. He also had one of his children with him, which made it a double tragedy, as they both were killed. After this, the people who were funding the company were much less enthusiastic, and eventually, the company just folded. It left a very nice portfolio of patents, for which I had been the major driver in writing and filing. But the patents led to fights over who got to benefit from them, and nothing ever really happened. I would even have to look in the patent office to see which ones actually survived.

Harden: Would you talk a bit about NIAID intramural efforts in other countries?

Kindt: I should note the number of trips to Africa I made. I went to South Africa a few times, and we tried to set up an AIDS program there, but some South African politicians and their advisors were really against it. One of the leaders there thought it was Western medicines that caused AIDS, and he popularized that idea. But with Cliff Lane, we tried to get a program started with the South African Armed Forces, probably the most organized group in that part of Africa. We succeeded to some extent and also had a pretty good tuberculosis program going in South Africa.

Mali, now, is a whole 'nother situation. You could go crazy, historically, with Mali. Timbuktu was a site of Islamic academe. There were libraries there, and it was a terminus of the caravans. So in Mali, there was a proud history of Islam and the Muslim people. And for that reason, their AIDS numbers were very low. There was no institutionalized prostitution in Mali as there was in Kenya and some of the other countries. But they did have a lot of tuberculosis and other diseases. And malaria—they were right in the middle of the malaria belt. Lou Miller's [Dr. Louis H. Miller] lab, mainly Bob Gwadz [Dr. Robert W. Gwadz], started a facility there and had hired a scientist to run it. Dick Sakai [Dr. Richard Sakai] was his name. The facility worked very well and NIAID funded it on a regular basis, so we were able to provide water, good water, to the lab. We were able to provide electricity on a constant basis, which is necessary for any lab work. And we had a little farm of generators run by a gentleman who actually had three wives, I remember. He was a nice guy, and he oversaw the generators so that the lab was never without electrical power. Our group brought in people from NASA (who had means to track mosquito movements), and people from different universities, both in Europe and in the United States. And because I could speak at least elementary French, a lot of the faculty members at the university in Bamako would confide in me and talk with me. It was a very good lab. I hesitate to tell you what's happened to it now because of the terrible civil unrest situation in Mali. It's terribly sad.

Finally, I was in contact with the Institut Pasteur in Tunis. I had begun some contact with Tunisia when I was at the Institut Pasteur in Paris, and then I was appointed to the board of external counselors for the Institut Pasteur in Tunis. This institute is quite interesting. It was organized according to diseases throughout the country. They had to address diseases like rabies and scorpion bites, which we didn't see in the United States. Scorpions—scorpion envenomation was a serious public health problem. I went there once a year to participate in the review of the various labs. That worked very well, and I have to say, the food was outstanding! The fish dishes in Tunisia are to die for.

Harden: Thank you, Dr. Kindt, for an excellent oral history.