NATIONAL HEART, LUNG, AND BLOOD INSTITUTE ORAL HISTORY PROJECT

INTERVIEW WITH

Edward D. Korn

JANUARY 15, 2019

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Edward D. Korn

Biographical Statement

Dr. Edward Korn was born in 1928 in Philadelphia, Pennsylvania. He received his Bachelor of Arts degree in chemistry (1949) and a PhD in biochemistry (1954) from the University of Pennsylvania. As a graduate student he was awarded fellowships by the University of Pennsylvania and the Damon Runyon Foundation. He began his career at NIH in 1953, when he joined the new intramural research program of the National Heart Institute, now the National Heart Lung and Blood Institute, working in Building 3. The NHI staff included Chris Anfinsen, Bob Berliner, Earl and Terry Stadtman, Jack Orloff, Martha Vaughan, to name a few, with James Watt as Director and James Shannon as Scientific Director. Of this original group, eleven have been elected to the National Academy of Sciences, two became Nobel laureates, and three became NIH directors. It was out of this creative, heady atmosphere that Dr. Korn began his research, first in the lab of Chris Anfinsen working on the hydrolysis of lipoproteins. Then moving on to his own work and lab, he has continued at the NHLBI for sixty-five years. Since his groundbreaking discovery of the first unconventional non-filamentous myosin, myosin I, in the single-cell soil protozoan *Acanthamoeba castellanii*, Dr. Korn's research has focused on the function and regulation of the actomyosin system. Since 1974, he has been Chief of the Laboratory of Cell Biology and served for ten years as Scientific Director of the NHLBI. He has authored or coauthored more than 260 peer-reviewed papers and more than sixty book chapters and edited books. He has served as an associate editor of the *Journal of Biological Chemistry*, served on numerous editorial boards, and is a member of the American Society of Biochemistry and Molecular Biology, the American Society of Cell Biology, and the National Academy of Sciences.

Interview Synopsis

Dr. Korn begins his interview with memories of growing up in Philadelphia and his undergraduate years at the University of Pennsylvania. He recounts entering graduate school at Penn in the physiological chemistry department (precursor to biochemistry), and choosing to work with Jack Buchanan. He discusses his postdoctoral possibilities and how and why he ended up at the National Heart Institute. Dr. Korn worked with a group of scientists in Building 3 during the 1950s and 1960s, a group now referred to as a "research dynasty" and a period now called a "golden age" at the Institute. Dr. Korn discusses at some length the dynamic, supportive atmosphere he found at the NHI as well as some of the colleagues he worked with and the positive impact James Shannon had on the basic science conducted and the Institute in general. He also describes many of the changes he has seen at the NIH and in the community of Bethesda over the last sixty-five years. The second half of the interview focuses almost exclusively on his scientific work. First, he recounts the colleagues and procedures of his work in Anfinsen's lab with lipoproteins. And, then, more extensively, he explains his lab's discovery of myosin I and what became his life's work—the study of myosin and actin. Dr. Korn also discusses his tenure as scientific director of NHLBI and some of the challenges and contributions he experienced in that role. He speaks about his wife Mickey, his partner of sixty-nine years, their two daughters, and decisions he made along the way that made for a long, successful career at the National Institutes of Health and a fulfilling life.

NHLBI Oral History Project
Interview with Dr. Edward D. Korn
Conducted on January 15, 2019 by Sheree Scarborough

SS: The following is an interview with Dr. Edward Korn. I'm at the National Heart, Lung and Blood Institute, NIH Campus, in Bethesda, Maryland. This is Sheree Scarborough and today is January 15, 2019. Dr. Korn, please say a few words to me just so I know that the microphone is working.

EK: Okay, you have the right day and the right date.

SS: Good. And I'm in the right place.

EK: And you're in the right place.

SS: Thank you. So this is a career-long—a lifelong review, really. I'd like for you to talk, if you would, a little bit about your family background and growing up. I know you were born in Philadelphia. Is that right?

EK: I was born in Philadelphia ninety years ago.

SS: Congratulations.

EK: Thank you.

SS: Not all of us are that lucky.

EK: And I have an older brother and a younger sister. My older brother is still alive but he doesn't know it. He's got dementia. My sister died a couple of years ago, so that I'm now alone. My father worked for an insurance company and my mother did not work. It was then called Connecticut General Life Insurance Company. It's now some other name, which I should know, but I don't remember. And he never was able to rise above the position of assistant manager of the Philadelphia office, because he was a Jew, and he was told by the president of the company that if he were not Jewish, he would be in Hartford, Connecticut, which was their home office, as a vice president, but that was the way things were in those days, and to some extent still are.

But anyway, I went through high school and college in Philadelphia. I went to high school at Central High School, which was an all-academic college preparatory high school, male only at that time. About ten, fifteen years ago, it became co-educational. There was also a girls' high school in Philadelphia, which, again, you could come from all over the city to Central or Girls' High. That was, again, all-academic. I don't know whether that's co-educational now or not. I had a normal growing-up, nothing unusual.

SS: So you were born in 1928, right?

EK: Yes.

SS: So your first ten years, I guess, was during the Great Depression. Do you remember much about that aspect?

EK: No, my father had this position as assistant manager. Well, I'm not sure he was then assistant manager, but he had a secure position, and so I don't think we really suffered. A lot of his friends were lawyers, were unemployed lawyers, but we, as a family, I'm not aware, I don't remember any problems really at all. I do remember Prohibition, because my father used to brew his beer in the bathtub. But anyway, I was not part of that.

So my brother was about four years older, four and a half years older, was a outstandingly good elder brother, included me in all of his games. We would play ball in the street. In those days our milk and mail were delivered by horse-drawn carriages. And the horses were very intelligent. They knew where every stop was. The milkman would get out with his milk for six houses, and not every house in a row, but six houses. And he would go from house to house and the horse would end up where he was going to have to get back into the wagon.

SS: Amazing.

EK: And the mail, the same thing. There was much less automobile traffic. We used to play in the streets, touch football and baseball games, and we'd have to clean up after the horses, but otherwise we could play with no problem. And so, my brother included me. When he was twelve and I was eight or something, he would include me with his friends in the games they played. And when it got cold enough, we would go ice-skating in Fairmount Park, I guess, is where it was. There was a pond that was there that would freeze over and he would take me along with him ice-skating.

SS: That is a nice brother. What is his name?

EK: Stephen. My sister's name was Leah.

SS: And I don't know Philadelphia very well at all. What area were you growing up in?

EK: In West Philadelphia. Well, it was West Philadelphia, but it was a neighborhood called Overbrook, not called West Philadelphia, which was southwest, really. We were north, northwest, and it was called Overbrook. And we lived in two different houses in Overbrook. When I started high school we moved to a totally different area, North Philadelphia, north of Olney Street, Oak Lane. It was towards Montgomery County, which surrounds Philadelphia, same name as the county here that we're now in. But it was north of Olney, which was an area, and east of Logan. It was Eighth and Chelten. It was Eighth Street, which is east of Broad Street. So that was close to Central High. I was able to walk to Central High or take a bicycle to Central High, whereas where we lived in Overbrook it was a long trip by bus. I don't think we moved there because I was going to Central, but I'm not sure why my parents moved there. It was a bigger house than we had before.

My grandmother, my mother's mother, lived with us for a little while when I was young, then she got to be too much to take care of and went to a nursing home, as they were in those days.

They were just terrible. You go into them and you would smell all the urine, and they were not taken care of very well, but it was the best you could have done. She lived for quite a while, actually. I was married when she died, I guess. My only wife, current wife, was at her funeral, so we were engaged if not married by that time. And my mother had one brother whom she talked to almost every day on the telephone. My father was, I think, the ninth of ten kids, one of whom didn't survive at birth, died shortly after birth. And he didn't know his father because his father died when he was about two years old. And his mother died when I was about two years old. My sister's named after her. I think she was conceived because they wanted a girl to name after his mother, to whom he was very devoted.

SS: But both your parents were born in—

EK: In Philadelphia. Their parents were not.

SS: They came from—

EK: Latvia. My mother's parents from Latvia and my father's from, I guess, what is now Austria. They came long before the Holocaust and long before Hitler. My parents were born in the late 1800s, and they were born here.

SS: Earlier immigrants [to America], then.

EK: Yes.

SS: You mentioned the Jewish faith. Were you practicing? Did you go to a synagogue when you grew up?

EK: I did as I grew up. As soon as I got independent of my parents, I stopped. I haven't been to synagogue—well, I've been there for funerals and bar mitzvahs of other people, but our children, we have two daughters, had no religious background at all. They knew the history of their family but they didn't and don't know much about religion.

SS: Is that because you're a scientist?

EK: No. It was long before I was a scientist. I was bar-mitzvahed and went to Sunday school until I was, I don't know, sixteen years old. But certainly after I started college [I stopped going]. My parents, they were reformed Jews, and they didn't go to synagogue, except for the high holidays on Rosh Hashanah, Yom Kippur. And we did have a Seder at our house where my mother's brother and his wife and daughter, only daughter, came, and an uncle who had been married but was separated for years. I never met his wife. He didn't get divorced, because he remained an Orthodox Jew and didn't believe in divorce, and they had one daughter who I knew quite well. She was older than my brother, maybe eight, ten years older than I. But we saw her quite a bit. She would come for dinner at the house and that sort of thing. But otherwise, I went to school, I had friends. There was a high school fraternity I belonged to whose name I don't remember.

SS: Did you have favorite subjects in high school?

EK: No. I didn't really. I don't think I had any favorite subjects. When I started college, I started out as an economics major. Actually, when I started was in 1945. That's just before the war had ended. My brother was in the service, in the Air Force, and as I said, as I was growing up, he included me in everything and I could more or less keep up with him and his friends, and I didn't know why I couldn't join the army also. If he could join the army, I could join the army. But since I was obviously much younger, I couldn't do that. But I went to the University of Pennsylvania, where he also went. And my father went there. My father was the first person to go to college in his family, and my mother did not go to college, and her brother did not go to college.

But my brother, as many people, he was a freshman in college when we got into the war, Second World War, obviously. And they then had all the college students presumably join some college reserve, supposed to keep them in college while they would owe time to the army. But then in the spring of 1942, they called all those people in. I can remember them all marching from the campus, Pennsylvania campus, down to the railroad station. And they got out of the army when the war ended in 1945, and my brother came back to college. So we were in college together and we had some classes together because he was delayed. He was going to graduate in 1945, but he graduated in 1948. And every class had a slogan, and their class, because it was full of people like him, was "Graduating a little late, Pennsylvania '48." I graduated the next year in 1949 from college.

Anyway, while I was in college, I started as an economics major and I actually had a class with my brother in economics, and that class, for some reason, turned me off. Unrelated to my brother, I'm sure. So I decided to become a chemistry major.

SS: What made you decide that?

EK: I had taken the required science courses, and I guess I liked the chemistry course enough that I decided to do that. I'm not sure now why. But as a result, I hadn't had all the courses required the first year to be a chemistry major, so I spent an extra semester as an undergraduate. I was nine semesters in order to get all the courses so I could be an honors chemistry major. And my grades in college were not particularly good. I was a B student, very few A's and very few C's.

SS: Was it a concern early on, not later—but before the war ended, was there concern among your peers and yourself that you'd have to go? Or your family members were there and you were concerned? I'm just wondering what the feeling was on campus with the young men especially.

EK: I don't remember that there was really much. By the time we were on campus, I was nineteen. Really, the war was almost over. It was ending. Through high school, there was that situation, but I graduated high school in 1945 and went to college in July 1945, and so the war was essentially over. The only thing was that because of the possibility that the war would not end, colleges were running three semesters a year. They ran all year, essentially. And so, I graduated high school in June, I started college in July, right away. And so, although I was there for nine semesters, I still graduated in June because I had three semesters the first year and then they went back to a two-semester basis.

So I applied to graduate school, and I was a chemistry major. My brother had a friend who was older than my brother. We went to camp, summer camp. My brother and I went for eight weeks out of town to summer camp.

SS: Which one?

EK: It was Camp Arthur, which was run by the YMHA, the Young Men's Hebrew Association in Philadelphia. It was very close to Philadelphia. My brother became a counselor as he got older, and had a friend who was a little older than he who was a counselor, Jerry Valentine. And he ended up, he went to medical school, and then while in medical school or just afterwards, I don't remember which it was, he began to work with a professor of biochemistry at University of Pennsylvania Medical School. Sam Gurin was his name, an excellent biochemist. I knew later. I didn't know at the time. Anyway, he took a chemistry course that I was taking.

SS: Your brother?

EK: No, his friend.

SS: Oh, your brother's friend.

EK: I didn't know what I was going to do. I knew I had to go to graduate school to do anything in chemistry. Biochemistry was not taught then in undergraduate school, and biochemistry was just beginning as a field in this country. And he said, "I think you'd enjoy biochemistry." So based on his recommendation, I applied to several graduate schools in biochemistry and only was accepted at Penn. The biochemistry department was then called physiological chemistry, was in the medical school, but they offered a PhD at the same time. They had an occasional graduate student, not a group of graduate students every time. So I was accepted. And shortly after I began as a graduate student, the first two years were all courses, course work, and the last two years—if you were lucky, it was only two years—was research.

So I got called into the department chairman's office. His name was David Wright Wilson, but D. Wright, he called himself. We called him Damn Wright. The students, that is. He called me into his office and the first thing he said was, "Mr. Korn, please zip up your fly." I had come from the bathroom and hadn't zipped up my fly. So that was a good start. And he said, "We accepted you even though your grades were not that good, because you had B's in the very difficult courses and B's in the very easy courses. We knew that if you worked harder, you could have gotten all A's. And certainly, a lot of A's. And so we're expecting you to work hard." As a graduate student, I did work hard. As an undergraduate, I mostly played cards. I played bridge; I played hearts. I belonged to a fraternity, Sigma Alpha Mu, which still exists, and I was more interested in the social aspects than I was in the academic aspects. Graduate school, I realized I had to change, and so I got A's in all my courses in graduate school, no problem. And they offered a master's degree. I didn't get a master's degree, didn't bother to get one. Well, actually, they may have stopped giving it that year.

The previous years, people got master's degrees and then they left school and got jobs in industry, and the department wasn't interested in training people who were not going to go into academic careers and do research. So they stopped offering a master's degree, but they still had an exam and you had to pass that to qualify to go on for your doctorate, to do full-time research. And I and two other students did that and became PhD students the same year.

SS: That is a small class.

EK: That was the first time they had more than one person getting a PhD at Penn. There were three of us. We all succeeded and got our PhDs.

SS: Who were the other two? I'm curious. Did you stay in contact with them or did they have successful careers as well?

EK: Well, one of them was a man named Mel Blecher. He got a job at Georgetown University, where he was told by other people, "Don't tell them that you're Jewish because they might not hire you at Georgetown." I don't know if that was true or not. He began a career at Georgetown, but then went to law school. The other one, Cecil Cooper, was the one I was closer to, and he got a very nice job at Case Western Reserve Medical School, had a successful career, was frequently voted the most popular faculty member in the department, and he did research. Both Mel and Cecil have died.

SS: So, in talking about your social life, too, you got married in 1950?

EK: I did. I was a graduate student when I got married. I started graduate school in 1949, in the fall of '49, and got married in June of 1950.

SS: So you must have been dating as an undergraduate?

EK: Yes, I was.

SS: And met your wife.

EK: Yes. She went to Penn. She was a much better student than I was. She was a Phi Beta Kappa student, and active in the Hillel Foundation. I knew her because fraternity brothers of mine dated her. She was one of the most attractive women on campus and very popular. And she was dating a fraternity brother of mine and then a second, another fraternity brother started to date her. He was from out of town, from Connecticut, from New Haven, Jerry Kaplan—and he was in charge of the fraternity kitchen, buying the food and hiring the cook and that sort of stuff. So the person he bought the food from, there was one group, company I guess, who supplied all the food, had a boat on the Chesapeake Bay. They offered him to have the boat with a pilot, whatever you call the person who drives a boat. It was a motorboat, but it could sleep four people. And he could bring a date and a friend.

So he had a date with this woman, and I had a date. I said to him the day before, "Suppose we switch our dates?" So he said okay, and the woman I ended up marrying was his date when we switched dates. She lived in Philadelphia, as I did, and we both commuted to college every day. So I guess I went by public transportation, because I didn't have a car, to pick her up, and she was surprised to see me and assumed that because I was coming in by public transportation as she was, and he was living on campus, that I was just stopping to escort her down to campus. But the two women accepted the fact that we had switched dates. So that was the first date I had with her, and we stayed in the boat I guess for one night or maybe it was two. But the men slept in one bedroom and the women in another bedroom, and it was our first date with each of these women. And she reminds me every so often that after, the next day when she woke up, she came out to give me a kiss good morning and I wouldn't kiss her. I didn't kiss her until after I brushed my teeth, I guess. In any case, she remembers that.

Anyway, we continued to date, obviously, and then we became engaged and got married while I was still a graduate student, which my father objected to strenuously. Her parents didn't object, but my father did. She was in graduate school for one year and then because we got married, she had to stop and get a job and work, so I really affected her career. I got one of the first NIH fellowships for graduate students. Well, the first year, I was given a position at the university. I helped teach the medical school class in biochemistry. I had just taken the course and I helped teach it as an instructor in the laboratory. I didn't do much. I didn't teach; I just was in the laboratory answering questions and telling them where the reagents were or this sort of thing.

SS: Let me stop there for a moment, because I wanted to ask you your wife's name.

EK: Her name is Muriel Evelyn, but she calls herself Mickey. Everyone knows her as Mickey.

SS: And her maiden name?

EK: Fisher. She grew up in the Bronx. She was born on Long Island. Her mother had health problems. Her physician said she'd do better in a higher, dryer climate, so they moved from Long Island into the Bronx. (Laughter.) So she grew up in the Bronx, in an apartment house in the Bronx. She looks Irish. The elevator operator started to call her Mickey, because he thought she was Irish. He called her Mickey. And he was surprised to find out that her parents were the Jewish immigrants who still spoke with a European accent. But she grew up as Mickey and is still known by everyone as Mickey.

SS: What was she in graduate school in?

EK: Sociology or social studies, whatever that was called then. And then after we got married and then came here, she got a job for the Ford Motor Company, selling cars to diplomats. So the diplomats would come in and she would place the orders and handle that for them. And then when we had children, she stayed home, essentially, with our two daughters. But she went back to school when they were old enough to be in kindergarten, essentially, and took education courses, and she taught for a while at the middle school, at the junior high school level.

SS: Were your daughters born in the fifties? When were they born?

EK: The first one was born in 1955, and then about two years later, I guess, 1957.

SS: So that was after you got your PhD.

EK: Yes, I was here. I got my PhD in 1953. I worked for a man named Jack Buchanan.

SS: Yes, tell me about that. I diverted you because you were starting to tell me about your fellowships and then I wanted to find out a little bit more about your wife. But tell me more about your fellowships and then the lab.

EK: Well, I got paid $1,000 a year by the department, and free tuition, to help in the biochemistry laboratory. The last couple, two years, I had a Damon Runyon Memorial Society Fellowship, and they gave me the fellowship and it was $3,000. And that was the second time I was called in to the department chairman's office. He said, "We can't let you accept that $3,000 because that's more than we pay our instructor Adelaide Delluva, who is on the faculty." And so I had them reduce it to $2,000 or something, I've forgotten. And I still had that Damon Runyon Fellowship when I came here. I got my PhD. Let me jump back.

I was a graduate student. My mentor was John Buchanan, Jack Buchanan, who was a young man about ten, eleven years older than I, but had already done some very outstanding things as a graduate student at Harvard, where he was a friend of Chris Anfinsen, who I began to work with here. And we'll get to that story later. But he was an outstanding scientist at the time, working on purine biosynthesis. Purine is one of the two components of nucleic acid, purines and pyrimidines. Long before people were looking at how nucleic acids are synthesized or their structure, they didn't even know how the purines and pyrimidines were synthesized. And he was studying purines, and Dr. Wilson, the department chairman, was studying how pyrimidines were synthesized. Cecil Cooper was working with him.

So I chose to work with Jack Buchanan, who was then a professor. He was not that old, but was a professor. He was married to a Swedish woman whom he met while he was a postdoctoral fellow in Sweden. And while I was nearing the end of my PhD research, he was offered and accepted the offer to create a department of biochemistry at MIT. They didn't have a biochemistry department. It was in the biology group, but they had a department and they wanted him to head it up. He accepted that, and I could have gone with him to help, as an instructor, but I knew if I did that, I'd never get my PhD. I mean, while still continuing research I would be helping to teach the courses and all that stuff. So he agreed that I could get my PhD and not come to MIT, and get my PhD at Penn, completed the work at Penn. So I did. I published one or two papers as my graduate work, but they were published after I got my PhD. They didn't appear in print until afterwards.

I had arranged to go for a postdoctoral fellowship which were becoming standard at that time, with a man named H.A. Barker at UCSF, University of California, San Francisco. I was familiar with his research, which was outstanding, because of two students who were husband and wife, Earl and Terry Stadtman—but Earl Stadtman in particular. And Earl Stadtman had gotten his PhD with Barker and then gone to Harvard. Now, I was also attracted to the fact that it was San

Francisco. I had never been out of Philadelphia, essentially. One meeting, a Biochemistry Society meeting in Chicago, I was there for a week. And I'd been to New York, I think, and that was it. I had a very good friend in college who lived in Washington, so I had been to Washington. In any case, I'll get to that later.

So H.A. Barker had spent the previous year here at NIH with Earl Stadtman, who was here at the time with Arthur Kornberg who won a Nobel Prize many years ago, had been here in Building 3 with everyone else, and Kornberg had just gone to Washington University as department chairman.

SS: And that's in St. Louis?

EK: St. Louis. Arthur Kornberg had just gone there. Barker agreed to accept me as a postdoctoral fellow, but then he had a mild heart attack or something and had to cancel, and he told me that a few months before I was to leave and drive across the country. And without my knowledge, he had talked to Arthur Kornberg, who had agreed to accept me in his program, but St. Louis didn't seem very attractive. Arthur Kornberg and Jack Buchanan, who I got my PhD with, had been competitors, and Buchanan had some feelings about this, so it didn't seem appropriate for me to do that. So I thanked Kornberg, but I said no. And then I was stuck, what am I going to do? I mentioned before that Jack Buchanan, who was now at MIT, was a good friend of Anfinsen's because he had been a graduate student with Anfinsen, and they were both in Sweden at the same time as well, or Copenhagen. So he contacted Anfinsen. Anfinsen agreed to accept me, so I came here.

SS: To NHI [National Heart Institute]?

EK: NHI, which was then a very different place. I mentioned in passing that while I was an undergraduate I had come to Washington where I had a very good friend who lived in Washington. His parents belonged to Woodmont Country Club which is now just out Rockville Pike. I guess you don't know this area.

SS: Not very well. I've been here a few times.

EK: Anyway, but when I visited my friend, who lived in downtown Washington, his parents belonged to Woodmont Country Club, and they took us to dinner at Woodmont Country Club, which was on what is now the NIH [National Institutes of Health] campus. So I was on the NIH campus before the NIH was here. It was built in pieces, the original land having been given to them by people named Wilson who owned a huge estate. They still had a house, and the house is still there. NIH uses the house now. They call it Wilson House. But the south end of the campus was still Woodmont Country Club, and I had dinner here when it was Woodmont Country Club. I was probably a graduate student. It was probably in early 1950 or something. It may have been when I was an undergraduate. But so I came here, and when I came here in 1953 as a postdoctoral fellow, there was still a nine-hole golf course, a public golf course, which was left over from the Woodmont Country Club on this end of the campus, and there were tennis courts on this end of the campus.

SS: A recreational area?

EK: It was a recreational area. Building 3 had all the research staff of the Heart Institute.

SS: And that's where you went?

EK: Yes, I came to Building 3.

SS: I want to go there next, but I want to ask you a couple of questions before we leave this earlier period. So, Buchanan. I have read your article about serendipity and luck ["The Discovery of Unconventional Myosins: Serendipity or Luck," *Journal of Biological Chemistry*, January 14, 2004] and you talk a little bit about lessons that you learned from him, lifelong lessons that you've used. I don't know if you can or wish to recap those for this recording.

EK: Well, I could read them. (Laughter.) They're here.

SS: Well, I really enjoyed reading that article. It helped me.

EK: That was done as part of the *JBC Reflections*, and it's still going on now, but they were begun in 2005, which was the hundredth anniversary of the *Journal of Biological Chemistry*.

SS: Which you've been very involved with, right?

EK: Yes. And the person who served as editor-in-chief for the longest time, for thirty-five years, is named Herbert Tabor. He still comes to work every day. He's a hundred years old.

SS: My goodness.

EK: He was one hundred in November, this last November.

SS: Amazing.

EK: Yes, he works at the bench every day. I come into work, but I work in the office. Well, part of the serendipity was not being able to go to H.A. Barker's lab and not going to Kornberg's lab, and the fact that because Jack Buchanan knew Chris Anfinsen and I came here and met Earl Stadtman, whose work as a graduate student was the reason I wanted to go to Barker's lab. And so, there was Stadtman in the same building as I was. The director's office, the director of NHI, was in Building 3. All the research was in Building 3, and it shared the building with other institutes, what is now NIDDK [National Institute of Diabetes and Digestive and Kidney Diseases]. I forget what it was called then. We didn't have the whole building. Shannon, who became director of NIH, was the scientific director. Jim Watt was the Institute director. Actually, I got called into his office too. I was scared to death. I checked that my fly was zipped up.

SS: (Laughter.) You learned your lesson there.

EK: He lived in downtown Washington on Macomb Street, which is near the zoo. My wife and I lived in a walk-up apartment house a block away. He needed a ride home. So he could easily look through all the employees of the Heart Institute. There weren't that many of us. And he found me, so I drove him home.

SS: So that was before the Metro.

EK: Oh, it was all before Metro, yes. There were buses you could take. I guess there were buses; I'm not sure. I drove, anyway. But whether there were buses to here, I don't know, but there was certainly no Metro. Anyway, he employed Shannon as the scientific director. When I came, he had been scientific director from 1949 to 1952, which was the beginning. 1949 was when the intramural program began. The Heart Institute was 1948 maybe, created by Congress. But the intramural program began in 1949, and Shannon was the first scientific director. When I came in 1953, he had already (in 1952) become associate director of NIH, which he was from 1952 to '55, when he became director, the longest-serving director and the most important director. He really started the whole extramural program, postdoctoral fellowships, pre-doctoral fellowships, and research grants.

And it's worth quoting, because it had a big influence on the Heart Institute, something I just found out last week. Irwin Arias, who still teaches a course here has published an article (Bridge Building Between Medicine and Basic Science) in which he states: "However it was the leadership and wisdom of James Shannon that resulted in postwar growth of basic science at the National Institutes of Health and of basic science departments in the nation's medical schools

(Farber, 1982). In part influenced by the Flexner report (1910) the Shannon model was based on the concept that diseases will be cured only when science produces fundamental understanding of physiology and pathophysiology." ["Bridging the Bed-Bench Gap: Contributions of the Markey Trust," National Research Council, 2004.] Indeed, Shannon had a big emphasis on basic science. He created in the NHI Laboratories of Chemical Pharmacology, Chemistry of Natural Products, Cellular Physiology, Metabolism, Kidney and Electrolyte Metabolism, and Technical Development.

SS: That is impressive. Just briefly, too, you served in the military service somewhere in here too, didn't you?

EK: Oh, that's the real serendipity or good luck. No, it's not serendipity, just good luck. I came on a Damon Runyon Fellowship, having forgotten all about the draft. I had torn up my draft card years ago. I was deferred initially because I got married, and then marriage was no longer an exception. Then I was deferred because I was a graduate student. I forgot all about the draft, never thought about it. I come here, I'm here about a month, and I hear from my draft board that I had gotten my PhD. I'm here a few months and I'd gotten my PhD and was now reclassified 1-A, which meant I would have been shortly called up for a physical exam, and if I passed the physical, I would be in the military.

SS: Now, is the Korean War going on at this point?

EK: The Korean War, yes.

SS: I know it ends in '53, but maybe it was still ongoing.

EK: Oh, yes. The Korean War was still going. The Korean War began two days after I got married. We were planning to get married on a Sunday, and my cousin, my mother's niece, only cousin on that side, got married on the Sunday, took the Sunday and got married. We couldn't get married. So we got married on a Friday. You couldn't have a Jewish wedding on a Saturday. So got married on a Friday. Sunday, when my cousin got married, the Korean War broke out, two days after we got married in 1950. But it was still going on in 1953.

Anyway, the Public Health Service existed on campus. I didn't even know about it. And it was mostly MDs who joined the Public Health Service as MDs, and they could avoid the war. So I was able quickly to get in the Public Health Service. So I served for two years in the Commission Corps of the Public Health Service. By that time, the war was over. It took care of my draft requirements, military requirements, and then I resigned and went into the civil service. There were some other PhDs in the Commission Corps, but relatively few. It was mostly MDs, and that continued for many years. The MD draft continued long after the regular draft stopped. There was still a medical draft. And so, even when I became acting-scientific director, which was 1988, all the MDs on campus were in the Commission Corps of the Public Health Service, and the draft ended and then they began to switch over.

SS: What did you do?

EK: Exactly what I was going to do. I did nothing different. I was with Public Health Service and I continued the research I was doing with Chris Anfinsen.

SS: Oh, that's wonderful.

EK: So it was pure chance. If I had gone to Barker's lab in UCSF, I'd have been in the army three months later.

SS: Oh, my goodness. Yes, that is luck.

EK: Yes, pure luck. I just totally ignored that I was subject to the draft. Anyway, so when I came here—I wrote some notes down because I knew I wouldn't remember, apart from the fact that I met Earl Stadtman. The Heart Institute staff had Chris Anfinsen, whom I was working with; Bob Berliner, who became scientific director when Shannon moved up to NIH. Bernard Brodie; Earl Stadtman; Terry Stadtman, his wife; Sid Udenfriend; Jack Orloff; Martha Vaughan, Jack Orloff's wife; Julie Axelrod; Don Fredrickson; Robert Bowman—Bob Bowman. Many of these people, most of them have passed away. Stadtmans are both gone; Udenfriend, Orloff, Martha Vaughn, Herman Kalckar and his wife, Barbara Wright Kalckar; Evan and Marjorie Horning. Notice lots of husbands and wives.

SS: Yes. Seems unusual. Was it?

EK: Universities would not hire husbands and wives.

SS: Oh, okay, that's it.

EK: NIH had no such thing, so that's exactly why it happened. In some cases, the Stadtman case, Terry Stadtman had been working with Chris when he was at Harvard. He came here; he brought Terry here. Earl came along as her husband. Earl was offered many jobs, but no university [position]. He kept all the letters. They wouldn't give Terry a job.

SS: Is that because they wouldn't hire a spouse or is that because they wouldn't hire a woman?

EK: Wouldn't hire a spouse. Though they didn't hire many women either, but they wouldn't hire spouses. So you had Evan and Marjorie Horning. Evan Horning had taught the chemistry class at Pennsylvania that I took where this friend of my brother's, who was a physician, came and took the course with me, who recommended I go to biochemistry. But he was here.

Anyway, the other institute who had a lot of space here was then called NIAMD, National Institute for Arthritis and Metabolic Diseases, now NIDDK. They had Leanne Heppell, Bernie Horecker, Bernard Witkoff, Jim Wyngaarden. Of the fifteen NIH scientists, eleven were elected to the National Academy of Sciences after they were here. Two—Anfinsen and Julie Axelrod—are Nobel Prize winners. Three—[Donald] Fredrickson, Wyngaarden, and Shannon—became NIH directors. Eleven of the sixteen NIAMD scientists in Building 3 became members of the National Academy, later. Two of Stadtman's subsequent postdocs, Stan Prusiner and Mike Brown, won Nobel Prizes. They were not here when I came. They came afterwards, but they were in Building 3. And it's just amazing that that group was here, that Shannon had brought this group in. NIDDK [NIAMD, it was then] had people in other buildings, but the whole Heart Institute research was there [in Building 3].

SS: Was Shannon the one who brought in people from the Goldwater lab [Goldwater Memorial Hospital]?

EK: Yes, Shannon brought the Goldwater people down, which were Berliner, Witkoff, I guess; and Wyngaarden. I've forgotten. You may have the names of them all, but there was a bunch of them, anyway.

SS: So I've read a little bit about the atmosphere then. It almost seems like a golden age in what you just described.

EK: Yes, it was just incredible, the quality of people.

SS: Tell me something about working there in that building at that time.

EK: Well, at that time, the Clinical Center was being built, the original Clinical Center, but it wasn't occupied. We moved in in the spring of '54, so we were only in Building 3 in that situation for a short period, when I was there a short period. But then we moved back to Building 3 several years later, a lot of us were back to Building 3, and we spent many years, until this building was built, which was 2011.

SS: Building 50.

EK: Building 50. It was built in 2011 and we were the first people to occupy it in the spring of 2011. So we watched 9/11 on television here, in this building. But so most of the time in Building 3 was after the Institute had expanded and the whole Research Institute was not there. The clinical people were in the Clinical Center at that point. So I don't have the memories I can recall. I was tremendously impressed by the quality of the people, but they became members of the National Academy later. They were all young people. They were all about ten years older than I. Well, maybe Shannon was older. But Anfinsen was eleven years older than I, and most of these people were younger. They were all very active, very dynamic, constant seminars every day. There was a daily seminar of the Heart Institute people where someone spoke about their research.

For several years, where Building 31 (which is an office building) is now, there was something called Top Cottage. Let me back off. Where the Clinical Center was built had been a place where there was a caretaker's cottage, but it was privately owned land. The Wilson's house is still there. They had a cottage, which we called Top Cottage. I'm told where the Clinical Center is built was a hill about up to the fourth floor or so of the Clinical Center. I'm not sure about that. I was told there was a hill there. And the cottage was on top of that hill. It was called Top Cottage. When the Clinical Center started getting built—this is before I came—they moved Top Cottage to where Building 31 is. And once a month—was it once a week or once a month? Once a week, all the biochemists at NIH, all the institutes that then existed, met in Top Cottage for a seminar from one of the speakers, one of the members. And once a month, all the biochemists, Johns Hopkins and NIH, met alternately at Hopkins and here. So that was an atmosphere and that was going on for several years, not just the first couple of years that I was here, but for several years later. And at Top Cottage, you were allowed to serve alcohol. It was the only place on campus that you could serve alcohol. Well, it was, again, a home that was private home that's out this way, which you could serve it for certain affairs.

I've brought you some pictures. The campus was totally different.

SS: Oh, so we're looking at printed photographs of the NIH campus.

EK: In 1941—this is Wisconsin Avenue, now Rockville Pike, which is a divided six-lane highway. It was a two-lane highway from downtown Bethesda out past NIH.

SS: Lots of trees.

EK: This is the campus. Our building is back over here somewhere. This is the whole campus in 1941. In '46, we're still looking at the campus. This is Building 1, 2, 3, 4, 5. So we were all here. This is where Building 50 is now. All trees. And the Clinical Center's back up here now. Behind Building 1, which is now a big parking lot, there was a baseball field. That's in 1946 before I came here.

SS: And who played there?

EK: It was the summer. And at lunchtime, people would go out and play baseball. So Building 3 existed, but this is before the Heart Institute existed, in 1946. During the war, Building 3 was used for disease research. Anyway, in 1948, the Clinical Center was begun. That's the big Clinical Center now. And this is where Building 50 is back here. And Top Cottage was moved over—you can't see it in this picture. The year after I came in 1954, the Clinical Center was built. We had moved into the Clinical Center from Building 3. It's still all trees over here. I thought there was a picture that showed Top Cottage, but apparently not. Anyway, it was a very small campus. Let me get back here. You can't really see it, but there's a cottage here.

SS: Is that it? Yes. Okay.

EK: That's the cottage there, and we used to meet once a week. All the biochemists met there.

SS: And share your research?

EK: And shared our research, and then a monthly with Hopkins alternatively here or the Homewood Campus of Johns Hopkins. Not the medical school, but the Homewood Campus.

SS: Does that kind of thing still go on, seminars with people talking about their research?

EK: Well, yes, there's always a guest speaker. Most departments have a weekly seminar within their department where they invite guests who could be from NIH, but more often from outside NIH now come in. We, in our Center, have a weekly guest speaker coming in, and then a weekly seminar when our postdoctoral fellows talk about their research.

(Break)

SS: Okay, we're back. So it was a very vibrant and fertile time.

EK: Yes. So Building 3, people have asked me am I glad I moved or whatever. Building 3 was like our home. It was all NHLBI before we moved here. We knew more about the building than the building engineers knew. We knew where the fuses were, the circuit breakers were. We would drill holes through walls without asking anyone. We moved here and it's like living in an apartment house. We can't do anything. We're locked in, as you know. Anyway, it's very different. But I should also mention that Bethesda has changed more than NIH has changed. When we moved here, Bethesda was a little, small town, nothing higher than two floors. If there were three floors, it's because the owners of the store lived above it. Rockville Pike was a total of two lanes all the way out to Rockville. There was no Beltway, there was no expressways, no I-95, no I-270s, no nothing. And it was segregated. Schools were segregated when we moved here. There were black schools and there were white schools, and you would never see a black in a restaurant or a store that we would go into.

SS: Was that different from Philadelphia?

EK: It was different, yes. In a technical way, it was different. That is, schools were not segregated in Philadelphia, and housing was theoretically integrated, but it wasn't, in fact. The blacks still had the one place and the whites in other places. So the schools I went to elementary school and junior high school, I don't remember there being a black in the school. There wasn't a black in the neighborhood. I mean, and our friends were all white friends. The high school I went to drew from all over the city. There were African Americans in the high school, and every high school had blacks and whites because they drew from a larger area. But here, the schools were segregated, and I think it was 1955 when the court order came in, they desegregated instantly, but neighborhoods were still segregated. Many neighborhoods had local organizations, community organizations, and they agreed and the policy was never to sell to an African American.

SS: An unwritten policy?

EK: Well, written within the local policy, yes, so in that sense, yes. I mean, when we bought a house within walking distance of NIH, there were no African American families there. Now there are a number of them. When the first African American family moved into our neighborhood a few blocks away, our next-door neighbors moved out. But it's so different now; it's unbelievable. And you go to any restaurant, there are as many blacks as whites, and neighborhoods, housing is integrated.

SS: You still live in that house?

EK: We still live in the house we moved into in 1955, yes.

SS: So you've seen a lot of changes. I mean, just talking about Bethesda and the roads and segregation.

EK: Yes, and our house, which cost us $23,000, would now be $750,000. The house next door was torn down and is being rebuilt now slowly. It'll be $1.5 million. It's going to be twice the size of our house. And the neighborhoods are changing, yes. This neighborhood, it's close enough to Metro to walk to Metro.

SS: Yes. And I imagine a lot of people who work here can't afford to live out here and they take the Metro.

EK: They live much further away, yes. They live quite far away. The initial janitorial staff at NIH-type person were people who never held a job before who lived way out in Montgomery County, many of them African Americans, but not all. They would take in laundry, do that sort of thing for income, mostly women, and they would have to commute by public transportation to get into here. Now all that stuff's under contract, and we're working today when the rest of the government's not working. We're working today.

SS: Right. Here we are [during the government shutdown].

EK: Here we are. Anyhow, when I first came to NIH, that first group was just incredibly able people, as illustrated by the elections afterwards to the National Academy and Nobel laureates. In basic science—there was no clinical research at all at NIH at that time. The Clinical Center hadn't been built, hadn't been opened. There were physicians who had been hired expecting the Clinical Center to be open, but they were all doing basic science because the Clinical Center hadn't opened. But a year later, it did, and then things began to change. But still, under the Shannon influence of the Heart Institute, but also all of NIH, it had a strong influence on basic science in addition to supporting clinical research.

SS: So tell me the difference or the importance of basic science, in your mind, for a layperson.

EK: Yes. Well, you think back, you could never treat diabetes with insulin if you hadn't discovered insulin. It's a glucose metabolism problem, which is actually one of the things that Martha Vaughan worked on before she came here when she was a postdoctoral fellow at University of Pennsylvania. I must have met her there before I came here.

SS: Because you both were working in Anfinsen's lab, right?

EK: She came down and worked at Anfinsen's lab, yes. Martha and a person named Dan Steinberg, worked with Anfinsen on, his work then was on protein structure and proteins. And then Martha and Dan separated from Chris and a new department was created, which Dan Steinberg was the head of and Martha was in and they were independent. They didn't work with Anfinsen. So she only worked with him for relatively few years. But anyway, Steinberg has passed away too. He left NIH years ago, went out to University of California at San Diego and La Jolla.

SS: Did you ever make it out to California?

EK: No, but after I was here, I was offered a position at UCSF biochemistry department, which I ultimately didn't accept. And then later, because Dan Steinberg had gone out to University of California at San Diego when it began a medical school, and he arranged, that was the source of offering me a position out there. I put a deposit on a house and almost took that job and decided not to, and subsequently I met some people who were there, who said, "You made the right decision not to come here but you should've bought the house, which had a view of the ocean."

SS: Yes, definitely.

EK: It was in La Jolla, where the medical school was. I would have gone there was when Reagan became governor, and they reduced the support of universities a lot. They've recovered from that, but for a period in there they were having a hard time.

SS: So you were funded well here?

EK: We were funded well here. If you did good research, you never had to ask for money, essentially. You didn't apply for grants. Each department had a budget and your department chief divided the budget up or the scientific director divided the budget up. So you didn't have to spend half your time, which people do spend at least half their time writing grant applications to NIH, to the Heart Association, Cancer Society, and that sort of thing. We didn't have to teach courses, although there is a school that operates here that I taught in for a little while, but there's no teaching responsibility at all, so you're full-time in research, which if you enjoy research is a wonderful possibility to have.

SS: Obviously, it was for you.

EK: It worked for me, yes. There was someone who died, a person at another institute, about a month or two ago who has an interview like this at some point in his career, and he was offered a position when he was young, very young, at a university, which he was tempted to take. And he took the advice of his mother-in-law, he says, which was: "If you're happy where you are, don't think of leaving." And that's basically what he did, and what I did, and a number of other people have done. People have left. A lot of people have left, of course, over the years. As the place got fully occupied, as it now is, the only way there are openings is if people retire or die. There's no expansion. But in the fifties and sixties, there was tremendous expansion, and it was especially good for women because as we said the universities were still not hiring couples, and even women to some extent.

SS: That seems remarkable here, just reading the stories about Building 3. And I saw Eugene Braunwald, his Centennial Lecture, a lecture about the history of NHI, and he talked about his wife and he talked about the atmosphere.

EK: Nina Braunwald was the first woman surgeon, board-certified cardiac surgeon, and she did the first valve transplant, here, at NIH. Nina died a number of years ago. But so yes, Nina and Eugene Braunwald could not have been on a university faculty.

SS: Right, and he just talked about how people would go over to people's houses and have dinner and drink wine, and there was just this convivial situation in the seminars that you talked about.

EK: Yes, that was certainly true, and the first thing when we came down here, my wife and I, within a matter of weeks or something, there was a party and all the staff of the Heart Institute was there in someone's home. We don't have parties like that now. The Heart Institute's too big to have everyone there. I mean, the group that I'm in now, there's about 100 people. There are more people in our laboratory—in the larger group, not my own research group, the Cell and Developmental Biology Center—than there were in the whole Heart Institute when I came. A lot more.

SS: Right. And so, you told a story about being in an elevator with a cow [in the article "An NIH Research Dynasty in Building 3: A Who's Who of Biomedical Researchers," by Heather Dolan in *The NIH Catalyst*, September-October 2012].

EK: Well, that was Nina Braunwald's cow. (Laughter.) In Building 3, there's an attic floor. Why they put the animal room up on the attic floor, I don't know. There was one elevator, and I was on the third floor, my lab was on the third floor. It's changed now, but you entered the front door, you went down a half a flight to one floor and up a half a flight to a floor. There was no floor where you entered. So I would go down a half a flight, take the elevator up to the third floor, and there was a cow in the elevator. And I got in the elevator and the cow lost control, and so I was ankle-deep, it is an overstatement, but, in urine. Why the animal room was up there I don't know. They would hose down the animal room and the water would back up in the sinks down in the basement level. It was just incredible. I started to say that the building before, during the Second World War, it was malaria research going on there. The building was the center of the Public Health Service malaria research program, but that all was gone when we came. That all stopped. The Heart Institute then NIAMS [National Institute of Arthritis and Musculoskeletal and Skin Diseases] took over the building.

But some of the people who came were MDs who were there, my age, were MDs who'd come to do clinical research in the Clinical Center, which hadn't opened. So they were working with Anfinsen and other people and doing basic research. And then when they went over there, they expanded into clinical research as well when they opened up.

SS: Right. Well, you mentioned the fact that NHI or NHLBI or maybe NIH hired these husbands and wives who were discriminated against in academic campuses, but also it seems they hired women. There were more women, and Jewish people were not discriminated against it seems.

EK: There was no discrimination at all. I mean, there were no African Americans on the faculty.

SS: But it was an atmosphere of nondiscrimination?

EK: Yes, there was certainly nondiscrimination. There were people who discriminated. One of the people who was one of the first branch chiefs, lab chiefs, he openly discriminated. We had a meeting many, many years ago, an out-of-town meeting, stayed overnight somewhere. He was openly anti-black. But as a general rule, no, very few. There were very few black PhDs or MDs doing research at that time, so we couldn't hire African Americans at a doctoral level. Well, but we certainly had black technicians. Chris Anfinsen had one when I came who was African American, a woman.

SS: So was that a directive from the federal government, or was that something about the atmosphere here, or was it Shannon, or was it Watt, was it one of the directors who was more open?

EK: I really don't know. As far as I know, it was not a directive. We've certainly, in more recent years, have had directions on down about integration, but I think it was just the policy of the people. Someone like Anfinsen had Terry Stadtman. And Earl was here because Terry was hired. Jack Orloff may have been hired—he worked with [Robert] Berliner, and may have been here before Martha Vaughan, but certainly Chris had no problem hiring Martha. The current deputy director for NIH of intramural research, Michael Gottesman, has a wife, Susan Gottesman, who was hired by the Cancer Institute, Ira Pastan, who brought Michael here. There are a lot of men who are here because their wives were hired directly.

SS: That's fascinating.

EK: Yes. So, Herb Tabor, the hundred-year-old scientist, his wife Celia Tabor was here as well, in the same department as he was, for many years. She's passed away a long time ago now. It's certainly been open to women, for sure, and as far as I know, a lot of Jews. I mean, there are a lot of people of Jewish descent. When I was a graduate student at Pennsylvania, the medical school still had a Jewish quota and a black quota. There were 10 percent Jews admitted and I don't know how many African Americans, but fewer African Americans.

When I was here for about two, three years, about two, three years after I got my PhD, Sam Gurin, who was a professor of biochemistry, offered me a position to come and join their faculty. There was no dean of the medical school at the time. The position was vacant and they were recruiting him. So I said, "Well, you know, you don't have a dean," and there's other reasons. He said, "Well, I can't tell you who it's going to be, but you'll be very happy with the new dean." He was the new dean and he was a Jew. So about five years or ten years after they still had a quota of Jews in medical students, they had a dean who was Jewish, so it changed there as well.

When I was an undergraduate at Penn, there were Jewish fraternities and Christian fraternities. No fraternity had Jews and Christians. There was an intra-fraternity society. There was a Christian intra-fraternity society and a Jewish intra-fraternity society. They were totally separated socially, not in school, not in classes, but socially. And Philadelphia having a fairly large Jewish population, the undergraduate school had a fair percentage of Jews. But socially, they were separated.

SS: You didn't really have friends outside the university, outside the fraternity system, that were non-Jewish?

EK: No. Essentially, no. Classmates but not friends, not social friends. I never dated a non-Jewish girl.

SS: It's amazing how much things have changed for the better, right, in that regard.

EK: Oh, yes.

SS: Let's take a break here.

(Break in audio)

SS: [Resuming interview after lunch break] Dr. Korn, we've set the stage, I guess. Let's now talk about the important work you've done here [at NHLBI]. Of course, we know we could probably talk for two days and still not really talk about it all, so we're going to have to just hit the highlights. But I'd actually like to start with your work in Anfinsen's lab, early on, and talk about how you changed direction, and then when you got the first electron microscope. Those first maybe ten years that you were here in that period, late fifties, early sixties, and talk about the work you did with him in his lab and about your particular work.

EK: So shortly before I came here, John Gofman, who was at UC Berkeley, had separated plasma lipoproteins into LDL, the low density and high-density lipoproteins, and correlated the low density with intensive coronary artery disease. And Shannon asked Anfinsen if he would pick up on this research. It wasn't something that Anfinsen did, really, working on plasma lipoproteins. But he agreed to do it and he started the project a year or two before I came, with people like Don Fredrickson and Bob Gordon and others. And then shortly after Gofman's discovery, people named [N.G.] Anderson and [B.] Fawcett found that if you took plasma from a person or animal and if you gave heparin, it cleared the lipemic plasma high lipid concentration. Let me know if you don't understand what I'm saying.

SS: You're good so far.

EK: Anyway, the turbidity due to the high lipids would clear, and so they called that a clearing factor, that there was a clearing factor released. So that's when I came, and I agreed to work in that area, which was totally different than what I had worked on as a graduate student, but I had no other place to go. I was leaving Penn. And you would ask why I'm still here, and the reason I'm still here I guess is because, in part, Anfinsen gave me complete independence. I had my PhD, came down on a postdoctoral fellowship. As we mentioned before, I was rescued from the army by joining the Public Health Service, but I could devote full time to my research. And from my very first paper, he refused to put his name on the paper. And so I was essentially independent. There were a lot of MDs in the group (Don Fredrickson, Bob Gordon, Joe Bragdon, Ed Boyle) but I was the only biochemist, with a biochemical background. So I assumed that if there's a clearing factor that is clearing up the lipids group, there must be an enzyme, a lipase, which was hydrolyzing the triglycerides. Am I'm losing you?

SS: I'm a good audience because a lot of people reading this are going to be non-scientists, but I think so far I'm with you.

EK: Okay. So I assumed that the clearing factor would be an enzyme that was catalyzing the hydrolysis of triglycerides to fatty acids and glycerol, and therefore resulting in clearing of the plasma. And so, I, in fact, characterized the enzyme as a lipoprotein lipase. A lipase is something that hydrolyzes lipids. And this turned out to be specific for lipoproteins, or lipids that were bound to a protein. It wouldn't hydrolyze olive oil, for example, just regular fat. But so it was a lipoprotein lipase. And that was the first thing I worked on here.

And then the lipids were still left behind, the lipoprotein with—so they had to be taken up by the cells by a process which is now well understood, but was not really interested then, by what's called endocytosis, which is the cell pinches off fluid and ingests that fluid. And so I thought, well, I have to study endocytosis, which is now a major area of research to which I'd contributed nothing. But I thought, well, if I want to do endocytosis and look for enzymes, I need a system, a good system to work on. So I was on a sabbatical year in Cambridge, England.

SS: Was that in 1958-59?

EK: Yes, 1958-59. And just in looking at the library there, I found that there's an amoeba called *Acanthamoeba castellani*, an amoeba that depends—all amoebae depend totally on endocytosis. They have no active transfer. But it can be grown in the absence of any other organism. All other amoebae needed another organ. They were feeding on bacteria or yeast, whereas Acanthamoeba normally did that, but it could also be grown in the laboratory on a defined medium. So anything that you're going to assay, any enzyme that you're assaying, came from the amoeba, not from bacteria that it was eating or yeast. So I started to look up at endocytosis, and I chose to work on the amoeba. And we looked at the amoeba. We were looking at the amoeba in the microscope, sections of the amoeba, and saw that it had actin filaments, what looked like actin filaments. And actin and myosin, as you won't know, are the principal proteins of muscle, for skeletal muscle, cardiac muscle, and occurs in non-muscle cells as well, although it was not known at that time that it occurred in mammalian non-muscle cells. It's involved in cell division. Actomyosin is involved in division of cells when they're growing and divide, and in chemotaxis, which is the movement of cells, moving cancer cells metastasize, metastasis depends upon their being able to move into the circulation, and out of the circulation.

And so I said, well, if there's actin there, there should be myosin, because at that time, the only thing that actin was known to do biochemically was to activate the enzyme of myosin, which is an ATPase, hydrolyzed ATP. It's an energy source. And the energy hydrolyzed by actin-activated myosin is the energy that's used for muscle contractions. So we started to look for myosin, and myosin is a very large molecule. It's two long protein heavy chains of 200,000 molecular weight and two smaller chains of 20,000 attached to each heavy chain. The whole thing aggregates to form a large structure. So we started to purify what we hoped would be a myosin, and we got something which was much smaller than all known myosins, and it ended up, it had a single heavy chain of 100-and-some thousand and a couple of light chains. It did not form filaments. Myosin and actin both form filaments which interact with each other, and the myosin moves along the actin filament and that contracts the system.

This was a single molecule, did not form filaments, a non-filamentous myosin. The myosin, it turned out later by DNA analysis and the sequence analysis, it had the characteristics of the globular domain of normal myosins, which has the active ATPase activity, but did not have this long coiled helical tail, which makes the filaments. So it's a non-filamentous myosin. Every other myosin known was very similar. So the people in the myosin field, which I was not in at the time and I've been in ever since, assumed that it was an artifact, a degradation product of normal myosin, which was known to be easily cleaved to what's called heavy meromyosin and to subfragment-1, which would be something about the size of what we were isolating.

So for about ten years, the myosin field didn't believe that this was real, but then we turned it up and another amoeba—*Dictyostelium discoideum*. And other people, not I, found this similar myosin in mammalian systems as well. And it turns out now that there are thirty-five different classes of myosin. The first class that was known is now called myosin II. We named it myosin II. And the myosin we discovered is myosin I, because that's two heavy chains and one heavy chain. And the other myosin classes are now known in order of their discovery.

So there's about thirty-five or forty different kinds of myosin, and mammalian systems have multiple—not all thirty-five—classes of myosin. So that got me involved with myosin and with actin. And so, since then, all of our research has been on actin and my first characterizing myosin I's, and then also looking at its interaction with actin and whatever. So it's been sixty-five years studying actin and myosin.

SS: Wow, because of your first studies on this were so groundbreaking that then that occupied your career.

EK: Yes. And the actomyosin field—it was always a major field in muscle research. It was limited to muscle because it was thought to be a muscle-specific system. But it turns out that there are these multiple myosins and every mammalian cell has multiple different kinds of myosins with different kinds of functions. It's become a much larger field, not just due to our work, of course, but due to the work of many, many people.

SS: Right, so with this work, were you still working in Anfinsen's lab when this went on or was this when you were out of his lab?

EK: No, I was out of his lab when this work went on.

SS: You had your own lab. I wasn't sure how that process worked. And somewhere along the line, you talked about getting an electron microscope and how that was important.

EK: The electron microscope was very important, yes. That may have been ten years later, when Don Frederickson was scientific director. Bob Berliner succeeded Shannon and Fredrickson, I guess, succeeded Berliner. And Frederickson had some extra money. I was in England again on a second sabbatical, and he came through England and asked whether there was anything I could use. Our budgets, then and now, are for our normal operating expenses. If we need big equipment, the scientific director has money he holds out and then decides among the various people who are requesting large-scale equipment or expensive equipment. And so, I said we could use an electron microscope. At that time I don't think there was any electron microscope in the Heart Institute. Maybe the pathology department may have had one for their pathological studies. So we had a microscope. Yes, that was very useful in looking at actin and myosin.

SS: Sounds like you had a good relationship with the scientific directors. Friendly, I mean. They wanted to fund your projects.

EK: Yes. With Frederickson and—I guess Jack Orloff must have been after Frederickson, and I was after Orloff. But Jack was Martha Vaughan's husband, and he asked me to be deputy scientific director [1982-88]. He wanted someone to whom he could talk about things that he didn't want everyone else to know. And I remember telling him, "Well, how am I supposed to know those things I can repeat and what I can't repeat?" And we left it at that. They were very different personalities, Martha and Jack. But so then he died of metastatic prostate cancer in, I guess, December 1988, and I became acting and then scientific director for ten years in replacing him, and Bob Balaban has been there for a long time, near twenty years, nineteen years.

SS: Wow, that's a long time.

EK: Doing a very good job, yes.

SS: So let's talk about some of your colleagues maybe that you worked with. You mentioned them in your article, I believe, when you were talking about the electron microscope. I think you mentioned Bob Weisman? Was he one of your colleagues in the lab?

EK: Bob Weisman? He was a postdoc.

SS: A postdoc, right. And you had several postdocs. Tom Pollard was another?

EK: Tom Pollard, yes. Tom was the one who was postdoc when we discovered myosin-I.

SS: He was working with you in your lab, right?

EK: He was working with me in my lab when I went to England the second time, and he continued for the year without me, although we were in constant touch. We didn't have email. By mail, we were in touch by regular mail. But he continued with the myosin I. So the first two papers on that myosin I were by Tom Pollard and he was the first author. He's gone on to have a fantastic career. He's been chairman in the department at Johns Hopkins, at Yale and other places. And he's appropriately a member of the National Academy of Sciences. Now, in the field, people are surprised I'm still alive, but he's about seventy-five now. He's probably one of the leaders in the actomyosin field, which I no longer am. I mean, we still work on it, but our contributions are relatively minor now. But Tom continues. Before that, when we were still trying to work on endocytosis, my first postdoctorate, Frank Davidoff and—who else? Bob Weisman was later. Wesley McBride. He died very early. He had a heart attack while out jogging and died prematurely. He had a position at the Cancer Institute. There have been major contributors. Bob Adelstein, whom you'll be interviewing, I'm sure. He's still at the Heart Institute, and is a major contributor to the myosin field.

SS: I'm going to pause this for just one second.

(Break)

EK: Oh, there have been a lot of people contributing, all of them co-authors on papers, the first author. People like John Hammer, who's still here and completely independent as an independent investigator. But you have a copy of this article ["The Discovery of Unconventional Myosins: Serendipity or Luck?" *JBC*, January 14, 2004.].

SS: Right. So, it sounds like you had a lot of good mentors early on, like Buchanan.

EK: Buchanan and Anfinsen were both very good. Even as a graduate student, I was significantly independent with Buchanan, but he was much closer involved in what I did. Anfinsen, not as much.

SS: Is mentor the right word?

EK: Yes, mentor's the right word, yes. They were the only mentors I had. Under Anfinsen, I became independent, really. He was a good example. He was still the head of the department that I was in and ran the seminars and did that sort of thing. He was a very important member of the NIH as a whole and made very important contributions. But I was independent, essentially, from the time I left graduate school until here, so I had mentors by examples but no one I was directly working under.

SS: It sounds like you also mentored people.

EK: Oh, yes, I've had a whole string of postdocs—most of the people in the photographs on the wall.

SS: Right. That must have been a good feeling, to help people along.

EK: Oh, yes. Actually, you're training people who will become independent investigators and doing the same thing, and most of them go back to universities or faculty members at universities. But also, that's how you do your research as well. I've never had an enormous group. Some people have twenty postdocs. I've had no more than eight at any one time, and usually fewer than that. But that's what gets the work done. You can only do so much yourself with your own hands. The first work on lipoprotein lipase, I was doing all the work. By the time we were studying myosin, Tom Pollard, Bob Weihing discovered actin, and we looked and we saw what looked like actin. He purified actin and showed that, yes, the amoeba has actin. It had already been shown to be in another amoeba by Ed Taylor, who was at University at Chicago, a major contributor to the actomyosin field. But Bob Weihing purified actin from the amoeba and it was the first complete characterization in amino acid sequence of the protein. And, yes, so you're dependent upon these people and the research gets done.

There are technicians in the lab. I've had very few technicians, but they're doing usually work at a lower level, what they're instructed to do either by the mentor or by the postdoc, who is guiding the technicians. Technicians are people with a college degree, but nothing beyond the college degree. So, yes, a mentor is a two-way street. You couldn't exist without the postdocs.

SS: Right. Good point. Well, what is the process of research in your lab? What's the process? Do you have an idea and you think, oh, I'm going to check that out?

EK: Well, I talk individually to all the people in the lab every day, essentially almost every day. But we meet as a group once a week and discuss what people have done in the previous week, so everyone is contributing, even the people doing something different. If you've got three postdocs in the lab, they're all doing different things, related but different. But they each are commenting and giving suggestions to each other as well as thoughts. If a totally new thing comes up, you read something, you hear about something, either I or the collaborator—I no longer have postdocs. I have two staff scientists, a position we call a staff scientist position, which are people who are permanent and who are long-term employees, but not independent, so they're not independent of me. Day by day, they're independent. They plan their own experiments, do their own experiments, but what they're doing research on needs my approval. But we talk all day.

So a few years ago, some issues of mammalian non-muscle traditional myosins these class II myosins were becoming increasingly of interest. And there were some things that I thought about doing that I discussed with Xiong Liu, a staff scientist of mine, and we agreed to do that. So he's doing day by day the work. He plans his experiments to answer questions, which we jointly discuss and agree on. I look at the results, I'll make a suggestion, you forgot to do this control or that, and specifically about things they do, and then where do we go from here. So it's a joint effort between me and the people I supervise.

SS: And you're saying that's different from the way it used to be with your postdocs?

EK: No, it's just that they have more independence. The staff scientists have more independence. And if I still had postdocs, when I had postdocs, it's been a few years now, the staff scientist could control, could be a mentor for postdocs. I mean, I might have postdocs, but one of my staff scientists could be the mentor of the postdoc. And that was true when I had both the staff scientists and the postdocs, and it's true in general around the Institute. You read the literature, you go to seminars, you hear people and it gives you an idea about your own research, different direction to take it or experiments you might do. Anyway, I could give you a copy of it if you wanted to. There's a list of my publications.

SS: I have a copy of the list of your publications. It's a huge list.

EK: Yes. Well, there are people with longer lists. Eugene Braunwald, you mentioned. He may have the longest list of anyone. He has more than a thousand papers. And so, usually the first author of the paper traditionally is the one who's doing the work. I now have three people working under me. They'll be the first authors of every paper we publish, any paper. They work in different areas and different ones will be first author. But so you can see, run down that list that you have there, of how many people are mentored. So the first thing I did the minute I got out of that field, which became very clinical, of lipoprotein lipase showing that it was in fact an enzyme, and it was a significant but not a major contribution. So the myosin I was the first, really, breakthrough that established my position in the Heart Institute and in the biochemistry field as a whole.

SS: And that paper came out in—

EK: Seventy-three. The first paper with Tom Pollard was 1973. There were papers before that, in the fifties and sixties.

SS: And it sounds like you were surprised by what you found, that that's not really what you were looking for.

EK: That's right. We were surprised. We weren't looking for actin or myosin. Fortunately, there were people in the Anfinsen laboratory, other independent people who were working on actin and myosin, so I was familiar with them but not doing anything in it at all. So when I saw in the electron microscope something that looked like actin, I knew what actin looked like and I knew about all the work that had been done on actin. It's now known that actin has many functions independent of myosin, but at the time the only function of actin had to do with myosin.

Actomyosin complex was the functional unit. And so, we knew that if there's actin, there's got to be myosin. And so, we looked for myosin and found this abnormal, what was then thought to be an artifact, but an abnormal myosin. Now, the class I myosins are the second most common class in all of biology. Class II myosins, the original myosin, is the most common. And myosin-I may have been the first myosin in evolution, and it's not clear, but there's a good chance that the myosin-I's were. But it wasn't discovered until Tom Pollard and I did it in the early seventies.

SS: I would imagine that's something that's wonderful about working in science. Exciting.

EK: Yes, yes. Day by day, it's quite repetitive and nothing happens day by day, but when a real breakthrough occurs, that's what you're looking for, yes. Hoping for.

SS: You're one of the few people who has achieved that.

EK: Yes.

SS: Quite an accomplishment.

EK: Well, I'm not alone, by a long shot.

SS: So, right after that, then you all of a sudden, I guess in 1974, you were made chief of the cell biology lab here?

EK: I've forgotten the dates. There was a section on cell biochemistry, which I was the head of in 1969 to '74, and then yes, that's right. I became chief of the laboratory of cell biology. This had to do with someone retiring because of health situations, and the laboratory was split up. When Anfinsen left NIH for a year, went back to Harvard, and then came back, but to a different institute. At that point, Earl Stadtman became laboratory chief, and there were two sections under the laboratory chief. And then one of the sections heads left, retired, and so I became independent as a lab chief, a small laboratory of cell biology, myself and a couple of other independent investigators. And as I said, in 1982, I became deputy scientific director under Jack Orloff. In 1988, was acting scientific director and 1989, became scientific director for ten years, but I kept the laboratory. Although I was in the scientific director's office all day, I'd come back here at four o'clock or something to Building 3.

SS: Where was the scientific director's office?

EK: In the Clinical Center, in the one that's called Building 10.

SS: Well, tell me something about those years. Tell me about your experience being scientific director.

EK: (Laughter.) The sequence was Shannon and Berliner, Frederickson, Orloff, and I, and now Balaban. I was the last one who knew Shannon, the first scientific director. So I think there was a continuity there that we no longer have. I don't mean it's worse. It's maybe better now that we're not tied to it. So I continued the same approach that the previous scientific directors had used.

SS: How would you characterize that approach?

EK: Well, let me back off a little bit and start again. Frederickson, when he was scientific director, had run a laboratory, and the feeling was that he may have favored his laboratory more than he should have. That's debatable. When Orloff became scientific director, he had been a lab chief as well. He resigned the lab chief job. He stopped doing research and was full-time scientific director. He died, actually, but before dying, he was tired of being scientific director but he had no laboratory to go back to. So I didn't do that. I kept the laboratory going while I was spending essentially full-time as scientific director. Also during that period, before that period, I was associate editor of the *JBC*, which I finally had to resign because I got paid for that, so I had to do it all on my own time. If you're getting outside money, you can't do it during the day. So I was all night and weekends doing the associate editor, and then all day here, to try and doing the laboratory. And so after ten years, I figured that's enough, I'd go back to the laboratory.

Balaban has kept his laboratory. He made changes, but I continued the same way. That is, there were principal investigators up until Orloff's time as scientific director. When I first came here, the scientific director had a budget. No one else had budgets. The scientific director approved every order you made. There was no problem. There was more money than we spent. While Orloff was scientific director, at some point it happened before he was scientific director, somebody decided to give a budget to a lab chief. The lab chief had the money; and the PIs had no individual budgets.

Orloff, towards the end of his time as scientific director, decided that each PI should have a budget. So the one thing I did as the deputy scientific director was establish the budgets for individual PIs, looking back at what they had spent on previous years. In some cases, it was difficult because it was all done through one pool. But I established it, and so I continued that, and that still continues where each PI has a budget of his or her own. So that came in and that was one of the changes that coincided with my becoming scientific director, and we had to modify those back and forth.

It used to be that we would recruit new independent investigators, new PIs were most often, as in my case, people who had been a successful postdoctoral fellow, would then become an independent PI. We established a different process by which when we had space and money for a new PI, it was advertised nationally and there was a committee who would select, interview, and have a seminar for the candidates. They would make a recommendation and the scientific director would approve the recommendation. So we changed the process of appointing new independent investigators. That was true in the Heart Institute. It was also generally true throughout NIH. The scientific directors meet once a month with the deputy director for intramural research, NIH deputy director. And so I would be involved there.

For example, the first research publication policy that NIH drew up, I chaired the committee of scientific directors to draw up that sort of thing. And related to that was the recruitment of new independent investigators—it shouldn't just be a successful postdoc, but we should look further. And I made a shift more toward biophysics. We had biophysics going on, but it was a relatively small group and we expanded the biophysics group, which now is much larger, which Balaban has continued. He's done this even more than I, creating new organizational categories. As biomedical science alters the direction, it becomes appropriate to have new areas of research, and Balaban has done that much more than I did it.

But mostly, it was overseeing the investigators, and saying when someone who was tenure track—like at a university, you come in, you're not tenured until you accomplish something, it's true here at NIH. You're not tenured, normally. You can recruit a senior scientist from outside who would go into a tenured position, but our new appointments are tenure-track positions. And so, you supervise the tenure track and the scientific director has the final say on conversion to a tenure position, and the budgets of the individuals are certainly under the control of the scientific director. There always are problems that arise in the scientific director's office, and it could be someone who's faking their data. It happens not often, but it happens.

SS: Did it happen under you?

EK: Someone was accused of it, I think incorrectly, and a lawsuit was threatened against the person, against me as scientific director, and against the Institute. I was going to be sued for a million dollars, but the judge never heard it. He threw it out, never heard the case. But it was an incorrect allegation. There were problems between some people that existed.

We have summer students, college students, sometimes high school students, come in and work for the summer. And we have a program where someone, after college, can spend a year here, extend it for two years, before, as they applied. It's kind of a gap year, when they're applying to graduate school or medical school. We had such a person, a woman, who accused not her mentor, but a colleague of her mentor of taking her into his car and raping her. And it turned out that was not true, that she had made allegations continual about all sorts of people. And that sort of thing comes up from time to time.

SS: And that fell on your desk?

EK: It fell on my desk, yes. But they're not major activities of a scientific director. It's really responsible for the science. Each institute has a board, an external reviewing board, that reviews extramural and intramural. So the scientific director also meets once a year and describes what the activities of the program have been and successes have been. So mainly it's a question of allocating funds, approving large equipment purchases which individual budgets are not sufficient for, and recommending promotions within the civil service or Commission Corps. There are promotions that go on and so you recommend promotions, which are approved at the NIH level.

SS: You had to stop doing a lot of your research. You had the lab but you weren't as involved in research, is that correct, when you were scientific director?

EK: Not as involved, yes. But I was here every day. I came into the lab every day.

SS: Did you enjoy being scientific director or did you miss the lab?

EK: I missed the lab. When I applied for the position, it was really because I've had scientific directors who were necessary for me to succeed, and I felt an obligation to do it, in part. There was also one other candidate who I would not want to have as my scientific director, so that was another reason why I applied for the position.

SS: What did you accomplish as scientific director?

EK: The director can transfer up to a certain amount, 10 percent, whatever it is, into the intramural budget without approval of anyone else, and with the approval of the NIH director, maybe more. I was able to get more money into the intramural program, significantly more money and more positions. Anyway, positions that are funded, whether it's a secretary or a technician or independent investigator, NIH had a certain number of positions, each institute does. And the intramural program, I was able to get expansion of the positions in the intramural program.

Before I was scientific director I had suggested a new faculty policy for outstanding tenured principal investigators, which ultimately became the Senior Biomedical Research Service with higher pay than the GS system. As scientific director, I closed the Surgery Branch, which no longer did research, and used the space to open the first bone marrow/stem cell transplant facility at NIH and hired John Barrett as director. We modified the selection process for tenure track scientists, requiring national advertisement, and recruited a number of outstanding scientists from outside NHLBI.

SS: That's quite a success.

EK: So, those things happened. The independent investigators are independent investigators, so I didn't get involved in their research. Every four years, every independent investigator gets reviewed by an external board, and so I chose the members of the external board as covering the fields that we were involved in intramurally. They would then review and I would review their review, and almost always there was no disagreement between the committee's opinion of the scientist and mine. But to the extent there was, I would have to either go along with the committee or convince them that they were a little bit too harsh in their assessment of the investigator. So I would interact with this external committee. They were independent and they wrote their review, which I had nothing to do with. I sat in on the meeting where they had a presentation for the investigator, but they ran the meeting, not I.

SS: Were they reviewing the investigator for a raise or for staying on in the institution or not?

EK: Well, in the extreme, it could be to let them go, but you need to have a basis for that. It was really for the amount of support that they would get. They might get a promotion, but also a budget increase or a decrease in budget or no change in budget.

SS: Based on their research?

EK: Based on their research. Totally based on their research, yes. There are independent investigators at all the institutes who lose their independent position. They're not necessarily fired, but they move to become a staff scientist in someone else's laboratory where they're working under someone else. And so, yes, there were a number of people that I convinced to retire if they were at the right age and appropriate age, or took away their independent resources.

SS: Is that if they weren't doing enough—

EK: Research. They weren't doing enough research getting published. I mean, the question is, what have you gotten published? You could be working like crazy all day for twenty years in the laboratory. If you never publish a paper, it's useless. And so you're basically based on the quality—mostly quality, but also quantity of publications.

SS: Much like academia.

EK: Yes, that's like academia. We do it the same way. But this outside committee makes recommendations. An individual gets reviewed every four years. There's someone getting reviewed every year. And based on those reviews, the scientific director makes decisions to expand, reduce, support, or eliminate support altogether. I certainly have never fired anyone. If you have a tenured investigator, to fire that tenured investigator is difficult. At a university, if he's not getting research grants, he can't do research, so he gets more of a teaching responsibility or administrative responsibilities or something like that. So, here, we take away his independence and put him under someone else; that happened for a few people while I was scientific director.

SS: That makes sense. So, following your career, you put ten years in and you felt that was good enough. You had an obligation, but you fulfilled it.

EK: Yes.

SS: Sounds like you did a wonderful job and had a successful time of being scientific director. But then did you go back to your lab?

EK: Back to the lab, yes.

SS: As director of your lab?

EK: Yes, I stayed head of the lab, the chief.

SS: And then were you still editing the *JBC*?

EK: No, I resigned from that after the first couple of years of being scientific director, because it was just too much.

SS: Right, I see that in 1993. [Referring to notes]

EK: It was just too much to do it all.

SS: I would imagine.

EK: So, Balaban, when he took over as scientific director, reorganized the system. He thought there were too many lab chiefs. We called them a lab if it was basic science, or a branch if it were clinical. Clinical branches. So we had a cardiology branch and biochemistry laboratory. He combined a number of these into what he called a center. So, for example, the biochemistry and the biophysics laboratory became a Center. And I was, for a short period of time, director of a center, which was a little bit larger than the laboratory. But then Clare Waterman was recruited. Dr. Waterman, was recruited directly, as an independent investigator. She was well established and she became the center director about fifteen years ago or so. I don't know what it's been. Ten or fifteen years or so. And I became the lab chief. And then part of the reorganization is that essentially, the laboratory that I was chief of had about three or four independent investigators. They are now still independent investigators, but they and I are all under the center director, who's Clare Waterman. She's an extremely good scientist. She just got elected to the National Academy at a relatively young age. She's in her forties, I guess, late forties. I'm not sure. So there's some reorganization that's gone on so that now I'm responsible just for the three people who work for me directly.

SS: Speaking of the National Academy of Sciences, that is an honor that has been bestowed on you as well.

EK: Yes.

SS: Tell me something about that. Is that something you ever envisioned? Did that change your life? How has that impacted you?

EK: Well, I don't think it's had any impact. It came, as it often does, relatively late in someone's career. When was I elected to the National Academy? I was probably in my sixties. I was already scientific director, so it was after 1989. It was in the early nineties.

SS: Oh, here it is, 1990.

EK: Nineteen-ninety, yes. So it's a year after I was scientific director. So I was sixty-two years old.

SS: You were young.

EK: So, obviously it had no effect on my career. I mean, at that point, I was scientific director. It's commonly the case. Not always, but commonly the case that people are elected at that stage in their career. There are many, many more people who are the equal of the members of the Academy who never get elected, that there's a numerical quota each year, only so many can be elected. And there are many, many more qualified people than the quota allows.

SS: I'm not familiar with the process. You have to be nominated by somebody?

EK: You're nominated by a member of the Academy. Well, by two members. The Academy is broken up into numerous sections. The biochemistry section is Section 21. There are multiple sections. When you're nominated, it's an annual vote within your section. You need a certain percentage. The top number of those go up to the class, so there's a section and then there are classes, and the biomedical research section are all in the same class. There's physiology, pharmacology, biochemistry, biophysics would all be separate sections. When you get to a certain range of the section vote, you get to a class vote, and there's an annual vote by the class. And you get a certain vote, you get high enough in the class vote, you go to the general vote for all the Academy. At that point, members of the class usually get in by order if they're top of the class, top group, they're going to get in. I mean, if I get to vote for an economist, how am I going to vote for an economist? So it's a long process, and if you don't go from section to class within a couple of years, you have to be nominated over again. It repeats itself.

So, in my case, it certainly made no difference in my career at all. I was already scientific director. I was sixty-two years old. There used to be mandatory retirement at age sixty-seven in the federal government, and so that has been abolished. The mandatory retirement has been abolished.

I don't think there should be a National Academy of Science. I accepted the nomination—well, not nomination. I didn't know I was nominated. Appointment. I guess for two reasons. First of all, to some extent, the honor was something that—like most people—I couldn't refuse. But also because Jack Buchanan and Chris Anfinsen, both of whom were members, nominated me and pushed to get me elected. You really only get through the process, unless you're someone really unusual about to win a Nobel Prize or something, because the people who nominate you ask people in your section and class to vote for you. So Buchanan and Anfinsen had nominated me, had worked to get me elected. I felt an obligation to them as well.

There are people who have resigned or refused to get in and have resigned for one reason or another. I don't see any point in bothering to resign. But there are so many people as qualified as the members who are not members, that I sometimes think there should not be an NAS. There's a catalog with a list of all the members and then there's a list of all those who were members who are no longer living. All dead members, deceased members, are listed. It goes back to I think when Lincoln was President, 1863, you're all listed. That's unusual enough. We go through all the deceased people. And then you go through former members. These are people who resigned. You still get listed as a member. (Laughter.)

SS: You can't escape.

EK: You can't get rid of it. So Richard Feynman, who's someone who writes frequently in the newspapers and two charter members have resigned. These are people who have resigned.

SS: So they resigned because it's non-egalitarian?

EK: The ones with the asterisks with a superscript one, which is all of them but one, are people who resigned. Two became foreign nationals and lost their membership because they changed their nationality.

SS: Well, that's quite a prestigious group that you're a member of.

EK: Yes. But it's ridiculous that you can't resign. So it goes back, I think it was during Lincoln's presidency when the Academy was begun. Then you die, but you're always going to be listed.

SS: You're in the book.

EK: You're in the book forever.

SS: Well then what would you consider your legacy? What are you proud to have as your legacy, if not being in the book of the National Academy of Sciences?

EK: Oh, the legacy is really the myosin contribution. The major contribution was showing that there's not just a single kind of myosin, but there's another kind of myosin, which has now expanded to thirty-five different kinds of myosin, each of which have different functions in the cell which are still being actively studied by an increasing number of people. So the myosin-I is certainly, that's what would have gotten me elected to the Academy, I assume. I didn't see the nomination.

SS: And your lab is still publishing. Articles are still coming out.

EK: Yes, we're still publishing. We published in 2018.

SS: That's amazing.

EK: We haven't yet published in 2019. (Laughter.)

SS: Well, get on it! (Laughter.)

EK: Well, it's very common and I don't know how familiar you are, but in scientific journals, almost every paper is sent back for revision or rejected. So we've got two papers now under revision that were submitted.

SS: So you'll be published again soon, this year.

EK: We should hopefully publish at least those two this year, yes.

SS: On the same subject matter?

EK: On the same subject matter, yes. One of them is being authored by a person who plans to retire in end of April, staff scientist retiring at end of April, in collaboration with someone at the

University of Minnesota. So that will end that research, so now I'm going to have to be down to two people, who happen to be husband and wife.

SS: That tradition still continues, of having husbands and wives working together here.

EK: Well, they'll continue, yes. They're just about fifty years old now. How much longer I'll be here, who knows? I don't plan to leave.

SS: You don't plan to retire?

EK: No. Well, I did retire a year ago.

SS: You're emeritus.

EK: I'm emeritus.

SS: But you come in every day?

EK: I come in every day. I work much less than I used to work, obviously, because I have a group of three people, not eight people, no administrative responsibilities at all besides that. But I still come in every day. And as I said two of these people will continue to work after I'm gone.

SS: Right. So that will be carried on, and the work in your field, as you say, is still quite active and they're discovering new things.

EK: Yes. They're not independent investigators. They're going to have to work under some independent investigator.

SS: Right, but I mean worldwide.

EK: In the field. Worldwide, the field will go on.

SS: Yes. Are there new directions in the field that you're excited about or want to learn?

EK: Well, we're not doing research in the most exciting or novel areas. I just don't have the resources to compete in those things. There are ten or twelve different classes of myosins in human cells. What are all these myosins doing? Some, we know are involved in division of the cell. Some are involved in the endocytosis process I mentioned. The opposite of endocytosis is a secretion, an exocytosis process, where—with insulin, for example, is synthesized in the pancreas, secreted by the pancreas—myosin is involved in that. But which myosins, and how do they function? How do they do it? So it's the function now, not the existence of the myosin, not the comparisons of them, not their polymerization or non-polymerization properties, but the functions of them, which is the major activity. And the molecular basis of their localization. If you have a myosin that binds to the cell membrane, how does it bind? What is the active part of the myosin that binds? We're looking at some of that.

SS: Interesting. What about for NHLBI or NIH? What about new directions there, do you see? Where do you think they're going?

EK: You should talk to Dr. Balaban.

SS: Okay.

EK: Well, I think clearly, a major direction that NHLBI and all of NIH is going is the molecular basis of disease. We now are having an increasing understanding of the basic physiology and biochemistry. For example, we now know that this is how sugars are metabolized. We knew that years ago, but all the various things. How and what's the abnormality in diseases. Some which are genetic diseases, you can identify the gene, and you know what that gene is—the sequence for a protein and the protein has certain properties. So you already know that, and how does that relate to the disease?

I quoted this earlier, Dr. Shannon said diseases will be cured only when the science produces fundamental understanding of physiology and pathophysiology. We don't now completely understand, but we understand very much more than we did fifty years ago. And so now, it's the relation to the disease. How does the disease derive? What's the molecular abnormality, the cellular abnormality, that leads to disease? And if you knew that, you'd have a more direct way and you could cure the disease. If you have a genetic disease, you're missing a protein that's required. If you can supply that—people trying gene therapy, mostly unsuccessfully, but trying.

But you can supply that gene. In the laboratory, you can do that. You can correct the genetic abnormality by injecting the gene that secretes the correct protein, and correct it. And so, this research now is really increasing, but not exclusively—there's still a lot of basic science that needs to be done. But certainly, the molecular basis for diseases is the area that ought to be and is expanding.

SS: And that will be found out through basic science, through what you talked about earlier, basic science research and biochemistry.

EK: Yes.

SS: Your field, really.

EK: Yes. But, you know, if you take the sixty-five years I've been in science, a lot has been discovered which now hopefully can be applied to diseases. There's still a lot to discover, but there's a lot that has been discovered.

SS: Yes, that's fascinating. Would you have any advice to young scientists starting out today?

EK: Well, it's difficult. There are now more PhDs being given and MDs in research than there are positions. So it's difficult. The people who we train now have trouble finding jobs, a lot of them do. I think I'd just have to say keep at it. You know, I think they should choose wisely based on their own interests and experience. It's very difficult to give advice because the field is changing and the number of positions are fewer. There was this huge expansion in the fifties, sixties, and seventies in biomedical research. Pharmaceutical companies now are, of course, still expanding. So it's a question of: If you want to go into research, you've got to be sure that's what you want to do, if you're going to go into academic research. I think the advice is that first of all, be sure that's what you want to do, and consider other options as well. But once you've made that decision, all you have to do is work as hard as you can.

SS: Sounds like you did that. I mean, you worked very hard but you also found yourself in these positions, with, true, some serendipity and luck, as you say. Following your nose along the way into research. It sounds like you didn't have a clear plan for what exactly you were going to research—things opened up for you. Maybe another way to say that is to ask, how did you do it? How did you forge your career? I know you've been telling me that for two hours.

EK: Well, I chose biochemistry in ignorance of what it really was, because it was not taught at the undergraduate level at most places, certainly at Penn. It wasn't a big thing in this country at all. It was much more in Germany. You had to know German and French to read papers. Papers were published in German and French as well as English. And you had to be able to read, at least, German and French.

SS: Could you?

EK: Well, you had to take exams in two languages to get a PhD degree.

SS: I didn't ask you that earlier. I should have. Do you read and speak French and German?

EK: No, I don't read and speak either of them. I never spoke it, but I could read scientific German and French. And there were translators at NIH on the staff. The library staff had people who translated for you. But that rapidly changed through the fifties, and America became the center of research, and journals were in English, except one French journal that refused to give up French. There was a journal, which had published in three languages: German, French, and English. But when I was a graduate student and early postdoc, the abstract had to be in English. Then shortly thereafter, they published in English only, so French and German scientists need to be fluent in English, but we don't have to be fluent in French or German. But anyway, I chose biochemistry. It seemed more interesting, and I was advised to do it by a friend of my brother's.

There, I had three choices of mentors. There were three—four people, I guess. There was the department chairman, D. Wright Wilson; Jack Buchanan, whom I chose to work with; Sam Gurin; and a young fellow just out of graduate school named Minor Coon. His name is Justin—Jud, he was called—Jud Coon. I used to know why, but I forgot. Anyway, there were these four choices. I chose Buchanan because his field seemed the most interesting. It was the precursor of understanding DNA and RNA, the two nucleic acids, which were obviously of essential importance in the cell. One of the other three people chose Gurin and the other chose Dr. Wilson, and Jud Coon was so young that we did not consider him—but he did some important work as a graduate student.

Do you know what amino acids are? The proteins are made from amino acids, and there are ten essential amino acids that you have to have in your diet because humans can't make them. Jud Coon got his PhD with someone who was at the University of Illinois, William Rose, who described a number of these ten amino acids. The graduate students would go on special diets, not including a given amino acid.

SS: They were the lab rats.

EK: They were the lab rats, yes. Anyway, so he was one of those graduate students who got his degree in the course of defining the ten amino acids. They had to go on special diets, give their feces and urine for analysis.

Anyway, the status was such that when I was a graduate student, there was no textbook. We learned biochemistry by reading the original papers in the journals. Anyway, I chose to work with Buchanan, and then later with Anfinsen, which as I said, was by chance. I mean, I was going to go to Barker's lab, and he was studying microbial biochemistry, microbes, which make fundamental contributions to biochemistry because you can study the intact cell and lysates more easily than you can study mammalian cells. Now you can do it with mammalian cells, with tissue culture or whatever. But techniques for culturing mammalian cells were not available, had not been discovered yet.

So when I came to NIH, I agreed to work on a total novel, for me, new field altogether. I had never worked on lipid metabolism. And I agreed to do that for three years, and I did it for a bit longer because it was going successfully, but I really had no interest in it, really. And then by accident, the myosin turned up totally by accident, and that was important, extremely. First of all, I had to prove it was right. The people in the field thought it was an artifact for ten years, and so I had to prove that it was there, which would be easier today than it was then. But then it went on from there, so it's always been an expanding field.

SS: Kept your interest.

EK: Kept my interest, yes.

SS: Well, I think the world should thank you for the work that you've done. Do you have any regrets about your career that you'd want to put in the record?

EK: No, none at all. Maybe I regret I didn't buy the house in San Diego.

SS: But you're not unhappy that you didn't teach there?

EK: No, I think that would have been a mistake. For me certainly, as for everyone at NIH in senior investigators positions, the fact that you don't have to write a grant is important. You are responsible, you are reviewed by an external review board. Your support depends upon your accomplishments, and they can be removed and are removed, completely from some people, but it's a lot easier than writing grant applications and teaching and trying to do research. So the faculty members, they're the ones who deserve a lot of credit for being able to teach students as well as do their research where they have to apply for money. So, no, I have no regrets at all.

SS: Are there any other fulfillments you'd like to talk about besides your work in myosin and actin that you've spoken about?

EK: You mean scientific work?

SS: Well, any, actually. You spoke about your ten years as scientific director. I think that that was very successful.

EK: I think also that as scientific director, I made some contributions to the NIH. As I said, the scientific directors met once a month under the leadership of the deputy director for intramural research at NIH. And several committees there do things, and I think I made some useful contributions to NIH by virtue of being a scientific director, both on the policies of research, publication policies, authorship policies, and some things are routine, such as supporting staff. We have a lot of stuff now that's done differently than it used to be. We had a graphic arts department. Now we can do everything on the computer. But it used to be your papers were published, you had figures in papers, they were done by a different department. And I was involved in a committee that reorganized those departments to make them more efficient. So they were useful contributions to the NIH, but nothing special.

SS: It must feel good to contribute to this major institution that has been such a part of your life and also helps so many people in the country and world for its research. Is there anything else you'd like to add that I haven't asked you today?

EK: You haven't asked about my wife, who keeps me going.

SS: Well, I did ask you about your wife early on, but tell me more about her and your daughters.

EK: Well, as my wife would tell you—we've been married since 1950, so it's been sixty-eight years—she's made my lunch every day. (Laughter.) I carry my lunch to work. I still do. You may have seen me when I came in.

SS: Yes, very sweet.

EK: And I have two daughters. One is a lawyer who practiced law for several years, and then disliked it intensely. She was married and divorced, but married a very successful lawyer. She lives in Washington, in Georgetown.

SS: What's her name?

EK: Sarah, whom I taught to read. She's the younger of the two, and the older one's Elizabeth. Sarah came down one day and my wife Mickey and I and Betsy "Elizabeth" were all reading, and she said, "I'm the only one who can't read." So I taught her to read before she entered kindergarten. Sarah was called Sarah; and I was Edward, called Ed; and Mickey was Muriel, called Mickey; and Betsy was Elizabeth, called Betsy. And Sarah was the only one that didn't have a nickname, so we gave her a nickname, Sally, which she used until she got out of high school. In college, she was Sarah, and everyone knows her as Sarah except for the immediate family. And she's been very actively involved in a variety of volunteer activities while her husband makes a good living as a lawyer.

Betsy, the older daughter, got a doctorate in education and ended up working at Phillips Academy in Andover, in administrative positions: counselor, college counselor and advisor and then I don't know what her final position was. She retired last year. So my daughter retired before I did. Well, it's the same year I did, but much younger. Anyway, she has two children, two daughters. One got her MD and is now working as a physician in San Francisco. She is married to a lawyer who clerked at the circuit court, not the Supreme Court. And they just had a daughter, so I'm now a great-grandfather.

SS: Well, congratulations.

EK: Thank you. She was born in April, I think, sometime. And the other one's not married, working in Boston for a health organization. My other daughter, Sarah, Sally, has a daughter and a son. The son is graduating from Stanford Law School in June.

SS: You have a lot of lawyers and doctors in your family.

EK: Yes. He's married, and he has a younger sister who's in New Orleans in education. She has a master's degree in counseling. So they're all fine, did well.

SS: So in addition to your career here at NIH, you're a good example for all of us. What is your secret? So you have longevity, you have a long marriage, you have raised well-adjusted, successful children and now grandchildren. Do you have any words of wisdom to pass along to the rest of us?

EK: When my granddaughter, the one who's now a physician, when she got married, she asked my wife and me to speak at the marriage. It was an interesting ceremony, actually. The week before I went out there, I was having lunch with some people here and someone said, "You've lived in your house for so long, you've been married for so long, you've had your job for so long. How do you do it?" And I said, "You have to make the right choices." And so, that's what I said at the wedding. I said, "These two have obviously made the right choices." My right choice in the house wasn't the house, but its location so I could walk to work, which I have enjoyed doing ever since.

And choosing the right wife was extremely important. I mean, I wouldn't have been able to get through graduate school. Well, I guess if I weren't married, I could have gotten through graduate school with what I was making, but buying the house and et cetera. When we bought that house, it was $23,000, as I said, while I was making $2,000 or something like that. So without my wife's income as well, we couldn't have bought the house. Most people were buying houses much further out than here, people my age, and they couldn't afford to buy the houses here. So, but its location, we could afford it because my wife was working and I was working. I stayed there because I liked walking to work rather than driving to work. And our children, I don't know. They did fine all the way through school.

SS: So it's the right decision, but it sounds like maybe you're being too humble. It sounds like there are some other ingredients in there, for example, stick-to-it-iveness, stubbornness, doggedness, and belief. I don't know what else. Does any of that ring a bell?

EK: You'd have to ask my wife. An interview with her might be useful.

SS: Well, I'll pass that along. Thank you, Dr. Korn, I've enjoyed our conversation.

[End of interview]