

**NATIONAL HEART, LUNG, AND BLOOD INSTITUTE
ORAL HISTORY PROJECT**

INTERVIEW WITH

Robert S. Adelstein

OCTOBER 4, 2019



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Robert S. Adelstein

Biographical Statement

Dr. Robert S. Adelstein was born in Brooklyn, New York, in 1934. He went to Stuyvesant High School in Manhattan. He received his A.B. from Princeton University in 1955, and his M.D. from Harvard Medical School in 1959. Dr. Adelstein completed his medical internship and residency at both Bellevue Hospital in New York and Duke Medical Center in Durham, North Carolina (1959-61), and a senior medical residency at Duke (1964-65). He then completed a postdoctoral fellowship with Dr. Edmond Fischer in the Department of Biochemistry at the University of Washington in Seattle, Washington. Having served as a Research Associate in the Laboratory of Cellular Physiology and Metabolism at the NHI's National Heart Institute from 1961-64, he returned to the Institute in 1966 and has stayed for fifty-three years. He has held the positions of Senior Investigator in the Laboratory of Biochemistry, Section Head of Molecular Cardiology, and Chief of the Laboratory of Molecular Cardiology in the National Heart Lung and Blood Institute, a position he holds today. In addition, Dr. Adelstein was assistant Clinical Professor of Medicine at Georgetown University Medical School and served as a faculty member in the Biochemistry Department and president for the Foundation for the Advanced Education in the Sciences (FAES) at NIH. Other distinguished appointments and positions he has held include Chairman of the Committee of Concerned Scientists (1977-79), Chairman of the Gordon Conference on Muscle Proteins (1981), and the Editorial Board of *The Journal of Biological Chemistry* for twenty years. During his career at the NIH, Dr. Adelstein has won many awards, including the United States Public Health Service Medal, an NIH Merit Award, and the NHLBI Mentor of the Year Award.

Interview Synopsis

Dr. Adelstein begins the interview with memories of growing up in New York, his early interest in science, and continues with memories from his undergraduate years at Princeton University. He also remembers his time at Harvard Medical School, including a summer he spent sailing on the North Shore of Boston at Manchester by the Sea. Dr. Adelstein's career has spanned important cultural moments which he shares: medical residency at Duke University Hospital in North Carolina during the crucial time of racial segregation and integration, a postdoctoral fellowship in Washington State during the heyday of the 1960s, and attending the 1963 March on Washington. Dr. Adelstein also remembers being at the NIH at a crucial time in its history. He was a member of Chris Anfinsen's lab in the 1960s. Anfinsen went on to win the Nobel Prize in Chemistry in 1972. In addition, he worked with and/or knew other significant figures in NIH's history, such as Nobel Prize winner Marshall Nirenberg, Richard Berliner, Eugene Braunwald, Martha Vaughn, and Jack Orloff. He talks about his work with the Committee of Concerned Scientists and his travels to Russia. Dr. Adelstein discusses his important work and discoveries, including his groundbreaking work phosphorylating myosin; DNA, RNA, and genetics; and the rare cardiac abnormality of Pentalogy of Cantrell. He closes the interview by mentioning the scientists who have worked with him in his lab over the years, including Mary Anne Conti, Xuefei Ma, and Sachiyo Kawamoto. He also mentions those whom he considers to be mentors and the many scientists he has mentored.

NHLBI Oral History Project
Interview with Dr. Robert S. Adelstein
Conducted on October 4, 2019, by Sheree Scarborough

SS: Today is October 4, 2019. This is Sheree Scarborough. I'm interviewing Dr. Robert S. Adelstein for NHLBI. We're in his office in Building 10 on the NIH campus. Dr. Adelstein, I've read some about your career, but I don't know when and where you were born.

RA: I was born in Brooklyn, New York, on January 16, 1934.

SS: Tell me some about your childhood.

RA: I grew up in Brooklyn, New York, which I thought was a wonderful place to grow up. I started my education there, at the Yeshiva of Flatbush. But then my mother died unexpectedly when I was twelve years old, and when my father remarried we moved to Manhattan. I enjoyed living in Manhattan because of all the cultural things that were easily available, such as museums and galleries and I went to Stuyvesant High School, which was a special science school in the city. From Stuyvesant I went on to college at Princeton, and then followed that with medical school at Harvard.

SS: Is that what made you interested in science, going to that high school?

RA: Absolutely. Actually even when I was in grammar school I was interested in science. I remember I once won the eighth grade prize for a science project at my grammar school. Stuyvesant was geared toward science and I began to enjoy it very much. It was a wonderful school at the time and remains so today. It was a nice mixture of students from a variety of backgrounds, at a time when there wasn't much diversity in New York. Then I went on from there to college.

SS: Tell me something about your college experience.

RA: I went to Princeton University. I must say that one of the things I particularly like today about Princeton is how much it has improved, because when I went there it was still sort of an isolated college. There were no women, which I think was not a good thing, and it didn't have a very forward looking leadership at the time I entered, which was 1951. Subsequently, I'm pleased to say that Princeton, particularly under the direction of Shirley Tilghman, who became president much later on, became a much more diversified university. I have returned to Princeton a number of times, both with my children, and particularly with my grandchildren, some of whom hope to go there. Following Princeton, I went on to medical school in Boston.

SS: So you got your B.A.? I guess it was called an A.B.

RA: Yes, it was called an A.B. at Princeton.

SS: In 1955?

RA: '55.

SS: What was that in?

RA: It was just in arts and sciences. I majored in biology at the time.

SS: What prompted you to do that?

RA: My interest in science and also the realization that I was probably going to go on to medical school, although it wasn't necessary. It was something I thought over for a while. I was actually thinking of going into the Special Program in Humanities, but in the end I decided to major in Biology.

SS: Did you have any special professors or classes?

RA: There was Buzzer Hall who was a very well known professor at the time and taught history. He liked to emphasize that history is very much determined by the characters who were making the human element of history, for example people like Winston Churchill or Adolph Hitler. I suppose to a certain extent we're seeing a little bit of this today, too.

SS: Yes. You were in college just after World War II.

RA: Yes. I was in college from '51 to '55.

SS: A few years after, so that was on everyone's mind.

RA: Yes, although we had moved on. It was a new era. [Dwight D.] Eisenhower was elected president, although I remember being very devoted to [Adlai] Stevenson at the time, who was not elected.

SS: And that was during the Korean War?

RA: The Korean War started in '51, right. I got out in '55 and went to medical school.

SS: Then you went to Harvard Medical School?

RA: Right. My brother had been at Harvard before, and particularly recommended it, and I followed in his footsteps in that sense. He had made an outstanding record for himself.

SS: What is his name?

RA: My brother's name is James Adelstein. He went on to become a distinguished professor at Harvard. At one time he was the academic dean. This was long after I was there. [Daniel]

Tosteson was the dean at the time had a few other sub-deans, and one of them was my brother, who was the academic dean.

SS: What was his specialty?

RA: He was interested in radiation biology, which became part of the field of molecular medicine in a sense.

SS: Tell me about your career at Harvard Medical School.

RA: My career as a medical student was not particularly distinguished in any way. I did author one of the skits for the second year show at the time. The second year always put on a comic show during the sophomore year and I wrote one of the skits, which paraphrased "My Fair Lady." Except instead of: "The rain in Spain falls mainly on the plane," I wrote "Clem the Phlegm condemned the ancient Schlemm." The Canal of Schlemm is a canal, in the eye.

SS: I didn't know you were a comedy writer. (Laughter.)

RA: I was trying to be a comedy writer at the time. Actually the skit for the second year show was quite successful. We even have a recording of it someplace. I had a roommate who was with me for the four years I was there, Bob Carey. Unfortunately he's passed away. And from medical school I went to take an internship as well as residencies.

SS: Did you specialize in medical school?

RA: No. Medical school was mostly general medicine. At the time I had no desire to become a surgeon, although I enjoyed surgery when I took it as a course. But I always was bent toward doing research and began carrying out research projects during medical school and during internship.

SS: I'm curious, what made you feel that?

RA: I had a strong feeling that if I went into the practice of medicine I could only help a limited number of people but if I could contribute to an important finding in basic research, my contribution would have more of an impact.

I spent my first summer in medical school at Harvard Medical School doing research on blood platelets, and my brother and a friend of his rented a little wooden shack, which they lived in on the North Shore of Boston, in a place called Manchester by the Sea, which actually has become well known because of the movie. The shack was right on the water there and they had a sailboat. And I had an idyllic time my first summer going up with them on the old Boston & Maine Railroad and getting off, and walking back, to the fishing shack, and then going sailing. They had a little sailboat at the time and I thought that was a terrific way to spend the summer.

However, at the end of my sophomore year I got a call from an uncle of mine who was working at the National Institutes of Health. This was an uncle who was in private practice in Brooklyn, New York, and shocked the family by leaving his private practice to go to some place, which no one had ever particularly heard of—Bethesda, Maryland—to become the clinical director of the National Arthritis Institute. That's what it was called at the time. I was all set for my second summer to go back to the North Shore. And he gave me a call, and he said, "Bobby, you have got to come here to the National Institutes and work with this fellow by the name of Gordon Tomkins because he is a very exciting investigator and I think you would enjoy it very much." But I was completely convinced that I wanted to go back to the North Shore and go sailing. I actually had applied for a summer fellowship at Harvard so I told him that if I didn't get the fellowship I would come.

I got the fellowship, called him up to tell him I got the fellowship. But at the end of the call, for reasons I don't particularly understand, I decided to go to Washington. Part of it was his ability to convince people to do things like that. So that was my first dealing with the National Institutes of Health. I came down here as a medical student. It must have been around 1957.

SS: Fascinating story. What was your uncle's name?

RA: My uncle's name was Joe Bunim and he was the clinical director at that time of the Arthritis Institute [National Institute of Arthritis and Metabolic Diseases]. Actually there

used to be a Bunim Room named after him. I'm not clear what's happened to it. It may still be around somewhere.

SS: Tell me about your first experience at NIH.

RA: It made a major impact on my life because Gordon Tomkins, the person who I had come to work with, was such an unusual person. He was very full of life. He would work in the laboratory and sing Cole Porter songs and get very excited about things. He went on to have a very distinguished career both at the NIH as well as at [University of California] San Francisco. Unfortunately, he had an acoustic neuroma, a tumor in his ear. In addition to being an extraordinary scientist he was a wonderful musician. He played a number of instruments including the harpsichord, which he built himself, as well as the saxophone. Unfortunately, he got this acoustic neuroma and he was operated on to remove it at Mt. Sinai in New York, and unfortunately he died following the operation probably because they didn't monitor him closely enough as to whether his wound was going to bleed. So he died at a relatively young age. But he was inspirational to a number of scientists who came here. That really hooked me on wanting to do research. If you can sing songs from Hollywood shows and do research at the same time it sounded like a lot of fun.

When I was finishing my internship and residency I applied to come to the NIH for what was then called the Research Associate program. The benefit of coming to the NIH, was because there was still a doctors' draft at that time. So either I was going to have to go work in some hospital or I could get the same kind of credit, which you needed as a

physician, by doing research at the NIH. So I applied to come to the NIH and in fact I became a Research Associate at the NIH in the National Heart, Institute, as it was called at the time.

SS: That was after you got your medical degree?

RA: That was after I finished my medical training and finished one year of internship and one year of residency. I returned later on afterwards to do a final year of residency.

SS: So for your residency, you went to two different places?

RA: Right. So what happened was a little more complicated. I was an intern at Bellevue Hospital. I was put together with a fellow—we were joint interns on the ward—who was supposed to take his internship at Duke Hospital, but his father had died and he didn't want to leave his mother alone in New York City. But he felt guilty because he knew that Duke was run as a very tight ship by an outstanding director of medicine, Eugene Stead, and he knew that they only had so many positions. So he asked me if I would I be willing to consider going to Duke for the second half of my internship.

The chief of medicine at Bellevue at the time was a very well known, outstanding physician and author by the name of Lewis Thomas. I went to him and asked him what he thought of the idea, since Duke was short one person for six months. He thought it was reasonable and he approved of it. So I went down to Duke and met Dr. Stead who was

known as a little bit of a tyrant at the time. This was a change for me. I had never been in the Deep South before. I had never seen segregation in force before. There were Black wards, which they called "colored," and there were White wards. It was completely segregated. I must say I was put off about that. But the program for the training of physicians looked very interesting. I remember I said to Dr. Stead, "I'd be happy to come and take this fellow's place." And I remember Dr. Stead said to me, "Well, Dr. Adelstein," with his thick southern drawl, "if you want to come here that would be fine, and if you didn't come that would be fine, too." So I took it as a challenge mostly because I had driven down with my faulty GM Corvair car, which kept breaking down ("Unsafe at any Speed" according to my Princeton classmate, Ralph Nader).

And it was an interesting challenge because in contrast to my internship at Bellevue Hospital where the idea is you learn by doing, this was a much more academically-oriented internship where you had, for example, we had noon meetings three or four times a week where various medical topics were being discussed. In fact, they even met on Sunday. They used have what was called "Sunday School" to discuss research projects and things like that. So it was a lot more intensive than it was at Bellevue. You were also on five nights a week and every other weekend, so the demands on you were even greater. I enjoyed it in a lot of ways, particularly for its academic approach. Then I went back to New York where I did a year of residency back at Bellevue and then I came to the NIH.

SS: When you say residency, is that just general medicine?

RA: Yes. It used to be that you had internship and then you needed two years of residency to qualify for internal medicine. Now it's changed. I think internship is now considered residency, so now there's something like three years of residency. I interrupted my residency at the end of the second year to come to the NIH. There was no reason I had to take all three years in a row. So I interrupted it at the time to come and become a Research Associate at the NIH. At the end of my time at the NIH I did two things. I did a residency at Duke and then I did a postdoctoral fellowship at the University of Washington in Seattle, Washington.

When I was looking for a place to do my residency, I remembered how much I did like Duke and I thought that I would do a year of residency there and then I would work with a Professor by the name of Charles Tanford, for my postdoctoral fellowship. I had been given the opportunity by the NIH to finish a residency and then spend another year to do more postdoctoral training. I went, thinking I would spend two years at Duke—one clinical and one research—with a scientist by the name of Charles Tanford who was a biophysicist and a quantitative biologist. I had never been very good with quantitation and I had this bizarre theory that I should do things which I didn't like or was having trouble doing.

I went down to Duke to do my residency and I met Charles Tanford, and as I shook his hand, my hand and his hand went down together, and he had a glass slide which we used to use in the ultracentrifuges in those days, which had a ultracentrifuge pattern on it. And

our hands hit the slide, which was at the end of the table, it went circling in the air and it smashed on the ground. I took that as a very bad omen. He of course said, "Not to worry. We can always do that again." I then decided that I really did not want to work on [that]. I was getting a little tired of the ultracentrifuge. I spent a lot of my time at the NIH with the ultracentrifuge to determine the molecular weight of actin. I wanted to do something I thought would be more exciting. I guess you could call that cell biology and things like that.

I called up a well-known scientist at the time by the name of Herb Sober in Building 3, and I said, "Where should I go if I had a year to spend on doing more research?" And he said, "You should go to Seattle and work with a guy by the name of Edmond Fischer." I didn't know Edmond Fischer, but I called him up and he said, "Sure, you want to come, that's fine." And then he completely forgot and I never heard anything from him until my Duke residency was almost over. Then I just reminded him that I was coming to be a fellow in his laboratory, and he said it was fine. So we flew out West for the first time, to Seattle, Washington.

I remember the first time I met Edmond Fischer, everybody called him "Eddie." He invited me, to join him and his wife who were in the forest surrounding Mt. Rainier. And he said he would leave his red underwear up as a sign for me where he would be. He gave me some vague directions of how to get there. I had a rental car and I had my wife and a very young child of about two years, and I went looking for Eddie Fischer in the forest. I was just about ready to give up when suddenly I saw the red underwear hanging from a

tree, and there was Eddie Fischer and his wife skinny-dipping, and he invited me to come in. Well, I refused to get below my underwear and my wife refused it altogether. That was my first meeting with Eddie Fischer.

SS: Welcome to the 1960s.

RA: Exactly.

SS: We have not mentioned your meeting your wife and her name, and all of that. You can include that story since we've brought her in now.

RA: This was while I was an intern at Bellevue. It was just after I got back from my stay during my internship at Duke. And sitting on my phone was a note from an aunt, and the aunt said, "You should call this young lady. She was a sergeant in the Israeli army." At the time she was not my favorite aunt. She subsequently became one of my favorite aunts. In other words, I really didn't think that anybody who she would suggest would be at all of interest to me. But since my aunt asked me to do it, I called her up.

She was born in Israel, indeed became a sergeant in the Israeli army and came to study piano at Julliard for a year, and that's how she was at Julliard. She had played the piano in Israel for quite a while as a student. She was a serious student and she came to spend at least a year at Julliard. I was an intern at the time. I remember she was living at 125th Street, Manhattan, which was where a lot of the Julliard students lived. I took her out, and

I was very smitten I would say with her from the first day that I met her. I spoke a little bit of Hebrew, but not very much.

I remember I took her back and introduced her to my fellow residents at Bellevue, at the time. And we began to court each other and we went out, and subsequently we got married just before I started my residency. We got married as I was finishing my stay at Bellevue for the first year of residency. We had a one-week Honeymoon and then we left for the NIH.

SS: What is her name?

RA: Her name is Miriam Appleman and now it's Miriam Appleman Adelstein. She is still playing the piano. She's a very serious pianist. She recently was taking lessons from a teacher who has just become the head of the Department of Piano at the Eastman School of Music in Rochester, New York. His name was Andrew Harley. I would say she was his favorite student—he would joke about taking her on tour with him. She's very advanced. As a matter of fact, she still practices every day and she's about to start with a new piano teacher.

She played for the NIH. When we first came to the NIH—my first year here—there was an NIH Symphony Orchestra. A lot of the famous scientists were members of the symphony. Chris Anfinsen, who went on to win the Nobel was playing the viola, and I recall that Robert Berliner, who was the head of the Heart Institute, also was playing in it. It was

arranged that she would play a Mozart piano concerto. Remember, the NIH was different in those days because anybody could come in, come out. It was no big deal. So when the orchestra gave a concert they would fill Masur Auditorium without any problem.

Neighbors could come in.

SS: What auditorium was it?

RA: Masur the main auditorium. She played a Mozart piano concerto with them. She also, for the occasion wore her wedding dress. I can tell you a little anecdote that when she was playing the piano concerto: Robert Berliner, who went on to become the head of the Institute and became dean at Yale, was playing something like the flute or the piccolo. The point is he forgot to come in and my wife had to smudge on through the part where he was supposed to come in. I used to make fun and tell people the reason I got my job is because Robert Berliner forgot to come in while he was playing with my wife. [There was] nothing real about that.

SS: In 1961, you came back to the NIH as a research associate. We were talking about Washington State.

RA: We're up to Washington with Eddie Fischer. I spent a year and a half with him in Washington State prior to coming back here.

[Break in audio]

SS: Okay, we are back after a short break.

RA: The first time when I came to work as a research associate [here at the NIH] I worked with a fellow by the name of Wayne Kielley who was a member of Chris Anfinsen's lab. I worked on a protein called actin, and that was between the years of 1961-1964. Then I went away for two years. It turned out to be two and a half years. At first I was at Duke where I did another year of residency. Then I decided not to stay at Duke for more postdoctoral fellowship, but I went to Eddie Fischer's laboratory. Eddie Fischer was one of these inspirational characters in my life like Gordon Tomkins. First of all he was a fantastic pianist, so he and my wife got along together very well. He's still alive. He's about to be a hundred years old. As far as I know he's been well. I met his granddaughter recently who was a postdoc at the NIH.

I was with Eddie Fischer and I got the idea at Eddie Fischer's lab at the time for my first major project when I returned to the NIH. Eddie Fischer had been studying with Ed Krebs the effects of adding phosphate groups to proteins, and just the addition of the phosphate groups would do very interesting things to proteins It would cause them to change their shape, and become activated. When I returned to the NIH I applied the same idea to the protein I was studying at the time called nonmuscle myosin. We discovered that simply by phosphorylating the myosin you could increase its enzymatic activity, and that this played a major regulatory role for one of the major contractile proteins (myosin). When you flex your muscle all you're doing is passing your actin filaments, passed your

myosin filaments. This has got to do with muscle contraction as well as with cell contraction and movement. It turns out every cell in the body has some of these contractile proteins in them, so they play a role throughout the body.

The idea of phosphorylating myosin and making it active was very unusual at the time and I like to think that we were one of the first investigators to do it. The fact is that for many of the important ideas in science there are other people who were doing similar kinds of things at the same time. There was a scientist named Apolinary Sobieszekin Austria who was doing this and there was a scientist named Sam Perry in England who was trying to do this with skeletal muscle. We always think we're original, but the truth of the matter is we're always accompanied by other people who are working in the same area.

SS: Do you remember what gave you that idea?

RA: About phosphorylating? It was just directly from the fact that Fischer and Krebs had found that they could alter the enzymatic activity of phosphorylase by adding a phosphate to the enzyme. My protein was myosin so when I returned to the NIH I wondered whether if it worked for Fischer's and Krebs maybe it would work for nonmuscle myosin, We got very lucky. It is also important to be lucky in research. I phosphorylated a myosin that was not from muscle, which is where most people think of it, but a myosin that came from platelets. We began to realize that all cells contain myosin in them. So I

phosphorylated myosin from platelets and I got a major effect on the myosin activity. At the same time, S.V. Perry was phosphorylating skeletal muscle myosin, and although he could do it, he wasn't getting very much of an effect at all. It turns out that there is an effect, but it's a subtle kind of effect. In that sense I was very lucky because I had used platelet myosin. In some ways that was our first major scientific contribution at the time. So I'm back at the NIH and now we're in the 1970s.

SS: When you came back from your postdoc and your residency it was 1966.

RA: So it must have been '67-'68 when I came back from Washington [State].

SS: Your early time here, 1961 to '64, you were in Anfinsen's lab and you were working with Kielley?

RA: Wayne Kielley, K-I-E-L-L-E-Y.

SS: That's an interesting time period and you were working in Building 3?

RA: No, we were working in Building 10 on the seventh floor. At the time Anfinsen was a member of the Heart Institute and was over on the seventh floor in Building 10.

Subsequently, while I was away, Kielley's lab broke off from Anfinsen. Anfinsen takes a job at Harvard that lasts one year. So he goes to Harvard, but when he comes back from Harvard one year later he doesn't go back to the Heart, Lung, and Blood Institute. He

goes into the Arthritis Institute, so he's in a different institute, but I'm still in Heart, Lung, and Blood. So I come back to resume my work with Kielley and Kielley now is part of Ed Korn's laboratory. So then I go to Building 3.

SS: In the late '60s?

RA: That's it. Then while I'm there—

SS: You came back as a senior investigator.

RA: I guess, yes. I go over one day to visit [Eugene] Braunwald and I say to Braunwald: "You people need some molecular biology or biochemistry because you're only doing physiology and I'd like to come and join your lab, and I also would like to work in the cardiac clinic so I can keep my hand in medicine." And he said, "Okay, you could join. You could work in the cardiac clinic and you can come over here, but we're not going to change the kind of work we're doing in physiology." My lab then moved into the Cardiology Branch.

So now I was independent of the other people, and I had a lab of my own. That must have been done by Berliner who was the head at the time. That must have been around 1966 to '72. Then I continued in cardiology, I became head of the section of molecular cardiology, and I was over here back in Building 10. Then subsequently I became lab chief—that was in 1981—and we got our own space in other places.

SS: You've mentioned some names that stand out in the history of NIH that are important: Berliner, Braunwald, and Anfinsen. Are there any stories you can share?

RA: One of the things about Braunwald, who went on to become chief of medicine at Harvard in the Brigham Hospital, I sat in front of his brother at Stuyvesant High School in New York. Adelstein, A, I sat in one seat, Braunwald was right behind me. When I see him I remind him of this.

SS: These are major figures in the history of the institution and you knew them.

RA: Yes. Anfinsen was very interesting. He would eat lunch together with all the fellows in his lab. We had a board hanging on the wall and they'd take it down, it had a couple of sawhorses, and lunch would always be served there with the postdocs sitting around. And there were older postdocs at that time when I just got back. We would sit around and discuss things like that.

Anfinsen was a very impressive character. People really loved him, admired him. He had a house right across the street from the NIH on Cedar Lane with a swimming pool, and he would invite people to come over and go swimming at the time. Marshall Nirenberg [also] had a swimming pool in his house, and I have been swimming in Marshall Nirenberg's pool when he lived there, too. Two Nobel Prize winners who interacted very

meaningfully with the younger people at that time. Everyone always admired Chris Anfinsen. He was a very strong human rights advocator.

Now we can transition. In 1976, I went to Russia for the first time. Maybe the meeting was in 1971 and I wrote this in 1976. There was a meeting in Russia of the Biophysical Society. A lot of people from the NIH went to what was then the Soviet Union and there was coexistence at the time. I went with my wife and we were a little bit shocked to find how backward things in Russia were. People were still using abacuses to add up bills and things like that. And while I was there I learned about these Jewish scientists, who were called “refuseniks,” wanted to leave Russia and go to Israel, and they were being persecuted.

For example, there was a world famous Russian scientist by the name of [Veniamin G.] Levich, and the Russians would put microphones in their apartments and actually monitor them. And while I was there for that meeting, I actually went to visit one of them. I visited Levich's lab and he showed me the KGB agent who was keeping tabs on his apartment, and in fact, just after my visit his telephones were completely cut off. At first I didn't believe these things and then I became a great believer, and that's how I became interested in some of these people who were trying to leave the Soviet Union but couldn't. I did this while I was at the NIH.

The best part to tie it in is I was asked to serve on a committee. [Henry] Kissinger had put together a number of scientific groupings of people who would exchange scientific ideas

with the Russians and I was asked to join a group from the Heart, Lung, and Blood Institute. As such, I got to go to Russia and meet a lot of Soviet scientists. We used to have joint seminars in which the Soviet scientists, the Russian scientists would present, and the American scientists would present, and there was great discrepancy between the quality of work at the time. We got to stay in the most interesting places and we got to travel all around.

For example, I went to Armenia because we had a meeting in Yerevan, and I went to Kiev because we had a meeting in Kiev, and we went to Georgia because this committee was always having meetings at these places and it was affording me a very interesting time of seeing all of that. I also became interested in the plight of the scientists who wanted to leave the Soviet Union and couldn't do that. So I joined an organization called the Committee of Concerned Scientists I began to work with them in that I would go to Russia, visit the scientist and, bring them things like jeans and food, which they could trade to get some more substance or better things.

I was also part of the NIH Cardiac Delegation to Russia. I would attend all the delegation meetings, but occasionally on our free time I would go out and visit the dissident scientists. This angered the head of the NIH Heart, Lung, and Blood Institute group that I was in, this was an official Cardiac NHLBI Delegation going to Russia, so he asked that I be removed from that group, which I was. Bob Levy who was the head of the Heart, Lung, and Blood Institute removed me. Subsequently, the people in the State Department thanked me for doing that. So there was a little discrepancy because Bob Levy wanted to

keep this thing going. The truth of the matter is the Russians were not going to end it because of anything I was going to do. They were getting too much out of it scientifically. Subsequently, the State Department invited me down with one of the people who had come out of Russia and that's how I got to go into the presidential Oval Office. I don't remember who was president at the time, probably Jimmy Carter.

SS: That was an interesting experience.

RA: Yes, and it was very meaningful for me and that's why I was able to get this [article] published in *Nature* ["Keeping the flame alight," September 30, 1976, Vol. 263, pp. 363-64], which is as you know a very well-known magazine, because *Nature* was very interested in the problem at the time.

SS: It's good to know about that. May I have a copy of that article? So back here [at NIH] you became the head of the section on molecular cardiology.

RA: Yes, but now instead of being in Korn's group I was now in the Cardiology Branch.

SS: So you finally got what you were wanting. Were you also practicing medicine?

RA: I was going to the Cardiology Clinic. Then for a while, as you know, most people here were turning over because they're not very permanent. Since I was permanent I was

beginning to collect quite a bit of the patients who were being seen in cardiac clinic. That subsequently has changed because now they have more people who don't turn over.

SS: That was the 1970s, and then in the '80s you became the chief of the laboratory of molecular cardiology, which you still are.

RA: Exactly.

SS: During that time period, Jack Orloff was the scientific director.

RA: Jack Orloff was the scientific director.

SS: How was your experience with Dr. Orloff?

RA: I got along with him very well. He was very funny. I'm sorry I'm precluded from telling you one famous story about Dr. Orloff. Let's just say that we had similar kinds of sense of humors. When I would go into his office to talk with him I would always come out with a smile on my face. As you know—or maybe you don't—he would say outrageous things and he'd say exactly how he felt. For example, if he didn't like Claude Lenfant interfering with him he was not above telling Claude Lenfant to stay out of Building 10 and to stay in Building 31. He was very straightforward. (I am sure that Dr. Lenfant was not intimidated.)

SS: What about his wife Martha Vaughan? Did you know her well?

RA: Yes, I knew her quite well. Our research actually did overlap a little bit at one time. There is a group at the NIH called Foundation for the Advanced Education in the Sciences (FAES), and I had become president at one time. They had a party—I forgot what the occasion was—over at the house, which we own on the corner of Cedar Lane and Old Georgetown Road, and we were there. It was one of the holiday parties and the last four people left at the party were my wife, Martha Vaughn, Jack Orloff and myself. And my wife, who is usually not very forward at all, said to Martha, "How about inviting us for supper." I don't know what got into her. Martha said, "Sure." So we went and we had supper with the Orloffs that night and so I began to know them.

I haven't told you much about my wife. After we got here, my wife was playing the piano, but then she realized she had to do something with her life, so she went to college and got a nursing degree, and she became an ICU nurse, intensive care unit nurse, and she became very good. In fact she was the best ICU nurse in the cardiac clinical care unit down at George Washington. So when they needed somebody to do a double shift, because Mr. Cheney had one of his operations, she was the one they selected to take care of him. Of course, Mr. Cheney unfortunately did not do such nice things later on. That's where she worked for quite a while as an ICU nurse. Then she moved to the NIH and worked first in the cardiac ICU, and then later in the in the x-ray department. So she spent a fair amount of time at the NIH and being a nurse.

SS: That is an interesting thing about the NIH. It seems there are a lot of couples that worked here.

RA: Yes. At the time we were here she was down in the ICU.

SS: Were your children young at this point?

RA: Yes, they were younger. By the time she was at the ICU they were getting older. We have three children. One of them is in San Diego, one of them is in Baltimore, and one of them is in Silver Spring.

SS: You knew Orloff quite well. You mentioned Lenfant. Did you have any dealings with the director of NIH?

RA: Well, I did skip over things. I did go with Ed Rall at one time for an interview on television. We were on a television show together. It was one of these local [weekly] news shows from Washington. I was head of the FAES at the time and we did appear on that television program to answer questions but I do not recall the issue. .

[Donald] Fredrickson who became head of the NIH was in the Heart Institute, so I knew him when he was the director. I think he was just before Orloff, and I had interacted with him. Initially when we got here, he used to have parties for the research scientists at this house. His wife was very imaginative and there was one party where she wanted all of us

to dress up in togas. I think a number of us resisted the idea of wearing a toga at the party. We got to know him because there were the social occasions. He originally was the head of the Heart Institute and then he became head of the whole NIH, but when I got here it was just the Heart Institute.

SS: Braunwald talked in a lecture online about how there were a lot of social interactions in a certain period of time at NIH.

RR: Yes. There were more social interactions. Fredrickson went out of his way to have social interactions. I don't recall that Orloff did particularly have it in terms of going to people's homes is what I'm talking about.

But, yes, going back to when we got here in 1961. The NIH is quite different. There are no gates. The campus is wide open. My wife and I would spend our Sunday mornings taking *The New York Times* and coming over to the NIH because it was like a park and reading the *Times* on the lawn at the NIH. There were tennis courts. The Building 10 elevators would go at a speed that would cause my stomach to drop. They were commanded by uniformed elevator operators who would greet you every morning and talk to you. There was a very nice kitchen in the basement of Building 10. There wasn't a huge cafeteria or anything like that. There was a small cafeteria. But there was in addition, when I was back here in the '60s, a place where you could get a hamburger made to order. So you could go down and have a much more intimate kind of arrangement with things.

I think people felt: “Hey, we've been dislocated,” most of us, from where we came. Very few people were born in Washington would come here and we would all come mostly from the North and there was a little bit, I would say, of banding together in a much more intimate kind of manner, which now the institution is so big it's not so easy to do anymore.

SS: Right. What about the lunch symposiums? Did you go to those? Maybe that was in the labs where people would talk about their work.

RA: We had that for a while. During lunchtime we would have a meeting to talk about the work and it was instituted again in this area. Right now other than eating sometimes similar with the people in my lab, we don't have official lunchtime symposiums, but we do have our lab meeting from twelve to one and I bring my lunch. Some people bring food, you're open to bringing food, but it's not always a usual part of it.

SS: You talked about the difficulty being in the South at Duke in the 1950s. What was it like here in the 1960s and everything happening culturally and politically in D.C., protests and things like that?

RA: It was very exciting. The first meaningful thing I did here at the NIH was to go to the March on Washington in 1963, and there were three of us. We were members of Anfinsen's group. One of us was John Potts who subsequently went on to become the chief of medicine at the Massachusetts General Hospital. There was myself and there was

a technician by the name of Jamie Godfrey. And we said, "Do you think you want to go downtown for the March on Washington?" It was so chancy at the time. People were not so sure how it was going to work out. Maybe some of the people were going to misbehave and it wasn't going to be nice. Anyhow, because there were three, we got brave. I can't remember how we went down. We must have taken a car and parked someplace, I can't remember that altogether.

And we went, and the moment we got there we were overwhelmed. This was like church picnic. This was not like a mad demonstration where there were going to be all sorts of mad indiscretion. It was just one big church picnic. I'm very proud of the fact that I have my button still left over from the March on Washington. John Potts came and Jamie Godfrey came, and we heard the "I Have a Dream" speech. In fact, we heard it very well because we were about ready to leave, so we moved towards where the speakers were speaking. People were spread out. And I remember Potts saying, "We have to hear this guy King speak. We don't want to leave before we hear King speak." So fortunately we stayed.

SS: I'll bet that was moving.

RA: That was terrific. Smartest thing we did.

SS: We haven't talked much about your work yet. Would you like to talk about that?

RA: Okay. So as I mentioned I first worked on actin. Actin was not a terribly exciting protein at the time. When I left Fischer's laboratory I thought I know what I'm going to do and that is I'm going to do to myosin what Fischer did with his phosphorylase and I came back here. At the time I had been given a technician. I had never had a technician before. Right after I came back from Seattle, I was given a technician by Wayne Kielley who stayed with me for forty to fifty years. She's still around. She comes in once a week now. Her name was Mary Anne Conti. Her married name is Salmon. The two of us did some experiments in which we just reasoned [the following]. Well, everybody knows how you regulate skeletal muscle myosin because there's a set of proteins that helps you regulate them the same for cardiac myosin, but no one knows what regulates platelet myosin, or myosin from other sources, and in addition smooth muscle myosin. So the trick was phosphorylation played a major role in regulation. We started and published one of the first papers in this, which was published in *Nature*.

Other people were working in the same area and soon published also together, at the time. That was major. There was a Gordon [Research] Conference, and I presented the work on myosin. There was a scientist there by the name of [Andrew Huxley], who was a very well-known scientist, Professor Huxley.

At the end of the Gordon Conference, Huxley was summarizing the Gordon Conference. He said, "I think we heard something very important at this conference, today, and that is about phosphorylation as a regulatory mechanism, and I suspect this is going to be true maybe for a number of proteins." I don't want to say we were the very first one. Fischer

and Krebs did it with phosphorylate, but he points it out. Obviously, that was very exciting for me at the time. Actually it helped me become head of the Muscle Gordon Conference the following time when we had it.

SS: Tell me about the Gordon Conference.

RA: The Gordon Conferences are a whole set, about thirty or forty, they've expanded of scientific conferences. There was one, which was devoted to proteins, and then as part of that became a particular conference that was devoted to muscle proteins. You'd go to one of these small boarding schools in Vermont and stay there.

I have a little anecdote I could tell you about my time in Duke. Dr. [Eugene] Stead was a "cracker". He was a Deep South kind of guy. We had a patient who came in to Duke when I was admitting, and the patient was black, and there were no black beds on the wards, so he had to remain in the outpatient department until a black bed appeared. The facilities in the outpatient department are not very comfortable. It's like when you go to a doctor and you have to get on a table, but you could imagine sleeping on that table all night it wouldn't be so much fun.

So I went to see Dr. Stead, and I'm sure I said it in a very calm manner, to back myself up I said, "I know and you know that all hospitals get federal funds and federal funds actually preclude this kind of discrimination. This guy came in last night and he had to spend the whole night in the outpatient department and he had an MI, myocardial

infarction, and that couldn't have been very good for him." And Dr. Stead said, "Yes, I agree with you, but I'm going to tell you Adelstein, you're about to go over, you spend some time at the VA hospital that is just down the road. By the time you get back from the VA hospital, this whole hospital will be integrated." And that is what happened.

And when they integrated Duke Hospital they did integrate it. They took the four-bed rooms and put blacks in with whites in the four-bed rooms, so it made quite a bit of difference. And the culture changed at one time. When you're working and you go to a black ward and white ward, you begin to think that there's something different about the people in the black ward, just from the fact that they're all kept separately. That was Stead's doing. That was to me one of the more meaningful things of my stay at Duke—was actually seeing that integration take place.

SS: You were there at a very interesting time.

RA: Right.

SS: Did you experience any discrimination for being Jewish?

RA: No. If there was, I never noticed it. My wife was shocked when she began to see "black only" and "whites only" when we first went down to see it. We had a young lady who came to work, cleaned our apartment once a week, and the last day when we were leaving I insisted on driving her home against her will. When we were leaving to go to Seattle we

were giving her a lot of things, which we couldn't take with us. And I drove her home, and then I saw what part of town she lived in. It was completely unrecognizable to me. There was no road. There was nothing. People didn't even have electricity. It was a different world and I would have never seen that had I not insisted on driving her home. So that made a big impression. The South has changed quite a bit from what it was then (but maybe not enough).

SS: Thank you for sharing that anecdote. I think that's what you were referring to earlier when you said, "I have something I will tell you." So you did this important work with myosin phosphorylation and became the head of this Gordon Conference the next year. Is that what the focus of your research has been?

RA: Yes. We became very interested in non-muscle myosin. But I'm unhappy because we had never done anything with DNA and RNA and genetics, and it was all passing me by. So I get offered a position at Washington University in St. Louis.

SS: How did that come about? How were you offered that job?

RA: I was getting a number of offers at the time. I was invited to look at becoming the chief of medicine at one of the Washington University in St. Louis Hospitals. In fact, I think it was the Jewish Hospital. I came back and told Orloff that I was offered this job. He said, "Well, what do you want to stay?" I said, "I want to learn how to work with DNA and RNA because all I've done is work with proteins. I therefore would like to take a

sabbatical, and I would like to take the sabbatical in Israel to work in the laboratory of a guy by the name of Howard Cedar.”

I want to parenthetically tell you right now that one of the hotter names in television on Netflix is a series, which comes out of Israel, some of them where Cedar's son has now become a world famous director. Some of his movies have actually been up for Academy Awards. His name is Joseph Cedar, C-E-D-A-R.

I said, “I'd like to go work and take a sabbatical, learn something about DNA and RNA.” My wife is, obviously, from Israel and her mother had breast cancer. And our oldest son, not with us being happy about it, had enlisted in the Israeli army and was being sent to Lebanon when the Israelis had made an incursion into Lebanon. So that was another good reason to take a sabbatical in Israel. So we went over and took a sabbatical in Jerusalem and it was very successful. I worked in Howard Cedar's lab.

And when we came back we started working with DNA and RNA, and that ultimately led to making a whole bunch of mice which were genetically modified, and which we are now distributing throughout the scientific community. Our plasmas have been sent to over eighty-five laboratories. We had gotten now into working with DNA and RNA. That's what I got out of that sabbatical.

SS: That's something that you felt like you were missing. Does that have anything to do with Nirenberg? Didn't he work on RNA and genetic code?

RA: Yes. In the sense he's the guy who cracked the genetic code. That opened up the field so that I began to feel all we were doing was working proteins and we don't know how to work with DNA and RNA. By that time it was quite a bit later.

SS: Tell me about your work with RNA and DNA.

RA: The fruit of that work is we made a whole bunch of mice that had mutations in them and we've used this to study some cardiac diseases. We took our favorite protein, we mutated it, we introduced the mutation into the mouse and the mouse developed cardiac symptoms.

We made a single mutation in the mouse in non-muscle myosin IIB and the mouse was born with the heart outside of the body, among other things, and with valvular abnormalities in the heart. While I was looking for the first time at this finding in the mouse with the mutations in the heart outside the body and other problems with the heart, there was a very well known cardiac scientist, Deepak Sristava, who was looking over my shoulder. And he said, "I know what that is. That's Pentalogy of Cantrell. Pentalogy of Cantrell has five different problems with the heart and the diaphragm, and is a congenital disease found also in humans. We made it in the mouse and we have it in humans. We then decided wouldn't be fun to see if we could find what protein(s) was mutated in humans to give us Pentalogy of Cantrell.

This led to my finding—or their finding me—a young lady in Florida who had her heart outside the body. But because she wasn't operated on immediately—what you have to do with these things is you've got to operate on them in the second or third day of life and put the heart back inside the body and correct a number of things, and so forth. But she wasn't, so she was walking around with her heart outside the body. She was born in Russia, but now she was in Florida. To make a long story short, BBC had gotten a hold of her and they did a program about the girl with her heart outside the body and they included me in that, and it's now been archived someplace in BBC. I don't know if I can reach it anymore, but that was fun and interesting. I still remain in contact with the young lady and she's still doing quite well, at least the last time I called.

SS: Was surgery done?

RA: No. If you see her, you can see her heart beating. But she's got a membrane over it, so you don't see the actual heart. It is covered by a membrane, which comes from her body but she's got to be careful.

SS: Did that help you in your studies?

RA: Yes. It helped us expand the study and initiated our search for what we thought could be the causative gene. What I can tell you is that in humans it's a lot harder than in mice and that there a number of genes that are associated. When we started analyzing humans who had this we found that there was more than one gene involved.

SS: Is this prevented today through genetic [manipulation]?

RA: No one yet goes into correct the gene before people are born. That's not possible. Today, still the best cure for it is to operate on the child very soon after birth.

SS: I believe you mentioned Pentalogy of Cantrell in the chapter you wrote about your muscle research [*NIH: An Account of Research in its Laboratories and Clinics*, DeWitt Stetten ed., 2014.] One of the quotes I liked in that article was that you learned that you could do a lot of excellent science and still come out wrong.

RA: Right. I don't remember that quote, but it's certainly true.

SS: That was an important part of your career, looking at that syndrome.

RA: Yes.

SS: What else would you like to include?

RA: First of all, I've been very lucky, especially now that I'm older and I'm not very much engaged. I have two excellent what are called "staff scientists." One of them is Xuefei Ma, who has been with me for a number of years, and the other one is Sachiyo Kawamoto, and they're presently helping me run the laboratory. They've been with me

for quite a while. They, along with Mary Anne Conti, who comes in once a week and has really been a major contributor to all my work. And I've been lucky to have some very good postdoctoral fellows, foremost of which is [James Robert] "Jim" Sellers. This picture was taken a year ago. There's Jim Sellers. He's now the head of his own laboratory. His reputation has been established very well in the field. And it gives me some pleasure to say that I think he's outdone me as one of my students. That's Mary Anne Conti and she's the one who comes in still once a week. This is Xuefei Ma who is one of the two people who still work with me, and that's Sachiyo Kawamoto who is another person who still works with me. This is Denis Buxton who is now over in the Heart, Lung, and Blood in the extra-mural program and this is Qize Wei who is now a Professor at Fordham University.

SS: We were talking about your work and other aspects of it that you wanted to mention. This brings up the whole question of mentees. You mentioned some of your major mentors.

RA: Tomkins, certainly Fischer very strongly, and Kielley, although Kielley did not rise to fame. Fischer went on to win the Nobel Prize. Anfinsen went on to win one. In fact, I used to joke and say people should hire me to work in their lab. After all they always end up winning a Nobel Prize when I leave. I've been very lucky in having a whole bunch of very good fellows who have been very instrumental.

SS: Who have you mentored?

RA: I've mentored the guy who is now the chief of cardiology at the University of Hawaii, Ralph Shohet. I've mentored a guy who is now a very well known research scientist at Yale (Michael Simons). Jim Sellers in some ways would have been another example, and he's done a lot of his own mentoring himself. I've mentored a number of people who have gone on to have very good careers. There are other people. This is Qize Wei who is now a professor at Fordham University. I've contributed to people who have gone on to make major contributions on their own.

SS: What advice might you give a young research scientist or doctor starting out today in your field?

RA: To develop your own interests, to persevere in what you're doing even when things don't seem to be working out as long as you believe that your ideas are right, but be able to be open to criticisms of your ideas that may help you at least make a synthesis of something that could be meaningful in science. I think to do this especially at this time when the whole question of "what is truth" is coming into play to remember that scientists make major important contribution to the idea that there is some reality and some truth which can't be made up, but which has to be investigated and understood.

SS: Thank you, very nice. What still excites you in the lab or in your field?

RA: Right now my colleagues excite me. Right now also we're going into areas where we didn't ever go before. For example, I have a postdoctoral fellow who is working on

mechanotransduction, which is trying to understand how cell shape changes or migrates. He's publishing some new and unusual things. I'm still very interested in the fact, for example, Sachiyo Kawamoto has her own program looking in an area which was related somewhat to what we were doing, but is different in that it studies a very basic process called alternative splicing, which means how do changes in genes alter proteins, which is I think very interesting. And Xuefei Ma is working on looking at vascular development. Developmental biology I find very fascinating. I actually used to teach a course in developmental biology. A number of years ago I became interested, late. I liked teaching. I am very lucky in having my office next to Yoh-suke Mukoyama, a young tenured investigator.

SS: At Georgetown?

RA: Originally Georgetown, but no, right here at the School of Foundation for the Advanced Education of Sciences. Alan Schechter and I, go back in the 1960s, started teaching a course in protein chemistry. At the time Connie Taniguchi was in his lab and we started to teach a course in developmental biology. And then a true developmental biologist moved in next door to me, Yoh-suke Mukoyama, and they still teach that course in developmental biology in the night school. They have around fourteen to fifteen students every year, and I think it's quite successful, and they let me give the first lecture. I think teaching is very important. People should know that you can come to the NIH, but also you can leave the NIH and have a career that also can be very successful. The NIH does have a component where you can have teaching. It's not just an institute as such.

SS: I don't know how you did everything. You taught and you were head of a laboratory and you did your own research, and you were a clinician.

RA: Yes, I was a clinician only in the clinic at the time. I must say I really loved seeing patients because you're beginning to hear a story of somebody else's life, and I've always found that very fascinating. I've met all sorts of people, most of them extremely nice. On the other hand I remember meeting in the outpatient clinic a policeman when I was at Duke, and he had cancer, severe cancer, and I was concerned. He took my hands and squeezed them. And I said, "That's great the way you can squeeze my hands like that." And he said, "Yes, I toughen them up by beating up the Blacks." This was a number of years ago, but this country hasn't gotten away from it. That's what I think the whole present administration to a certain extent is about, that we haven't come to understanding of this problem like a lot of the rest of the world has. We still have some learning to do. I can't remember, a very well known black author, beloved really.

SS: Toni Morrison?

RA: Yes. She spent time at Princeton and they had a nice article about her recently.

SS: In addition to the things that I said you were doing, I forgot to add you were on the editorial board of the *Journal of Biological Chemistry*.

RA: I was on the editorial board and actually it was over twenty years when you put it altogether, I went on and went off

SS: You were included in many societies and received honors and awards, which we haven't mentioned in this interview.

RA: Yes. I'm not that outstanding. I've had a very interesting, exciting, somewhat productive career at the NIH, but I'm one of many. There is something about the NIH, which I think does allow people to do very exciting things so that I could go abroad to Russia or to Israel, or to various parts of the world where I would never have been had I not been a scientist. I used to collaborate, for example, with scientists in Armenia and things like that, in the past.

SS: So working at NIH has afforded you certain opportunities.

RA: Absolutely.

SS: Do you plan to retire?

RA: Yes, I am going to retire. You can see how old I am. I'm eighty-five, and I'm going to retire at the end of next year. I will be honest in that my mind is not as nimble as it was a few years ago and I can feel that this would be a good time for me to retire. I run the laboratory with the great help of Sachiyo Kawamoto and Xuefei Ma at the present

moment. I think I'm still able to give young people good advice about how to direct their lives and maybe what medical schools or graduate schools to think about going to, and things like that, and how you want to be careful in your life. As Shakespeare said, "Reputation is of importance," even though it was Iago who said it, but it is important.

I've had some not pleasant experiences where a young lady has had to leave the laboratory because she did something independent of the lab, but very foolish, and that's hurt me. I've been around to see all sorts of things and unfortunately she unintentionally or intentionally hit somebody else and she had her career at the NIH terminated along with her husband. So I've seen a lot of bad things as well as lots of good things.

SS: I think your mind seems pretty nimble to me.

RA: Well, maybe.

SS: What would you say is your legacy?

RA: I think the legacy is the same thing that I think I received from my mentors and that is to be as honest as you can in your research at all times, and to not be afraid of changing your mind if the scientific facts begin to show that you've made a mistake.

For example, I was once convinced that another laboratory was wrong and we were right about a certain protein and the importance of that protein. I was trying to deride the

function of that protein. But I went to work in another lab at the NIH and found that in fact I was wrong and that they were right. Just like I've had people come to my lab who doubted the importance of phosphorylation, for example, in regulating myosin and have done experiments, and left the lab convinced of the fact. There was a big fight over something called cofactor rather than phosphorylation as being critical, people coming to the laboratory and doing experiments.

There is nothing in science that beats going to the laboratory, which you may have a disagreement with and carrying out experiments to be sure that you understand what's going on. That's the thing we have to remember most about science. It's not going to be the noise that's going to prove the point. It will be the experiment that proves the point. Lots of times there is room to say, "Well, I didn't realize that these experiments are so subtle and more difficult, and maybe I'm wrong. Maybe I just didn't appreciate that."

One of the things I'm proudest of is I've also played a role in having symposiums in honor of people who have had a big influence on my life. There was a scientist at the National Cancer Institute by the name of Claude Klee who made some major contributions and who died suddenly. I organized a symposium in her memory because she was a very decent and hard-nosed scientist who would let only the facts speak for themselves. I learned a lot from her.

I think to all young scientists at the NIH I would say, "Get out of your laboratory get into other labs and see how things are done, and you can learn a lot from the way other people

do it. You don't always have to take a sabbatical someplace. You can just go around and visit other labs at the NIH to see how things are done.” One of the wonderful things about the NIH is that you can find somebody who is an expert in practically any of the biological sciences or realms where that can be very useful and meaningful, so you can learn something.

SS: Did you do that?

RA: We certainly tried to do that, yes.

SS: Will you be remembered in the field of myosin research?

RA: I don't know. Whether I'm remembered or not I don't think is of any importance. I honestly don't think it has any importance. I'm not looking for a legacy of: “Oh, this is the Adelstein idea.” No. Science is not meant to work that way. I know that twenty years from now, maybe fifteen years from now, people who looked up my papers they'll say, “Isn't that sweet that we thought that way.” I think we're moving on and that's the whole thing about science. Yes, you can give credit to people who have done things, that's all very nice, but the most important thing is to move on and advance it, and to make sure that we're being true to ourselves, and I don't think personalities should play that important a role.

SS: Where is the field moving?

RA: I think the field is moving faster than one realizes when we begin to understand how the genes exactly work. Yesterday I heard a lecture about what's going on inside some of the parts of the gene where we still don't have a complete understanding of what's going on. In a sense you can look at the whole concept of understanding the genes as just being in its infancy. Forty, fifty years from now, particularly at the rate things are advancing, particularly with respect to math and the hard sciences we're going to look back at this as probably a little bit of: "Oh, wasn't that nice that they thought that way back then." Look how we think now that advances are being made. If only we could make the same kinds of advances in science in our social situations it would be much better.

SS: Yes. Is there anything else?

RA: Not that I can think of. You've been very generous with your time and if you can think of anything more let me know. I've got to be honest in saying that getting into your eighties does slow your thought processes a little bit and one has got to be aware of that. That's why in a sense I'm looking forward to next year and beginning to just spend my time worrying about how my grandchildren are doing.

SS: Has your wife retired as well or will she retire?

RA: My wife is retired, but that means she still plays the piano four or five hours a day, and she's still taking lessons.

SS: It's been quite an honor to spend some time with you and I appreciate your humility.

RA: You've been very patient. I certainly appreciate the time you spent and I enjoyed talking with you.

SS: Thank you very much.

RA: If I can be of any further help you'll let me know.

[End of interview]