LEONORE A. HERZENBERG

An Oral History
conducted by Joyce Kiefer

STANFORD HISTORICAL SOCIETY ORAL HISTORY PROGRAM

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Leonore A. Herzenberg
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Introduction

This oral history was conducted by the Stanford Historical Society Oral History Program, in collaboration with the Stanford University Archives. The program is under the direction of the Oral History Committee of the Stanford Historical Society.

The Stanford Historical Society Oral History Program furthers the Society’s mission “to foster and support the documentation, study, publication, dissemination, and preservation of the history of the Leland Stanford Junior University.” The program explores the institutional history of the University, with an emphasis on the transformative post-WWII period, through interviews with leading faculty, staff, alumni, trustees, and others. The interview recordings and transcripts provide valuable additions to the existing collection of written and photographic materials in the Stanford University Archives.

Oral history is not a final, verified, or complete narrative of events. It is a unique, reflective, spoken account, offered by the interviewee in response to questioning, and as such it may be deeply personal. Each oral history is a reflection of the past as the interviewee remembers and recounts it. But memory and meaning vary from person to person; others may recall events differently. Used as primary source material, any one oral history will be compared with and evaluated in light of other evidence, such as contemporary texts and other oral histories, in arriving at an interpretation of the past. Although the interviewees have a past or current connection with Stanford University, they are not speaking as representatives of the University.

Each transcript is edited by program staff and by the interviewee for grammar, syntax, and occasional inaccuracies and to aid in overall clarity and readability, while maintaining the substantive content of the interview as well as the interviewee’s voice. As a result of this editing process, the transcript does not match the recording verbatim. In the rare case that a substantive deletion has been made, this is indicated at the relevant place on the transcript. Any substantive additions are noted in brackets or by footnote.
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This oral history should be cited as “Leonore A. Herzenberg, Stanford Historical Society Oral History Program Interviews (SC0932). Department of Special Collections & University Archives, Stanford University Libraries, Stanford, Calif.”
Abstract

Leonore (Lee) Herzenberg reviewed her life story with emphasis on her upbringing in New York; her studies with her husband Leonard at Cal Tech; the scientists they worked with in France, at Stanford and elsewhere; getting the Genetics labs started when Stanford’s medical school moved to the Palo Alto Campus; and her experience as a woman in science. Lee also describes her involvement in the social protests of the 1960’s and ’70s. She discusses her approach to scientific problems and what she learned from her mentor at Cal Tech, Nobel Prize winner Barbara McClintock.
Leonore (Lee) A. Herzenberg
Biography

Leonore (Lee) A. Herzenberg is currently Professor of Genetics and Director of the Herzenberg Lab at Stanford.

Lee came to Stanford in 1959 as a research assistant in her husband Leonard (Len) Herzenberg’s laboratory. This was just after the Medical School moved from San Francisco to Stanford and Len had been recruited as an Assistant Professor in the new genetics department. Lee was later promoted to research associate, then senior research associate, then to Professor (Research) in Genetics in 1989. Len and Lee have always jointly run their laboratory.

Lee has received many professional honors and awards. In 1981 she was awarded a doctorate of science from the Sorbonne in Immunology. She is an author on more than 450 scientific articles and an inventor of about 50 inventions (which have generated $1.5 million in royalties). The Herzenberg Lab has generated a total of about $500 million in royalty income, far more than any other lab at Stanford. Some has been used to fund researchers in their lab. After Len Herzenberg died in October, 2014, Lee Herzenberg continued to direct the Herzenberg Lab. Lee is widely known for her studies of the murine immune system. She was very involved in the creation and development of the Fluorescence Activated Cell Sorter and the establishment of monoclonal antibodies that are widely used as reagents in life science research and are models for the therapeutic antibodies used in several diseases.
Kiefer: Today is May 15, 2014. On behalf of the Stanford Historical Society, I, Joyce Kiefer, am going to interview Leonore Herzenberg. We’re going to talk about her career and her life.

Lee, the first thing I want to ask you is when and where were you born and where were you raised.

Herzenberg: [00:00:25] I was the next-to-the-last baby born in Harlem Hospital on February 15, 1935. My parents at the time lived across the river in the Bronx in what was a working-class neighborhood, nice neighborhood but working-class. They lived there about four months. When I was four months old my mother decided that she’d had enough of that place and enough of my grandmother, my father’s mother, and she moved out to Brighton Beach along with her mother and her sisters. She took an apartment in a very nice building right near the beach. At that time Brighton Beach was, again, a working-class neighborhood, very, very intellectual, lots of discussion and talks all the time, and she loved that.

Kiefer: Did you have brothers and sisters?

Herzenberg: [00:01:31] I had one brother seven years older.
Kiefer: You mentioned that there was a lot of intellectual conversation in the neighborhood. What would you say was the general feeling about women? Did people at that time in your community have the feeling that women had a certain place or were things more open?

Herzenberg: [00:01:55] That’s a good question, actually. Brighton Beach was a much more liberated atmosphere. It was one of the reasons, I think, my grandmother decided to move there. I think she wanted to be nearer the ocean, and it was a new neighborhood at that time. These were new apartment buildings and she liked to live in them. She lived probably about six blocks away from my mother. It was all very comfortable.

In terms of women, my mother had three sisters, all of them were very bright. The attitude in the family was that women worked, but that was actually a very Jewish attitude, which people don’t really appreciate these days. If you go into the history of it, in the shtetl, the women were often the ones who were the breadwinners. If you were able to you ran a stall, you sold things, you did things so that your husband could go and study, and that was the ultimate goal. Most families didn’t achieve that. Both husband and wife needed to do as much as they could in order to survive. Women were expected to read and write, but they read Yiddish, not Hebrew. They were not allowed to read Hebrew, and they were not meant to be educated, but they were certainly meant to be commercial.

One of my aunts ultimately ran a very large store, a television store. They were the first to introduce television into—I guess it was Queens at
the time. So if you go back to the attitude in the neighborhood, I grew up in that kind of environment.

The other aunt was married to a schoolteacher and, in principle, stayed home. She was more or less a kind of costume designer. She went back to work as soon as her youngest was about eight years old or so. She sold women’s undergarments at Best & Co. on Fifth Avenue, which was really a hotshot place to get to work at.

So all three sisters were very bright and very, very different. My mother was closest to the aunt who ran the store, and I grew up in her store. I guess one out of every two weekends, at least, we would go out and help Aunt Ruth at the store because that was what was needed. I was closest to those cousins. My father was closest to the uncle.

This is a very interesting—“intellectual” is not really the right word---but my father and uncle were very much into engineering. The uncle was one of the first television engineers, and in 1939 he had the television booth at the World’s Fair. So we would go to visit Uncle George at the World’s Fair and see the television. You could see yourself on television. It was amazing. It was a little postage-stamp screen, but you stood in front of the camera and you could see yourself, and that’s what the booth was about.

George was doing this when the war broke out. My father was completely knowledgeable about what he was doing because they would sit and talk about it for hours. My father was an electrician but didn’t do house electrical work. He did heavy factory installations and specialized in moving a factory and being able to set up the machinery so a minimum amount of
time would be lost. In that sense he was an efficiency engineer. So he’s a funny guy, but the two of them together were always together and always talking about that kind of stuff.

My cousins weren’t that interested, but I was. I used to sit and listen to them. I didn’t know much about what they were doing, but I certainly did love to hear them.

Kiefer: I think you said you had helped your father wire a house that he built?

Herzenberg: Well, he rebuilt our house. World War II hit at that time and everything changed. My father tried to volunteer. He was very, very patriotic. He really believed strongly in the United States. He believed in a United States in which working people and black people and everybody had a vote. He believed in the principle of democracy that Roosevelt was establishing at the time. And Mayor LaGuardia. My father had worked for Fiorello LaGuardia’s campaign [for mayor of New York City] before I was born and really just believed strongly that people who have a vote should be able to vote. The whole American gestalt about what a citizen should do he believed and really wanted to do and felt everybody should do. He was very strongly behind the war effort.

He and his brothers had some of the first cars that people could buy, and they tinkered with the cars. He knew about electricity and was one of the first master electricians in New York City. I think he had license number twenty. He conducted his business out of our house, so, again, I got to listen to all the things that went on with the business, his customers calling, what
he was going to do for the customers, talking to the inspectors. I just grew up in the middle of that.

My mother ran his business for him. She did all the bookkeeping and took telephone calls and did whatever needed to be done. That was also done from our house, so I was in the middle of that. I was in the middle of civic-minded people. He was extremely civic-minded, and he believed everybody should be.

So going back, when the war broke out my father was just starting a business in fluorescent lighting. He understood that fluorescent lighting was the direction to go in the city for all sorts of industrial installations, and so he started something called the A-Light Company (A for Adlerstein, my maiden name). He got himself a small shop downtown, built fluorescent fixtures and was starting to install them when the war broke out. Now, had he been like most people he would have seen that his business was a business to make fortunes on.

My father probably saw that, but he was not interested in fortunes. He was interested in doing his civic duty. So he simply shut the business down and tried to get into the army. Well, of course, he was too old and he had two children, so they didn’t take him. Then he said, “Well, I’ll volunteer and I’ll do electrical work for the army.”

He wound up doing electrical work for the Brooklyn Navy Yard. Everyone who volunteered to work there was asked, “What do you want?” He checked “electrical,” so they gave him a test, wiring a particular circuit. They gave them the wires and had a little circuit on the board. He took one
look at it, and bang, bang, bang, he’s done and hands it in. They said, “Wait, wait, wait. You couldn’t have done that.”

He said, “It’s OK.”

The guy says, “Well, this is wrong. It’s not the way it says.”

“Well,” my father says, “It works. Try it.” [laughter]

He said, “Yes.”

“Well, this is a faster way to wire that same circuit.”

“Fine. You can be an electrician for the navy!”

What being an electrician was then was going into the hulls of ships and wiring circuits. The circuits were fixed and extremely simple because the other guys on the crews were barbers and similar things. One guy was a fruit store vendor. These were people who just knew enough about wires to be willing to tie them onto posts.

This was just driving my father crazy. The foreman knew much less than he knew. The ship wasn’t being wired correctly according to the way he would do it, and this really annoyed him. He said, “Here I am, I’m trying to volunteer for the war effort, and they put me doing something which is really useless. The barber next door can do this. I can train this whole crew to do it correctly in no time at all.”

He tried to complain, it didn’t do any good. Finally my father noticed that the electrician crew kept breaking electrical bulbs that they used to light their work on the ships. These lights were called “trouble lamps” at the time. They’re just an extension cord with a bulb at the end with a little wire cage around the bulb so if you hit it, in principle, it wouldn’t break. But the
crew kept breaking the light bulbs anyway. This bothered my father no end. I mean, I’m seven years old at the time so I’m in the middle of all this discussion, and I’m listening. I remember this as well as if it were today. He was so annoyed with this, and he was telling my uncle about this and saying, “This just can’t be. We can’t do this.”

So finally he figured it out. He told the guys on the crew he could do his work and he had plenty of free time left over. He said, “You just come and tell me whenever you want a trouble lamp moved. Don’t move it. I’ll bring it over for you.” And he would put it up on the pipe and give it a hitch so that it couldn’t be knocked over and it couldn’t be broken.

Well, about a month or two after he started doing this he got called in from somebody who was higher up the line. He went into the office building and he stood in front of the desk, and he thought they were going to commend him for saving light bulbs. And they said, “Are you the guy that’s been fixing the light bulbs?”

He said, “Yes, I am,” and he started explaining why he’s doing it.

He says, “Well, the problem is that we’re piling up light bulbs in the warehouse because your crew isn’t breaking them fast enough. You stop doing that.”

That was enough for him. He said he’s getting off this ship. He said, “I’m not going to go and cost the government money. Let somebody else do that.”

So he studied out the Navy Yard buildings. There were two of them. I’m telling you this long story because it really was the kind of thing that
conditioned me to not take no for an answer. He figured out that if he went up to the fifth floor there was a crossover into the high-level administrative building, and you could go up there. You’d walk up the stairway, you’d get up there, you’d get across this thing, then there was a stairway that he found to walk up, and he turned up in the Admiralty Office and said, “I want out.”

And they said, “What do you want to do?”

He said, “I can teach electricity. I can teach all these guys all this.” Ultimately, he spent the rest of the war teaching electricity to flight pilots.

Now, had he been, again, somebody who was interested in anything other than doing something creative he would have just hung with that school. After the pilot school closed down it became a big trade school. Nope, he went back to his business. By this time other companies had gotten in with their war contracts, so he never was able to make a business out of the fluorescent lighting, but he went back and continued to do contracting.

As I got older I would complain about school, and he and my mother had to figure out what to do about me. They offered to put me into a school for gifted children, but we lived in Brighton Beach, and this was in Avenue D in downtown Brooklyn. To get there I would have had an hour a day on the trolley to get there and another hour to come back. My mother, of course, would have had to go with me and pick me up every single day that way. None of that made any sense. It wasn’t clear that going to a gifted school was really what I should be doing anyway.

So in the end they kept me in the school that was across the street from my house. I would cry, “I don’t want to go to school. I’m bored. The
teachers they don’t like me. The kids punch me.” All sorts of things would happen because I always knew the answers. They could have skipped me, but they didn’t want to skip me. I don’t know most of that. It was done out of my hearing.

But the net result was that by the time I reached ten or eleven my parents decided they could teach me a lot by homeschooling, but not to keep me out of school, which they would never do. They believed I should be in school. But one day out of every two weeks or two days out of every two weeks, I was allowed to stay home. I didn’t stay at home, but I went to work with my father. At that time he was driving around the city. He had a couple of teams working, and he would bring whatever supplies they needed—pipes, plugs, whatever—and then he would give them instruction. And then he’d go and collect money from the customers and go to check out some new jobs.

This meant driving all over New York City. During that time we played learning geography by finding every license plate possible. We found all the states. At that time it was forty-eight, and I had to remember and be able to call them off as to what they were.

He loved math problems, and that started before we moved into the house that we rebuilt. It would have been 1941 so I was maybe six years old or so. He would just do math problems with me every morning, so we’d just sit. He might ask, “If a bathtub could hold ten gallons of water, and the tap came in at two gallons a minute, how long would it take to fill at two gallons a minute?” How many minutes to fill the bathtub? He would just invent all
kinds of problems like that. My mother wasn’t too keen on this. [laughs] She said, “What does a little girl need to do with that much arithmetic?” But on the other hand she was a bookkeeper, and she was bright in arithmetic herself, and she saw it as arithmetic.

My father actually had no schooling in math, but he ultimately wound up deriving the calculus to the extent that he encountered various problems. He just figured out and understood limit theory. I mean, I only realized this when I was in high school and I learned it formally. But that’s what he had done, so he had that kind of mind. It was always with me, doing stuff like that.

My mother wouldn’t let my father teach me to speak and understand Hebrew. He bought the books. He was going to teach me. But my mother said “no”. Jewish boys were all taught to read Hebrew phonetically and to minimally understand the language for their Bar Mitzvah. But my mother wasn’t for me understanding and speaking Hebrew. My father had already taught me to read Hebrew phonetically. In fact I could successfully read phonetically from the time I was about two and a half, I think. My father used to like to show my reading off to an elderly rabbi who used to sit on the boardwalk in Brighton Beach, which was near Coney Island. As close as I can figure that Rabbi died around my third birthday.

**Kiefer:** This is interesting.

**Herzenberg:** [00:18:21] In a sense this turned me into somebody who felt I was expected to perform intellectually. My parents realized I was pretty bright, and my
father in particular, but my mother as well catered to that brightness. They wanted me to learn all kinds of stuff.

The old rabbi I mentioned used to sit wrapped up on the boardwalk in Brighton Beach winter and summer. My father was the kind of person who really cared about people and would talk with all kinds of people, young and old, at least that was the way my parents were. Respect and the need to care for the elderly was built into our culture. My parents had met while entertaining at old people’s homes, where my father would translate songs from English to Yiddish. “South of the Border.” It’s hilarious in Yiddish. So he would translate all these songs, and he, my mother and some other young people would perform them in Yiddish at the old people’s home.

So after moving to Brighton Beach right after I was born my father found this older man, the rabbi I mentioned, who was sitting alone on the boardwalk. Typical for my father he thought this was a good person for him to take care of, so he stopped, sat down and talked to him. Ultimately, he started bringing me with him to talk to the rabbi and read from what’s called the Siddur, the Hebrew prayer book. I would read it. I know I was between the age of two and three because we moved away from there by then and we heard the rabbi died. I was able to phonetically read the morning prayers, and this old rabbi had me come and read this to him every day. He loved having me do this. It was very reinforcing for me.

I’m bouncing back and forth between the times I was two to seven years old when a lot of this formation was laid. Basically it was my job in the
family to acquire as much learning as I could, anything that was learning was good. I was rewarded a huge amount for this.

My mother was a little worried about this way of bringing me up. I would have to find a husband. She grew up in a traditional situation, and that was what girls were supposed to do. But, again, by the time I was nine my mother’s closest sister, Ruthie, was running a big business. She’d always been in business with her husband, who was my father’s closest friend. Ruthie had always run the store and the business, and my mother thought that was great.

The stories they told always celebrated the lack of traditional femininity that Ruthie’s role implied. She was a very good-looking woman, and she liked to dress well. But the only car she had to drive for a certain period of time while she and George were trying to gather together as much money as they could to buy a business, was a small panel truck. One day we were all going to go visit someone, and she was driving the truck with me and my cousins in the back and my mother in the seat on the side. Ruthie was all dressed up in a Persian lamb fur coat with a hat and big muff—she looked beautiful—and high heels. Some guy got mad at something she did in the truck. It was a driving thing. And the guy got out of his car all ready to fight with this truck driver, and she got out of the truck and, and he said, “It’s OK, lady. It’s OK, lady. Don’t worry about it.” [laughs] It was that kind of stuff.

Family stories were all full of that. Ruthie was very capable of dealing with whatever she had to. Her husband was a very interesting guy who, as I
said, was my father’s best friend. He was an Arab, born in Beirut. We were probably the only Jewish family in New York at that time that had an Arab in it. By the time I knew him George was culturally as Jewish as the rest of us, but he always pointed out that he was Arab and a Christian Arab at that. So we all celebrated Christmas with George. It was his, George’s, holiday to say what we did and how we celebrated. Not religious. We just went to his and Ruthie’s house for dinner. George loved to cook, so on Thanksgiving and Christmas he would cook elegant dinners. He’d make a beautiful turkey. But for all of that George was very much a part of a Jewish family and had no religious or cultural connection outside it. His mother had kept much more of her Syrian ways and would cook us great Syrian food whenever Ruthie and George took us to visit her. But while she was different George was not. Growing up this way was for me a multicultural experience even though none of us knew it by that name.

Kiefer: So would you say that within the family and also within, say, the neighborhood, women were pretty well respected and listened to?

Herzenberg: Well, within the family, definitely. [laughs] My father used to say, “When Reggie (my grandmother) says, ‘I want to talk to you’ she doesn’t want to talk to you, she wants to hit you with the fry pan,” So women definitely had a very strong voice in the family.

Outside, the neighborhood was such that I think it did accord women a voice, but for all of that, women knew their place. My mother was worried about what would become of me. The neighborhood was worried. I had a friend who was probably an intersex girl. I befriended her because
there was something odd about her and because she was very bright and fun to play with because she would do anything.

There were a bunch of girls in the classroom who were what we called “girlie girls,” and they had no use for us. They made it very clear that we were freaks, all of us, not just the one girl, but those of us who were getting As on our report cards and would speak up and say the right answers in class. There were four or five girls in our group, maybe 10-12 in the girlie. My closest friend was a rabbi’s daughter, Annie. We all went from first grade to sixth grade together. There were very few new people in the neighborhood.

The entire school was Jewish. There were two kids who went to school on the Jewish holiday, only two out of the entire school. The people in the neighborhood were mostly garment-industry workers, small business owners, that kind of stuff. It was a very contained neighborhood between Coney Island and Manhattan Beach. Manhattan Beach, only a few blocks away, had bigger houses. People there owned their own businesses and “had money.” By Silicon Valley standards right now the people in Manhattan Beach really didn’t have money. [laughs] But they were able to buy what clothes they wanted. My family couldn’t.

I made all my clothes, actually remade. I could sew everything. Aunt Ruth’s clothes, which were great, never went to waste. When she decided she didn’t want something anymore I got it and cut it down and made it into whatever I wanted. My mother was very good at sewing. My other aunt, Tillie, was very good also. Ruthie never sewed. Maybe she sewed a button
on or fixed a small tear. I’m sure it wasn’t anything more than that. She might have known how to sew, but she didn’t have time to do that and run the business.

So as I grew up, it was a mixture of women knowing their place but also not. It was different in many ways. The women didn’t have to entertain for their husbands. They had their place at home, but to the extent that they could manage to make some money, that was good. People did whatever they could along those lines. Mostly the women didn’t go to work. They stayed home with the kids. This was right after World War II when there was a tremendous pressure to stay home with your kids because the government wanted to open up the jobs for the men who were coming back from the army. So there was a very conscious pressure to stay home. No latchkey kids!

**Kiefer:** When you were in high school, say, towards the end of high school, did you have an idea of what you wanted to study, if you wanted to go to college, or what would be your place in the world next?

**Herzenberg:** [00:27:10] That’s a good question. I’m going to back you up on that a little bit. I will come back to that.

My father ran a Boy Scout troop, and, again, as part of his civic duty, he needed to do this. My brother was in the troop for a while and then became an Explorer Scout, and so my father then ran the Explorer post. My brother was wild. He was very wild. Cut school, et cetera. However, he ultimately got a PhD at Princeton and became the head of computing and psychology at Old Westbury College in New York, which is one of the
SUNY colleges when they first started the SUNY thing. He was a very interesting character. My mother would rant at my father, “Teach him a trade because otherwise he’s going to be a juvenile delinquent.” So my father taught him electricity.

By the time my brother was eighteen he was out of the house. He joined the army. This was just pre-Korean War. My parents didn’t like it, but they thought, “Well, maybe at least it’ll keep him out of trouble.” [laughs] He was also very, very bright, could get an A, 100 out of 100, on his physics regent and fail English. And everybody asked, “How could you fail English?” Well, he didn’t turn in his papers. [laughs]

With my father running the Boy Scout troop in the summers, I was allowed to go to the Scout meetings with him. I tried out the Girls Scouts, and I came home and they asked me, “Well, what did you learn?”

I said, “Oh, it’s going to be great. We’re going to learn how to bake cookies, and we’re going to learn how to sew buttons.”

My mother said, “You already know how to sew buttons. I can teach you how to bake cookies. What are you going to learn there?” [laughs]

I was excited by whatever they told me. But my father said, “Right. You go to the Boy Scout meetings with me.” So I never went to Girl Scouts, I went to Boy Scouts, which was good. My father would run a class, basically. Every Scout meeting was a class. In the summertime he’d run that class on the beach on a Sunday morning.

The one thing that I was not allowed to do was not to answer a question if I knew the answer. Since he knew what I knew would be the
answer, I didn’t have a choice in this. I couldn’t just blink my eyes. So I learned in a crowd of eight boys plus me, sitting around in a circle on the beach. He would ask questions about geography, about science, about whatever he thought to ask a question about. The boys were older than me, but if I knew the answer I was to speak up. He probably put questions in that he knew I could answer and was testing me. That’s what taught me to speak up in a roomful of men, and that was probably the single most important thing that I had going for me when I got older, when I started heading towards doing real intellectual stuff.

I would be asked again and again by women, “Don’t you feel uncomfortable? You were the only woman in there.”

“Yes, I guess I was.” I related to the men as persons, and I knew them all, and it didn’t matter to me that I was the only woman in that room, and it didn’t matter to them either. They were used to me, and that was it.

So now heading back to high school, of my teachers in high school, two of the three I remember very well were women. Sophie Wolf in biology and Ruth Goldstein in English and film.

The high school was fantastic. If you take a list of the people who came from there, it ranges from Arthur Kornberg here, Paul Berg, Arthur Miller. The number of people who did very well at different things was really outstanding.

Kiefer: What high school was this?

Herzenberg: [00:31:59] It’s Lincoln High School.

Kiefer: In New York City?
Herzenberg: [00:32:01] Yes. It served Brighton Beach, Coney Island, and a portion of—I don’t even know what you call that neighborhood. A highway divided the Brighton Beach side of it from the other side, the Avenue Z neighborhoods. We also had Woody Guthrie in the neighborhood. Brighton was a mixture. It was a constant fairly friendly argument between people in the Communist Party versus the majority, which was made up of various shades of left of where my father was, which was Democratic. He had worked in electoral politics. There were people who argued electoral politics is no good. Others said electoral politics is good. There were some card-carrying Communists prior to World War II, but after the war there were a very few that I knew about.

The whole neighborhood, however, was a fellow traveler in one way or another. Workmen’s Circle [Jewish fraternal organization committed to social justice] was an important organization. My parents, politically, spent their time getting a library put into our neighborhood. It took them about five years of begging City Hall, but we finally got it. My mother and all the kids had to walk either to Sheepshead Bay or to Coney Island to libraries. That was a dangerous walk because there were the kids from the Catholic church who used to beat up the Jewish kids. [laughs] These were complex neighborhoods you’d go through, and you’d get stoned. So my mother fought to get the library put in. She lived in the library. We couldn’t buy books, not all the books she wanted to read. In a poor neighborhood whoever heard of buying books? So it was fantastic to have the library nearby.
There was also a gay guy who moved in. We lived on a lane, and he and his mother moved close by. They were South African Jews who had relocated to New York. She was in her seventies and he was in his forties, I guess, and took a strong liking to me. They both did. I became an extra daughter for the mother. My mother was very close to her. She held and comforted her while she died. You couldn’t get an ambulance in those times. These days our neighbor would have lived. She had a minor heart attack.

Mary Finn had a library. She and David were readers, they were educated, and she had a library in the house. She would let me read from that library. It was an all-fiction library. But I remember that bookcase. I can still see it. I was allowed the books but not the ones on the bottom shelf. She would eventually say, “OK, now you can read these.” She would pick out stuff and guide me through. So I read—oh, you would know all the names if I could think of them right now—but they included the short story “Rain” by Somerset Maugham about Sadie Thompson. That was the kind of thing on those shelves.

David, who was a hairdresser in one of the big department stores, was always there. He looked at me…this was when I was just going into high school, and somebody had given me a horrible permanent wave, and my hair was such a mess. He looked at it, and he said, “No, no, no, this has got to go.” [laughs] So he cut my hair.
Then he took me to my first concerts, and he took me to his friend’s apartment in Greenwich Village, and I would hear music and see things. The neighbors would be on Mom. “You let him take her?”

“He’s not going to do anything to her. He loves her.” [laughs]

So I grew up with this freedom with people in my life that very few in our neighborhood ever saw or heard, the Scouting, my father’s friends in the Scouting who were craftspeople and very bright people who were trying to lead Scouting in a direction that it refused to go, but he tried hard.

There were just all sorts of things like that, and they didn’t make me be a girl like the girlie girls. They had no real desire for me to be that, nor did they see that I would have ever been able to be that.

I had an uncle, a much younger uncle, very close to my brother. My brother took me to see Pete Seeger when he played to a room of ten people. He hung around with that crowd. My uncle was friends with Seeger himself and the Peckskill crowd. So my uncle was very left. I don’t think he was ever a card-carrying member of the party, but he and my father would talk politics all the time about it. He was very much involved in the artistic and intellectual community, and his best friend was Paul Robeson. Robeson loved my grandmother and was always asking my grandmother to cook for him.

**Kiefer:** When you were in high school did you decide that you wanted to go to college, or was that expected in some way?

**Herzenberg:** [00:38:19] Well, of all my friends in high school there were few girls. One girl was Communist Party. Her family was card-carrying Communists. They
were very intent on party stuff. But she was my closest friend. My brother said, “No, I know about that stuff. You’re not going to join the Young Progressives, the “youth arm” of the CP in those days. They’re no good for you.” OK.

Of the two friends I had, one of them went to Young Progressives and went to college. The other one got married to a butcher. That was what her parents had in mind for her, and she was happy with that. Those were the only two friends. The rest of the people that we hung out with were the boys.

Creative Writing Club, I was very active in that. I published a story in the Creative Writing Club. I was very active in the film class. Ruth Goldstein was very active. She had a film class that was phenomenal. It was what you would get in the best film class in colleges today. They were making *Cyrano de Bergerac*, and she had an “in” with the filming of it. She brought the actors from *Cyrano*, José Ferrer and these people, to our class. Our whole class saw the first showing of it. We did all sorts of things like that, so we were given wonderful stuff. The class was mostly boys. There were girls in the class, but it was mostly the boys who led the class.

When it came to deciding where to go, we all had to go and see our counselor. The boys I hung out with got scholarships to Harvard and Yale and Princeton. Schools like that were working to bring in ghetto kids at that time, Jewish ghetto kids particularly. This was right after *Gentlemen’s Agreement* was a film, and the colleges were beginning to open up, so there was place for people.
I went, and the counselor said, “What do you think you want to do?”

And I said, “Well, I don’t know. I guess I want to go to college.”

He said, “What college do you want to go to?”

I said, “I don’t know. I guess Brooklyn College.”

He said, “That’s a good idea. What will you be?”

I said, “I don’t know. I guess I’ll be a teacher.”

He said, “That’s a good idea.” And that was the end of my college counseling.

So I did get to Brooklyn College. I aced the entrance exam. I was seventeen and a half. It was 1952.

Herzenberg: [00:41:15] Anyway, I got to college. Everyone in the family thought I should go to college. There was nobody saying, “No, she should go get married.” That was not really what the line was. I had lots of boyfriends. I was very cute and had different kinds of boyfriends, very different. I probably liked the more abusive ones more as I look back over it but never really hung with them because I wouldn’t allow myself to be abused, but they were interesting. “Abusive,” meaning ones who thought they would get into positions of power, moving towards what they considered powerful.

One in particular organized me to work for his father for a summer.

One summer I worked folding dresses in a small retail dress shop. That was awful, I hated that, and they didn’t like me either because I couldn’t keep my mind on that. I was more interested in “Look at the design,” or whatever it was.
So the next summer I worked for Freddy’s father, and that was an extremely bad experience. Freddy’s mother ran their insurance business out of their home, but she was away for the summer. So my job was to come in in the morning and go to Freddy’s apartment, which took me about half an hour on the train or so.

In the beginning, it was fine. Then his father started meeting at the door without any pants on and all sorts of stunts and chasing me around the house. And I was too embarrassed to tell my mother what was happening. My father would have gone and killed him. You know, you just were so embarrassed you didn’t know what to say. I would call my girlfriend and say, “What am I going to do? What am I going to do?” So I started taking my girlfriends to work with me. I did everything.

There was this boy in the neighborhood who really liked me, and he said, “I’m going to kill that bastard.” I had told him what was happening.

“No, I need the money.”

So Freddy’s father never actually did manage to do anything to me, because I was faster than him. He was a small guy. He couldn’t really overpower me. But it was like a real experience of knowing what it was to be chased around the block very lewdly and very embarrassingly. He was a little bit afraid I would tell his son what was going on, too, so he wasn’t quite there in terms of this, and eventually the job terminated somewhere in mid-August. I just had enough. And nobody made me go back. But I just couldn’t tell my parents what he was doing, and I really understand where these kids come from, but it really was that kind of experience.
Finally I started college. Brooklyn College was a coed college. It was not a boys'-college-turned-coed college. It was coed from the beginning. It was a great place. Nevertheless, the girls were there to knit socks and the boys were there to get an education, and I was not a socks-knitting girl. I knew how to knit socks. I knit beautiful socks. [laughs] Freddy got me to go down on a fraternity weekend one year. I remember riding down on the train knitting the socks.

[telephone interruption]

Kiefer: So to continue with Brooklyn College as you began school there—

Herzenberg: [00:44:57] First of all it was scary. As I’d said, I was spoiled by my parents because they did a lot of my education. So as big as my high school was, it was tiny compared with seeing this huge mob of people. It was hard to kind of flow in. I didn’t really know what I wanted to be in any sense. I didn’t even have a feel. I mean, I had excelled in high school in creative writing. I hung out with the math team although I was never on the math team. I did student time in the biology lab helping us to prep for classes.

So I had all of these interests, and I landed in Brooklyn College. Now I’m trying to decide what am I going to do. You have no counselors, you have no real guidance, nobody knows you, no one knows what you should do. I don’t have any family members who ever went to college, except for my brother, and he was at that time at LIU [Long Island University]. He finished LIU. So he gave me some guidance, but he really didn’t exactly know what to guide me with. Actually, I wound up guiding him and finding an opening for a job for him in the psychology lab that ultimately led him
into getting a Princeton PhD in psychology and into academics. So I was probably more of a guide for him than he for me. [laughs]

So I was kind of lost, and I decided to take, in my first year, biology, English, and math, which is pretty standard. My English teacher turned out to be a homosexual drunk. No, not bad, not bad. He was a very good guy and quite talented, but he liked the boys. He didn’t dislike girls and he was not attacking to girls. In fact, he kind of put them up on a pedestal. But it was more boys who hung around him than girls. He wasn’t drunk during the week in class, but he would invite people to his house, and he would get drunk, and you could come to his house. I went a couple of times. Nothing bad would happen there. It was kind of like if you could be that way, that’s how it was good to be. So we were lucky to meet him.

The math teacher was a disaster. I took statistics, and all he talked about was baseball statistics. He had no use for a girl in his class. Biology, on the other hand, was an excellent program, and so I gravitated much more solidly to that.

There was something called the Society of Biology and Medicine that I joined because you could join it. It cost a dollar to join or something like that. The Society was formed so that people who were headed to medical school would be able to say they belonged to a Society that was interested in biology and medicine. But it actually was run by a few people with serious interests in biology and medicine. They understood about graduate school, and the professors were very good about this. Some of them were, very, very good.
Len [Herzenberg] and his buddy Jerry Schiff and another guy named Billy Aaron are the three I really remember from there, and they all got PhDs. They all went on into academics as research scientists. Jerry taught at Brandeis for years. Len, you know. And Billy Aaron was at Friday Harbor.

There were some women in the group, and this is where the story gets interesting. As a freshman I was very excited to belong. The big thing about biomed, as it was called, was that biomed had an office. It had half-sized classrooms, half width. You had double modules and single modules. It was a single-module office, and we had some desks in there, and a place by the window where you could sit on the radiator or air conditioning, whatever it was. That was my favorite perch.

You could bring your books and stuff in there, you could hang your coat on a hook, and you had a home. When you came in in the morning, you didn’t have to hang out at the lounge, which is what most people did, but I found it not possible for me. There were too many of the girlie girls in the lounge, and I didn’t relate to them or to the whole sexual marketplace that was going on in there.

I wanted knowledge and I wanted to learn. I had been brought up that way in my family. My father and mother took me to every museum in New York. When they kept me out of school it was to do something educational, so either it was to go around with my father when he was driving around the city and could teach me all day long, or it was to go with my mother to one of the museums or something else, a Broadway show or whatever she thought was culturally worthwhile.
So BioMed was more like home for me, and it was that for both men and women---boys and girls, as we called ourselves at that time.

In the spring we had a social where we sold plants to raise money for a scholarship to send someone to a summer course. Usually people chose the Woods Hole, Massachusetts, summer course. We had access to a greenhouse, and the greenhouse professor was glad for us to grow the plants. So we would care for them throughout the year and then have a plant sale usually right before Mother’s Day. This was how we made money for our scholarship fund. After that we’d have a big banquet to give out the scholarships. So as a freshman, I got recruited into this.

My advisor was Jerry Schiff, who was Len’s [Leonard Herzenberg] best friend. I liked him and could come to him at the club and say, “What class should I take?” Or, “I don’t understand this thing. Can you explain?” If it was botany he could explain anything. The advisors were there for you as older brothers and sisters.

Before the scholarship banquet, I said, “What can I do to help?”

They said, “You can help by being responsible for the cocktail party,” which actually meant buying and putting out nuts and pretzels before the dinner. I think the whole event was nonalcoholic because it was in the school lounge.

I got assigned to work with Len, whom I hardly knew. He had a car, and so he was going to drive and get the nuts and pretzels, and I was to be his helper. I really enjoyed it. We had a very, very nice time doing that. He was interesting to talk to and fun.
I had a boyfriend at the time who was on the fencing team at NYU, and we were kind of breaking up, but it wasn’t anything. My parents were not particularly in love with Martin. He was a nice guy, but they thought he was rather strange. They were right. But he was beautiful. He was probably the most beautiful guy I ever went with. [laughs]

Anyway, Len and I bought the nuts and pretzels and put them all out at the party. Then one of the girls in the club came over to me and said something like, “You don’t know it, but some of the boys and some of the girls are attached. There are all sorts of things you don’t know about. You’re just a freshman.” Bottom line, stay away from the boys in the club. Which one of them was I supposed to stay away from? They’re all my friends. I had no idea.

So the following Monday, I went to Betty. She was a straight arrow. I said, “Betty, who was I being told about here?”

She said, “This girl thinks that Len is her special prize and that he belongs to her, and anyone who sits next to him, she carries on about. Since you were helping him do stuff for the party she was probably picking on that. Don’t worry about it. Just forget it.”

Well, of course, I did worry about it, and I made sure to stay away from Len as far as I could stay, which was pretty far. So he didn’t find me.

Then I got poison ivy and was out of school for two weeks just before finals. My problem was that I needed somebody to help me get through my statistics exam because I wasn’t very strong on it to begin with, and then with having two weeks out I was way behind. This professor was
not about to give me even a break. So I asked Jerry if he would help me, and he said, “I don’t know much statistics, but Len does.”

I said, “Well, never mind. That’s OK.” [laughs]

But somehow Len came looking for me, found me, and said, “I heard you needed some help.”

I couldn’t very well say, “Oh, I’m not going to do this with you.” Anyway I liked him. He was a very nice guy. So he started tutoring me, and I guess within a couple of weeks we realized (as much as anyone can realize at that age) that we were very compatible and just basically fell in love.

Kiefer: He was a senior and you were a freshman?

Herzenberg: [00:55:52] He was a senior and I was a freshman. I often say it was probably my parents who picked Len for me. He was the boyfriend I brought home who they thought was the one I’d be best off with. They liked him.

And Len was very fun. After we went out a couple of times his greatest interest in response to “What should we do tonight?” was to go home to my house and play gin rummy or bridge with my parents. This kind of social card playing was a big thing in our culture.

Len and I just hung out together from that time on. We got through finals, I passed statistics. Being together was just easy. “Where are you going? I’ll pick you up. We’ll go to school,” et cetera.

We used to go back to his house, because his mother was not there in the afternoons. I would do my homework and he would help me, but he also would do whatever he had, too. We could kiss each other in the house, and there was nobody to see us doing it.
We used to listen a lot to *Iolanthe*, to Gilbert and Sullivan. That was our idea of a great date. [laughs] We’d sit close to each other. But anyway that was fun.

Len won the club scholarship, so he got to go to Woods Hole for the summer. That meant he was going away, first to Woods Hole and then to Caltech, where he’d chosen to go to graduate school. We had a solution to being separated (I think it was Betty, again, who organized this for us). Girls could come up to Woods Hole and make beds in the student dormitories. The students were exclusively male that year (probably always). The dorms always needed chambermaids. After making beds you could hang out at the library, in the classrooms, whatever you wanted to do. A number of young women like me got in. We slept in a dormitory, and it was all very straightforward. We got room and board in exchange for the bed-making.

Do you know what Woods Hole is?

**Kiefer:** It’s a lab in Massachusetts.

**Herzenberg:** [00:58:17] Yes. It was the summer biology community. Everybody, the very famous biologists and these people would come, and they would have labs there or they’d read in the library or whatever they did. But they would go to Woods Hole for the summer. It was the equivalent of going to the Hamptons or something if you were rich.

**Kiefer:** Did they have women students there, or did the women, the bed-makers, also get to observe?

**Herzenberg:** [00:58:50] The women were the bed-makers. There were no women in the class. But the professors didn’t care if I wanted to hang out with Len while
he was in class. I got dorm work done early and then came to the laboratory.

Len and I did a research project there. We had clams and would drop some starfish extract on them. This made the clam’s heart beat very quickly. Actually, they were pectins, and starfish were their predators. It would make the pectins active when you dropped the extract into them. We were just observing the response, quantitating it. Basically we were learning the species and learning all the invertebrate stuff. And whatever Len was learning he was teaching to me. So for us the beginning of our academic life was when I got up to Woods Hole.

My parents were shocked, because I would never sleep at anybody else’s house, but I just took off and went to Massachusetts for the summer. My father said he was waiting for the call, “Come and get me,” because I wouldn’t even sleep at my Aunt Ruth’s house. But I went up there and I just stayed, because that was where I belonged, and it fit. I was liberated from Brighton, from New York, from all this stuff that was there.

I spent another year at Brooklyn College, but Len and I got formally engaged by the end of the summer. He gave me a small ring, and his mother had a fit. She said, “You’ll change your mind.”

He said, “No, I don’t think I will.”

She wanted him to marry a rich girl, that was the problem, somebody with breeding and culture, which my parents did not ever aspire to. [laughs] They aspired to education. It was a different thing, knowledge. That was not in Annie’s book. She wanted Len to “marry well”. But Annie was a great
woman, she really was, and I wear her own engagement ring to this day. It was very precious to her, but she gave it to me a couple of years before she died. She said, “I want to know you’re going to wear it.”

Kiefer: How lovely.

Herzenberg: [01:00:44] This was her prized possession in her life. She had worked during the depression to earn the money for it, even before she met Len’s dad and got engaged to him. She married very late for a woman at that time because she loved her work. She was a legal secretary and really ran the whole office, even then.

Kiefer: So at the end of that summer, Len went off to Caltech and you remained at Brooklyn College.

Herzenberg: [01:00:54] Right.

Kiefer: You must have missed him.

Herzenberg: [01:01:02] Well, it was three dollars a minute to make a phone call from New York to California in those dollars, not our dollars, so I don’t know what you’d figure. It was about fifty dollars a minute. So you didn’t call, you wrote. We wrote letters at least twice a week. We’d go back and forth with letters. I did miss him, but I knew what I was doing, and that’s where I was going. I was supposed to spend three years and finish college, and he would spend three years and finish graduate school, and then we would get married.

But after about three months apart this didn’t make any sense. I enjoyed being home with my parents. I had no real reason to feel I had to go out and date. I just was not interested in dating. But he was worried that
I would, which he didn’t want. And he was lonely out there, very lonely. There were couples and there were non-couples, but he was neither.

Somewhere around October or November he said, “OK, we're going to get married, and I’ve got it all organized. I can save this much money. You’ve saved some. We can do it.” Len had one of the first US government fellowships. Out of the fellowship stipend he got for the first year he saved enough money so that with the fellowship continuing we could manage to live the second year (and a third, as it happened).

Despite the cost, Len’s mother had a twenty-minute phone call with him trying to argue him out of getting married, but it didn’t work. She couldn’t convince him.

My parents told her, “We don’t have to have a big wedding. We can have a small wedding. When he comes out here you can have this discussion.” My parents took the position, “They can do what they want.”

Annie, bless her soul, came from this Flatbush middle-class group, all her friends, they had “money”. Each one had a wedding or bar mitzvah, and when you had that everybody brought large checks. She’d given large checks to all her friends’ kids, and she now wanted to collect them back. Len was her only son, and she wanted the pomp and circumstance. She loved pomp and circumstance.

Somewhere during the discussions, after my parents suggested that we wait until Len comes back for the summer and then decide, Annie thought about it and said, “No, I’m not going to convince Len otherwise. I bless them. Let them get married.” And she had an engagement party for
me with all of her family and mine. She ordered a beautiful heart-shaped cake because my birthday’s right next to Valentine’s Day. She acquiesced, and I became very much part of the family. She never really complained, but she never did fully approve of me, at least until many, many years later.

Annie bought me the kind of [calling] cards they had at that time. I still have them. They were cards with “Mrs. Leonard A. Herzenberg” on them. [laughs] And you were supposed to send them. I used them to thank everybody for the gifts, and the rest of them I never used again. After writing my “thank you” notes I had no place to use a calling card like that. She imagined that’s what academic people would do, but nobody ever did anything like that.

So that’s how we wound up getting married, being in touch with ourselves, and then just driving out to Caltech the next season. That was the beginning of my real education.

Kiefer: So that was the second half of your sophomore year.

Herzenberg: [01:04:43] Well, you see, my freshman year started in February, I graduated early from high school. You could do that. I started second semester, so I ended formal schooling with a year and a half. Then because Len’s mother said, “She’ll never finish her degree, and she’s got to finish her degree,” I enrolled in Pomona College.

Kiefer: That’s one of the Claremont Colleges, correct?

Herzenberg: [01:05:11] One of the Claremont Colleges. And at that time, to drive from Pasadena to Claremont, (there were no freeways), you just drove down the equivalent of El Camino here and went through the various towns, sort of
like 101 used to be. But they had the very thick tule fogs and I had to drive that distance. I didn’t have a problem with driving except driving through those awful fogs to get there. Fortunately, I met a young Jewish refugee from Ukraine who was also going to the school. She lived halfway between my house and the school. So I would drive to her house, and then we’d drive together through the fog, which was a lot better.

But the big problem with Pomona was that they were teaching us how to do gram stains, staining bacteria to classify them by the method of the class. The biology was lame. The curriculum at the time was very old and was basically aimed at teaching lab techs and people who would never become biologists. My organic chemistry was great, and history was OK, but the rest of it was disaster.

I would drive to school and then come home to Len’s lab at Caltech, where I would spend the rest of the day until the gang went to dinner. Caltech and the lab was where real science was for me. This was when Jim Watson had just come back to Caltech. There were, I think, amongst our teachers, five future Nobel Prize winners. The most exciting, vibrant biology and genetics was going on at that time.

There was also politics. Our group at Caltech was fighting the Tenney Committee, which was the equivalent of the McCarthy committee in California. Everything going on was in that community, in that one Caltech building called Kirchhoff. George Beadle was our chair, the professors, our scientific guides. The most exciting political leadership was from a guy
named Matt Nesselson, who, like most of the other people in the group, later became very, very famous for molecular and biochemical studies.

So I got into -- was in -- that community. My schooling was something outside that. I just simply had to pass exams. And after one semester of doing this Len and I realized that we were paying for me to go and do something which was really stupid, and it was expensive. I wasn’t learning anything. I was learning was when I came back to Caltech and went to the lectures and listened to the grad students and professors talk.

So I left Pomona. I don’t know what they thought of me at Caltech. I can’t even imagine what they would think of this eighteen-year-old, and that’s all I was. I was 18 1/2. I was tiny and had a “mouth.” I was everywhere. What they thought of this, I don’t know. I know I loved it.

There was a woman named Mary Emerson, who was the wife of Professor [Sterling] Emerson. Mary worked in the lab and collaborated with her husband. I know that she worried what would happen to me. She was motherly towards me in that sense, feeling that they ought to do something with me and that I should continue to get a formal education. So she encouraged me to ask whether I could sit in on the classes. I did, and the response was good. They said I could informally take the labs and the classes. Caltech didn’t admit women at any level at the time. They said that if I took the exam for a class I could have a letter from the professor saying what grade I’d gotten on the exam and how I’d done in the class.

It was great. They treated me like a regular student. It was hard but fun. There were only two undergraduates who did biology, so there really
wasn’t an undergraduate program. It was all graduate school classes that the
two Caltech undergraduates and I attended.

**Kiefer:** In biology or the school itself?

**Herzenberg:** [01:09:36] In biology. There were lots of engineering undergraduates, but
there were two who had a biology major. I was thrown into that category, so
now there were three of us. You were just treated like a graduate student
because it was a class. There were ten graduate students in the class, and
there were two or three undergraduates, and you just simply did what the
graduate students did because that’s what you’re expected to do.

So I started my graduate education without ever having finished my
undergraduate. I never got formal credit for the classes, but I had letters
from Max Delbrück [biophysics] and Henry Borsook biochemistry, Ray
Owen [immunobiology] and others saying I had gotten A’s in my classes.
Ray Owen considered himself my graduate professor. I was very proud to
“belong” to him, and somehow he was always proud that I considered him
my professor. He’s still alive but he’s nearly 100 years old now.

**Kiefer:** Ray Owen, did you say?

**Herzenberg:** [01:10:42] Owen, yes. He reappears in my narrative as I go along.

**Kiefer:** Did you ever work with Jim Watson [co-discovered structure of DNA,
Nobel Prize]

**Herzenberg:** [01:10:53] I knew Jim Watson. [laughs] At the time he would say he was not
a socially “apt” person. I think he’s never really become an apt person even
though he’s very famous, highly respected and has accomplished a
tremendous amount. [laughs] Jim taught one of the classes that I remember taking. So, yes, Jim was around.

Len’s professor was Herschel Mitchell. He was very good. I would hang out with him and Len. He was divorced and remarried, and he remarried a biologist. She worked with him at the lab, Mary Hoolihan. I could say what I wanted to him, even argue. Despite being a pseudo-student, I was not put down or told not to talk.

The department put me to work for Albert Tyler [biology] who needed a technician and secretary. He didn’t know what to do with me, since I was always asking questions. He didn’t have a clue at that time as to how to handle this. [laughs]

I hung out for lunch with Ed Lewis [Edward Lewis, genetics, Nobel Prize] and his grad students, who worked just down the hall. I don’t know who else. I’m just naming various names.

**Kiefer:** Did Caltech at the time have just men in the student body?

**Herzenberg:** [01:30:42] That’s right. Undergraduate and graduate.

**Kiefer:** And the faculty was mixed, if Barbara McClintock was teaching there.

**Herzenberg:** [01:30:50] Barbara McClintock, well, she was not on the faculty. I don’t think the faculty had any women. We had a woman postdoc. That was an unusual thing. A couple of women postdocs, as a matter of fact. But there weren’t any women faculty members in biology at that time. There were women who were very well respected in the department and doing research, but they were research associates of some kind. I don’t exactly know what position they had.
Mary Emerson was one of the women across the way from us, and they were all throughout the school. There were a number of…and Marguerite Vogt was there. She worked with Dulbecco [Renato]. I eventually shifted to working for her and Dulbecco. I washed dishes for them. Marguerite was, again, the same thing. She wasn’t on the faculty.

Kiefer: Was there encouragement for you or were there some roadblocks for your presence there?

Herzenberg: [01:31:50] There were no roadblocks for my presence. No, it was very encouraging. Everyone in the department was happy that I could find a job there. They let me into all the classes, and they were very, very encouraging.

All the way through I was just a fixture in the department. I belonged there. You know, when I look at it I say, “How could they do that?” Nowadays you’d have to be official to hang out there. It was nothing official. I was just allowed to do this.

Various professors knew me, and I’m sure they talked about me. They clearly they recognized that I was very bright and wanted to learn, that I was learning, and they took it as their responsibility to educate me because it’s our responsibility as professors to educate people. I grew up the same way and I do the same thing in my labs.

Kiefer: Sure.

Herzenberg: [01:12:22] Caltech was an incredible place. But the most important person for me was Barbara McClintock [Cytogenetics]. Barbara was this tiny little woman. She was littler than me, and I was little. She washed dishes in a restaurant in order to be able to make enough money so she could grow her
corn and do the corn genetics that she was doing, because women just didn’t get in anywhere.

On the other hand, George Beadle, who was the chairman of our department, recognized how brilliant she was and what she was doing. So he somehow invited her to Caltech. I don’t know the whole story there. But he invited her to Caltech, and she became a legend there. She was at that time about forty-five, I guess. She seemed very old to me. The joke always at Caltech was that there were nine professors of genetics in the department, which was headed by George Beadle, and George Beadle had to teach the undergraduate class. [laughs] So they were all geneticists, in other words.

But when it came to McClintock, it was said that Andy Anderson [E.G. Anderson], who was the corn geneticist, understood McClintock’s work, that McClintock talked to Andy and then he to everyone else. Everyone else talked to Andy, that they didn’t understand her. She was up on a pedestal even among all of these biologists.

On a Friday night Len and I used to drive to Los Angeles to get a Chinese meal because you couldn’t get Chinese food in Pasadena. [laughs] So we’d always drive down there and then we’d hang out at the music bars, Dixieland jazz, mostly. We wanted to invite her. We stood outside her door. The place was all dark. Everybody was gone, and there was a light beam coming from her door. And if you can picture these two kids going up to the edge of the light beam and standing in the shadow of the light, and my saying, “Let’s invite her to go with us.” And Len saying, “Well, but she might be busy.”
“No, no. Let’s.” I was talking this quiet way because we didn’t want her to hear us. “But maybe she’d like to go.”

“Yes, but we shouldn’t interrupt her. She’s busy. Look, she’s writing notes.” We could see her.

I said, “Yes, but maybe she’d really like to go. We could ask her, and if she doesn’t want to go, she will say. We’ll say it in a way that she could say no.”

So we finally got the courage up, and we knocked at her door. “Dr. McClintock.” It was eight-thirty, I guess. “We’re going to drive into Los Angeles and get some Chinese food. Would you like to come?”

“Oh, I’d love to come.” [laughter]

So we took her in to Los Angeles, and we went to this seedy Chinese restaurant that we always went to that had good food. It was really nice talking to her, but I was so naïve that I actually said, very simply, “How do you make all these fantastic discoveries?” That’s an odd question to ask a famous scientist, but I was young enough not to know that.

And she said, “Well, it’s actually very simple. I go along doing my regular genetics, and I classify everything and I make hypotheses and I continue with this. But sometimes I find something that absolutely doesn’t fit into anything that I can think of. So then I repeat it to make sure that it’s repeatable and it’s real. And if that’s true, I store it in my brain as an exception. I keep a lot of these exceptions, and whenever another exception turns up, I scan it against all the exceptions that I have. Then I get little clusters of exceptions that seem to relate to one another, and finally they
suggest some theory, and they suggest an experiment. Then I do the experiment. And if the outcome fits, a theory click into place and I’ve made the finding.”

It’s all very simple. In fact, it’s how I work. I’ve worked that way all my life. She just taught me in that one minute what you do. But that is a very unusual way of working for a lot of scientists. So you can classify scientists. There are those that are paradigm breakers. There’s a book on this. It’s called the *Structure of Scientific Revolutions*, and it’s Kuhn [Thomas S.]. Everybody knows that book. It’s well known amongst people who look at things like this.

But anyway some years later I found that book and said, “Oh, this is an exact description of what we do.” But the point was that Barbara McClintock just gave me the code, and being given the code by someone who was highly respected, and I said, “OK, that’s the way you do science.”

So there are those who are paradigm breakers, that’s what’s in this *Structure of Scientific Revolutions*, and there are those who are paradigm extenders, solidifiers, fixers, cementers. Most scientists are the second type. They don’t like to live in the edge of the unknown, where you’re actually watching for something that breaks a basic paradigm. They’re happy if they can show that this gene actually does these two things and not just this one thing. That’s OK. But developing a new paradigm, saying that there’s a new kind of genetic material that has not been known before, that kind of paradigm breaking is really only for a few scientists who do it. And you don’t need to do that type of paradigm breaking to win Nobel Prizes. In
fact, most of the Nobel Prizes have gone to people who have created a
whole new field but not necessarily made the paradigm-breaking
observations. So that’s the area I like to be in, and it’s put me on the outs
with most of my colleagues most of my life. Not only was I a woman, but I
was a woman who dared to do this kind of stuff and argue this way.

**Kiefer:** To be a paradigm breaker.

**Herzenberg:** Yes. To be very simple, I got the idea that there is not a stem cell
for the hematopoietic system, there’s more than one stem cell, a different
one for the cells I work on and the cells that comprise most of the immune
system. This really drove us and a lot of other people to spend twenty, thirty
years to actually prove it. I postulated it together with Len somewhere back
in the eighties, and we just proved it this year. But now it’s “always been
that way”. Everybody, it seems, always knew this was true. [laughs]

**Kiefer:** So was Barbara encouraging to you? Would you say she was like a mentor?

**Herzenberg:** She wasn’t a mentor. I saw her several more times while she was
visiting at Caltech but not in any special way. But she was a mentor in that
she was the person, a woman, who told me how to do what I wanted to do
with my life and how to do it. I would go and see her whenever I happened
to be somewhere where she was. This happened four or five times. I’d
knock on her door and I’d say, “Dr. McClintock, you may not remember
me.”

“Oh, I remember you. You took me to that Chinese restaurant.”

[laughs]

**Kiefer:** Perfect.
Herzenberg: I never asked her if she remembered the question I’d asked her. I was too embarrassed to ask such a question, but she probably did remember and was probably bemused, not just going to the Chinese restaurant but by these two Jewish ghetto kids from New York. We spoke with New York accents, and we knew nothing of the rest of the world. We were learning it fast, but we were ghetto kids.

There were people at Caltech from all over. There were corn people, and some of them were third-generation academics at Caltech. It was the most elite place you could be at the time. That’s why Len was sent there. His professors at Brooklyn College, particularly Priscilla Pollister, told him, “This is the school you have to go to. I don’t care if it’s across the country.” They were amazed that he could get in, and they really felt, and rightly, that one of theirs had really made it.

Kiefer: So the Nobel Prize winners that were there in the course of your time were Jim Watson and…


Kiefer: Was Jacques Monod there?

Herzenberg: Jacques was not there. Jacques had been there before we came. When it was time for us to move on (time for Len to graduate and do a postdoc), we decided on France as our next stop. I had a French girlfriend whom I was trying to arrange to come and stay with me. She’d grown up in the house behind my house in Brooklyn, which was very strange, because, as
I said, the whole neighborhood was Jewish. There were only a couple of other kids.

My friend's backyard and our backyard abutted, and my mother liked her mother. So we, as ten-year-olds, became friends, and I was in their house as much as I was in my house for two years until they moved away. But Claude and I kept in touch. She had some real tragedies. Her father had left her mother, and there was a mess. I mean, things like this didn’t happen, but they happened to her. Her mother went back to France and took her and her sister with her.

I was trying to arrange for Claude to come back to the States in some way and stay with us until she could go back to school or do what she was going to do. That fell through. At the same time, Len was trying to think of where to go next. Herschel Mitchell, his professor, said, “Well, why don’t you go to Monod’s laboratory in Paris. That would be really good. Two other people from here went there as postdocs”

So Len came back and said, “Let’s go to Paris.” I thought, that’s fantastic, because I would see Claude and we’d be able to get ourselves going. So we went from Caltech to Paris and to Monod’s lab.

I’ve just finished reading a wonderful, wonderful description of Monod from 1930-something onward, of his life and his major part in the anti-Nazi Resistance in Paris. [Brave Genius by Sean B. Carroll] The book covers the period from the thirties to the sixties, till just a while after we left Paris. It’s an incredible biography of Monod with Camus. Way later than that, after we came back out here to California, I got interested in Camus
and Existentialism, and I started reading all of Camus. I don’t remember that I knew that Monod was friends with him. Camus had died before we got to Paris, so it might not have been talked about that much.

Anyway, Len and I land in Paris. I’m pregnant by this time, intentionally. We’d been married 2-3 years. You’ve got to have a baby. You had to have your kids at that time. So we decided after graduate school we would try and get pregnant. I was pregnant by the time we left New York.

We sailed on the old Normandie because you couldn’t go by plane. That just didn’t happen. You had a trunk to carry. You went the cheap way, lowest class on the Normandie.

**Kiefer:** This is what year?

**Herzenberg:** [01:25:35] This is 1955. Yes, ’53 to ’55 is Caltech, and ’55 to ’57 is Paris.

I have a wholly new perspective on those Parisian years after having read this book about Monod. Maybe one of the saddest things about me losing Len was that this book came out after he passed away. We would have read this book out loud to one another, and we would have enjoyed it enormously because it’s an understanding of Jacques that we didn’t have.

When we got to Paris Len said, “You like to paint, you like to do art, you like to do writing. Why do you want to hang around the lab?”

And I said, “I just want to go to the lab. I don’t want to do anything else.” [laughs]

He took pity on me. I was pregnant. He said, “OK, OK, come to the lab if Monod will say OK.”
So I went to Jacques and I asked him. He said, “Of course you can do that.”

Jacques was in the lab a lot. We saw him every day whenever he was in residence in Paris, which was most of the time at that time.

**Kiefer:** When you went to France, what institute or university were you connected with?

**Herzenberg:** [01:33:16] None at the time. Pasteur Institute is itself, and Monod had his laboratory there. In France, if Monod said you could work in the lab, you could work in the lab. That was the end of that.

So I worked with Len all the time, except on one great project that I decided to do by myself. Len thought it was crazy. Funny project. [laughs]

In Monod’s lab, we worked with something called galactoside. It’s part of lactose. It’s just sugar, and bacteria will grow on the sugar. But if you substitute sulfur for one of the oxygens, it’s called a thiogalactoside and Jacques had said, “Nothing will grow on a thiogalactoside. It just won’t grow.” That was true, so far as we knew. We didn’t sterilize any of our solutions. We just dissolved the thiogalactoside into solution. We never had a problem with it. Nothing ever grew on it. It was very central to the work we were doing. Everybody in the lab was working with it. There were several thiogalactosides and you never sterilized any of them.

I said, “But Herschel Mitchell [Len’s Caltech professor], said something will grow on anything. So I went outside and scooped a handful of dirt and put it in a flask and put some water in it, and put the flask in the
cupboard. I used sterile water so I would know if whatever grew came from the dirt, and I put the flask in a cupboard.

Two weeks, four weeks later, I don’t know, I took the flask out and sniffed it. Hmm. There’s sulfur coming out of this. There’s something growing in here. [laughs] “Look, Jacques, something grows in the thiogalactoside.”

“Oh, that’s amazing. That’s really interesting.” [laughs]

Except for one thing. After that time, we had to sterilize all the galactosides. Everybody did. They were showing it and sniffing it and taking the plug out. Bacteria got into everything. We had to sterilize all the solutions after that, so I left my mark permanently on Pasteur in terms of bacteria in the air. It was funny. [laughter]

Kiefer: That’s interesting.

Herzenberg: [01:35:44] It’s a funny thing to do.

So reading this book now I really got into remembering a lot about Pasteur, and I asked myself, how did he see me? Why was he so willing? I was pregnant. When we had the baby, I hung out with the baby at the lab. I would come in in the afternoons with her, and it was great. Jacques was around a lot. He’d come into the lab and talk to us a lot. Not that he’d sit down and talk to people, but he’d just always be talking to us about the science, about whatever.

I know I can remember explicitly one time. I learned French. Somebody gave us a housekeeper. We couldn’t afford a housekeeper, but they said, “If you’ll give her room and board, we’ll pay her salary. We don’t
want to lose this housekeeper.” So we had a room upstairs that we could let her have, what’s called the maid’s room in France.

So Françoise came to work for us. She was my age, maybe a year older, two years older, something like that. She had been taking care of their babies, and she spoke only French. So my French had to get better than it was. I learned to speak French with her and to understand French that way.

Jacques used to push us to speak French, and then he would switch to English. He was fully bilingual. When he’d get impatient with our halting French, he’d switch to English, and then you’d tease him, then he’d say, “Who started speaking English?”

And we’d say, “It was you, Monsieur Monod.”

“Oh, OK, I’m sorry. Back to French.” Jacques was very cute. [laughs] He was a very sweet guy, a very nice guy, and very bright and broadly cultured. For example, he had to choose between leading the Pasadena Symphony Orchestra and staying in science. He was a wonderful cellist.

So one day, I said, “Eh, bien,” with a very country accent to Jacques. It means “Oh, good, or OK”. I don’t know what I was talking about. It was a slangy comment that I just said to him.

And he said in a mock shocked voice, “Lee, où avez-vous appris dire ça?” “Where did you learn to say that?” [laughs] Remembering that interchange is important because it reminds me of the social nature of the intercourse that we had with Jacques in the labs. It wasn’t like he only came in and talked to us about science for a few minutes. He took over the education of the Americans who were there.
We ate lunch in a common dining room. It wasn’t a dining room; it was one long table that we sat at in a glass-roofed old lab that was converted to a lunchroom. Everybody ate at this table. It was required. Nobody said you must, but Jacques and Andre Lwoff, who was even more senior than Jacques, intended you to do that, and you knew it.

The discussion at the table was often politics. The French scientists, from Monod down, were all very left-wing but not communist. This was after the split when the Communist Party—you don’t need that history. Hitler and Russia made a pact that put them both on the same side. Now, the French Communists were very anti-Hitler, and so a very large number of them didn’t really want to follow Russia. They belonged to the party because the party was what a lot of left wing people, aka liberal now, belonged. Others were like me, who understood that they should never join the party but were friends with people in the party. Anyway, most of them split with the party at this point.

I grew up with this ping-pong game. I have very early memories of that discussion of should you join the party, and my father saying, “No, it’s really not a good thing to do, that they don’t, in the end, have the goals you want to reach, and that’s not going to happen, that they’re going to fool you into thinking that, and you’re going to be part of the party, and you’re supposed to go with party discipline and all the rest of it, but it’s bullshit. Don’t go for it.” So I learned this. My brother said the same thing and kept me out of party even though I had friends who joined. My family made this
kind of politics quite clear to me before I could even really understand. But as I grew up I wanted to understand it, and when I did I agreed with them.

Right in 1940 there was a big split and a lot of people moved away from the party at that time. Jean-Paul Sartre remained until after the war when the Cold War was beginning, and it was very clear to them that the party was not there.

Paul Robeson went back to Russia because he believed so firmly that Russia was going to have equality and the working man would be elevated. It was incredible bullshit, and I knew it because I was reading both sides of the game, even at that time. Robeson went back there and then he found the discrimination was horrible. It was worse than anything he’d encountered, and he came back a broken man. He had been vilified in this country for being a Communist and for going to Russia, and once that dream disappeared he returned incredibly depressed. He didn’t commit suicide, but it would not have been a shock in the condition he was in. It was really tough.

Kiefer: How many people were there?

Herzenberg: [01:38:55] I like to name off the people in that lab and interacted in this milieu, because it’s amazing who was there. They all agreed with my family. Georges Cohen and François Bloch, who became Minister of Science and France’s top minister, his wife Francoise, later ex-wife. Americans Harlyn Halverson and George Siskin were the two Americans who were there at the time. There were always a few Americans in the lab, but I think they weren’t as interested in the political lunch discussions as I was.
We were usually a group of about ten people. We could fit around this table which had room for about fifteen or sixteen people, and lunch was at that table all the time. These are all people who became very famous French scientists. If you were at Jacques’ table you were, or became famous, or at least did great science.

In the middle of our second year in Paris a draft notice caught up with us. Len had been drafted and he was way overdue for responding to the notice. It had been sent by postcard to Pasadena, forwarded through several offices and finally sent to Paris. It was months since it had been issued, but finally got to us. Len could be held up for draft evasion.

So we had to just pack up and go back to the US rather quickly so Len could “report” for duty. This was dreadful. We would just have to stop doing science, leave the lab we loved, et cetera. Shocked, we went to Jacques and told him what had happened.

He calmed us and said, “Well, you know, you can go to the NIH and you can do your service there. If you do that you can continue to do the science. There’s this guy Harry Eagle, who’s been after me to come and teach what we’re doing here to his lab. Why don’t you go in my place, m’ boy?”

Len had wanted anyway to work with animal cells. It was really what his goal was. He really didn’t want to stay long term in bacterial genetics, although shortly thereafter the studies being done got Jacques the Nobel Prize. However, at the time we were on the team. We did our part getting
towards the famous experiment that resulted in the Nobel Prize. However, we left before it was done.

**Kiefer:** And that experiment, roughly speaking, involved what?

**Herzenberg:** It’s called the PaJaMo Experiment because it was Pardee, Jacob, and Monod, the three initials, and it was basically showing that bacteria had genes and chromosomes and could mate and transfer genetic material. It started the whole molecular revolution.

We had worked on the system and helped to bring it to the point where that experiment could be done. In working on our part of the study I learned kinetic assays and rigorous quantitation. I loved to do plot data and do mathematical stuff. Len preferred good solid experiments, and I liked to do the theoretical, the statistical, the plotting of the data, the things like that. So, particularly after the baby was born, he would do the experiments and bring the data home to me. Then I would sit down and plot it all out and find out what the kinetics looked like. I loved that kind of mathematical stuff. It’s not high-level math. Most people doing this kind of work didn’t really like doing that, but math of this kind was something my father had brought me up to do. So it was just a natural extension of where I’d been before.

It’s an interesting story in terms of where I went from there and my next publication, actually, which I just happened to notice the other day. It was 1960 by this time.

**Kiefer:** So it was 1960, or ’59 or ’60, that you went to the National Institutes of Health?
Herzenberg: [01:43:37] We arrived there at the end of ’59.

Kiefer: And that was in Washington, D.C.


Before continuing with this, I’ll give you one more comment to do with Paris. In reading the history of Monod that I mentioned I couldn’t help wondering “What did Jacques see me as?” I mean, I know what I saw him as, and the history in the book is exactly as I saw him. It is such a good history that it is exactly as I saw him, but it filled in enormous numbers of details I had no idea about. One of them was I had no idea what his wife was like. I knew that she was a curator of one of the museums there, Musée Guimet. I didn’t know that she was Jewish.

Anyway, the description of Monod’s wife in this book made me think that probably Jacques saw me as an American ghetto version of his wife. There were just things that I saw that I immediately identified with. He was very, very much a feminist and felt his wife could have a career, and he supported her. They had twin boys born during the war. She was hidden away in a small town, and he would visit them during the war. But he was very busy heading the Resistance. He actually wound up being the head of a large…you should read this book. I mean, it’s gorgeous.

It’s called Brave Genius.

Kiefer: Brave Genius.

Herzenberg: [01:45:43] Yes. Oh, I can’t recommend it more. I realize that Jacques believed that young women should be part of science, and he did have a woman in his department at that time. He would give women a chance to be
scientists, and it was not because he was liberal. It was because it was in him
to really look at people, not look at what their labels were. In that sense, I
really saw a lot of my father also in Jacques, and in reading this book I could
see that there's just this element of personality that looks at people, doesn’t
put labels on them to begin with. Super cool.

Kiefer: That’s wonderful. By the time you went to Washington did you have more
children or did you just have one daughter?

Herzenberg: When I went to Washington Len and I had decided I should
work for someone else. I should be a laboratory technician. I had a career,
and I needed to get paid. He met one of his Caltech co-graduate students,
and that guy was looking for a technician, and so he arranged for me to
meet Bruce. His name is Bruce Ames. He’s very well-known for the Ames
Test.

Bruce said, “OK, you be my first technician. I’ll make my mistakes
on you.” I said, “Well, you’ll be my first boss. I’ll make my mistakes on you.”
[laughs] I don’t know that we actually said that to each other, but we
probably both thought it.

And Len went to work in Eagle’s lab learning somatic cell genetics,
and I went to work for Bruce. Bruce was still doing bacterial genetics of the
kind that I had done in Paris, and I knew how to do all the stuff that he
wanted me to do. Well, certainly not all. He taught me a lot. He was a good
teacher.

If Len was somewhat organized, Bruce was incredibly organized. He
was a graduate student from the same department, same professor as Len,
H.D. Mitchell, but they were very different. Bruce was very organized as he went through graduate school. He sequentially studied four enzymes in work that translated easily into a thesis. He worked this one, then he did this one, then this one, and finally this one. Very organized! Each one was a paper. Each one was organized, neat, clean.

Len’s thesis was a mess, in contrast. [laughs] At one point towards the end of it his advisor (H. K. Mitchell) said, “Answer one question. Why did you do this experiment? It is a very interesting finding, it is extremely interesting, but why did you do the experiment?”

Len explained, and Mitchell replied, “Well, that’s logical.” But basically Len agreed, it was a curious although very creative path. He had spent much of his graduate time sequencing proteins, but he wound up proving that the putative enzyme he was working on was not a protein at all, even though everyone initially thought it was a protein. It basically didn’t make any sense as to how he got the idea that the “enzyme activity” he was following was actually due to a mixture of fatty acids [laughs]. It was just brilliant intuition followed by good science to prove the fatty acids were responsible for the activity. However, at the time the thesis was to be presented it was not clear which fatty acids were responsible. It would take months to work this out. So the thesis was a mess.

Mitchell, however, said, “Look, you’re already committed to Paris, they’re expecting you there, you’ve got to go. Just write up your thesis. I know you know how to do research. You’ve learned everything you need to learn, and it’s time to go on.”
People have somewhat different attitudes of what a thesis should be. Some people still think like Mitchell but others, particularly now, think you’ve got to publish a Nature paper or you don’t have thesis. I don’t believe in that at all. I would never force a student of mine to do that. If the University rule is you have to publish a paper. OK, get a paper published. It has to be good, but it doesn’t have to be acceptable to the Nature editors.

**Kiefer:** You moved to NIH.

**Herzenberg:** [01:50:22] You wanted to know about how we did in Washington.

**Kiefer:** Right.

**Herzenberg:** [01:50:24] I worked for Bruce and I was his technician. He expected to tell me what I should do. I agreed. He was the boss. So we did things in the orderly way he expected. However, I would always be asking questions. He didn’t think about research in the same way I did. He didn’t have the McClintock view about exceptions and the need to remember them and follow up on them. He was, still is, very successful. We just thought differently.

Len was working upstairs in another building. There were other people in his lab I got to know. I did my work in Bruce’s lab, but I hung out around Len’s lab. Following in Len’s footsteps, I was more interested in the science going on there than I was in the bacterial genetics that Bruce was working on. I wasn’t uninterested. I didn’t go to sleep at night puzzling about questions about things I was doing in Bruce’s lab.

Then Bruce went off…I’ve written about this, actually…Bruce went off to St. Louis to the Kornberg department to spend six weeks there.
Again, this was still a time when you couldn’t afford to make a phone call. Nobody would think of calling someone to ask what to do. So he left me with, “I’ve synthesized the substrate for this enzyme. Just take this through. You know how to do it, get all of this information and have it lined up for me so when I come back we’ll go ahead and we’ll have all four enzymes in the pathway.” And that was great. He gave me the stuff, and he raced out the door to go make his train for St. Louis.

I took the stuff in to look at the spectrum of it just to check to get a feeling on the concentration, and it had the wrong spectrum. I said, “Wait a minute. This is not the right spectrum. That means it’s not the right compound. There are no experiments to do. Everything Bruce told me to do I can’t do because this is not the right compound.” This was catastrophe. As I said, you didn’t figure that Bruce will be on the train for three days but when he gets to St. Louis I can call him and say, “What should I do?” You just didn’t think that way at that time. You didn’t use the telephone that way.

So instead, I went to one of the other well-known enzymologists, Alan Mehler, who was in our department and said, “Alan, what should I do?”

Alan said, “Well, think of something.”

I said, “What do you mean, think of something?”

He said, “Well, do something productive. You’re smart enough to do it. Just do something productive.”
So I thought for a little bit, and I thought, well, it might be worthwhile to study the kinetics of the induction of the enzymes Bruce and I were working on. This goes back to Monod. Like Monod, Bruce and I were looking at several bacterial genes that were closely strung together on the chromosome. In this case the genes controlled the synthesis of the amino acid histidine.

The important thing that Monod had shown was that the linked genes in the galactosidase system were all coordinately controlled, so that if you ran out of glucose in the culture and you put lactose in, the lactose enzymes would all come up. If you ran out of lactose they would go down, the glucose enzymes would come up. Len and I worked on this quite intensively. It was the first instance of concomitant regulatory control of the expression of linked genes on a chromosome.

So in Bruce’s system was there were four enzymes that were known to be linked. They were close to each other on the bacterial chromosome and encoded the sequential synthesis of the enzymes Bruce was working on. Basically, the four necessary enzymes were encoded by linked genes sitting on the chromosome next to each other just like in the Monod system.

My thought was that these histidine enzymes ought to all be coordinately controlled. Therefore, if I had a way of starving the culture of histidine it would force these enzymes to increase to supply the missing histidine. If it was like the Monod system, they’ll all come up together.

I readily figured out a way to starve the culture. We had tools. I used one of them. I hadn’t asked Bruce about doing this beforehand, and I was a
bit worried. Bruce didn’t really like the Monod stuff. It was too—I don’t
know if flamboyant is the right word—it was too McClintock-ish, you
know. It was like finding a new phenomenon, this regulatory thing. It just
wasn’t his style. He did fantastically good science, but this wasn’t his style.

But not knowing what else to do to fill time until Bruce got home, I
did something that was my style. I threw this stuff that’s called formal
histidine into the histidine deficient medium and grew the cells on it. And,
sure enough, all the histidine enzymes came up together.

As far as I know, this was the second example of a bacterial operon
coordinately controlling the synthesis of bacterial enzymes. Had we
published it right when I found it, it would have been an amazing
verification of Monod’s operon hypothesis. However, publication waited
until someone else, Charles Yanofsky here at Stanford, actually did the same
experiment with a different amino acid before Bruce was comfortable
publishing the work I had started. When I came back from his trip to St.
Louis, he listened to the results I got in his absence and then said, “Oh,
that’s nice. Now I’ll redo the synthesis that failed, and let’s get working on
the enzyme that we needed to do.” He had a commitment to complete the
mechanisms for the four histidine synthesis enzymes, and that needed to get
done first.

This is just how it went. Bruce wasn’t interested enough in
establishing the new paradigm, and I didn’t know enough to do so. I didn’t
know what was going on in the field. I wasn’t reading, and I didn’t even
know this is something the field was looking for. However, Bruce did
eventually write a paper describing this and included me as a co-author. This was the correct and honorable thing to do, but it is not always done. Bruce’s integrity stands out loud and clear.

**Kiefer:** And what was the name of the paper again?

**Herzenberg:** [01:57:27] It’s Ames, Garry, and Herzenberg. [*The Genetic Control of the Enzymes of Histidine Biosynthesis in Salmonella typhimurium, 1960*]

**Kiefer:** Didn’t you return to the Sorbonne and obtain a doctorate?

**Herzenberg:** [01:58:32] No, my degree from the Sorbonne was well after I was here at Stanford, and I wasn’t in residence. You didn’t have to be in residence there to get your degree. The degree that I have is a doctorat d’Etat en science. You’ll have the formal name of it [University Paris V (Sorbonne). Doctorate d’Estes – Science. 1979-1981. Equiv. D.Sc.- Immunology.] Basically the French system at the time was that when you finished doing your initial research you would apply for funding to continue doing your research from what was called CNRS [Centre National de la Recherche Scientifique]. If you got this funding you could have your own laboratory and continue to work and publish as a scientist. You didn’t need a PhD to get funding.

At some point you would apply for the doctorat degree, which would *diplome* you to be able to teach in a French university and to have graduate students. It was like applying for either associate or full professor status. So you’ve already had your lab. You’ve already had what we would call the assistant professor’s job here. You’d have your lab. You’re allowed to get funding to do your research, your report. You do all the rest of that.
To qualify for the doctorat degree you had to have a history of really good publications, and you had to be able to speak French. I had the publication history and then some; I had more trouble getting someone to attest that I could speak French. I mean, I could. But you had to prove you spoke French, which I did.

The more important requirement, really, was that you had to present a body of work that a jury would look at and decide was the equivalent of work done by professors in a French university. It was like a tenure review rather than a PhD exam. It was really a very different system. French university systems now have a Ph.D. system where you write your thesis just as we do in the US, based on a few years of graduate work. But at that time in France you wrote a thesis for your doctorat, which required much greater accomplishment.

The thesis that I wrote was fun to write. I wrote it on work that I did, some of it with my own hands, all with my major intellectual participation. I had students who had worked on major parts of it, but that was OK. What they wanted was your body of scientific work, work that led and work that made major scientific contributions.

I wrote the thesis, applied formally for the doctorat degree, was reviewed by outstanding experts in my field and was awarded the degree with commendations. I based this thesis on work that was blocked for some years for funding reasons. I want to go back to this work right now. I’m certainly very glad I wrote the thesis.

[End of May 15, 2014 interview with Leonore Herzenberg]
Kiefer: This is Joyce Kiefer interviewing Leonore Herzenberg at her home on
Stanford campus. We are continuing the interview that we had last week.
Today is May 21, 2014.
We are going to talk about the time when you and Len [Leonard Herzenberg] came to Stanford.

Herzenberg: [00:00:27] We went to the NIH in 1957. He had to spend two years there as
a member of the Public Health Service in order to satisfy military obligation.
The end of that term was September 1959. We had to look for where we
were going next, and Len was spoken to by several people at the NIH who
wanted to keep him as a staff scientist, which was quite a good position to
have. Many of our friends were well situated there. The NIH was a very
good place to be.

Len was offered a suite of laboratories one floor away from where we
were in Building 10, and we had decided that was where we would go. This
would have been fall of 1958 when the decision was made. When he
finished his Public Health Service time in 1959, he would switch over to the
civilian NIH. We were happy about this.
However, it was Thanksgiving and we planned to go up to New York City to have Thanksgiving with our parents. I was three- or four-month pregnant with our second child. We piled the little one into the car and were ready to go. We said, “We’ll just stop off at the NIH to pick up our mail,” before we took off for New York.

I didn’t have any mail to worry about particularly. I was working for Bruce Ames at the time. Len picked up his mail and handed it to me, and we started driving on. There was an airmail envelope there and we didn’t get that many airmail letters.

I pulled it open and there was a letter from Josh Lederberg inviting Len to join his new department here at Stanford. At that time they didn’t do things like requiring one interview and two interviews and all this stuff that goes on now. The head of the department decided he wanted you, recruited you, and if he wanted you he could hire you, and that was that.

We were driving along and this was like a big bomb dropped in the car. [laughs] Oh, my God, this is a great offer but, no, we had committed to the NIH job, although there were no formal papers signed yet. We had a beautiful house that we had bought. We had fixed it up and painted it all and done all the things you do when you buy your first house. It was really lovely. We were happy with our lives.

But it was certainly interesting, so we talked about it back and forth. We had been in Southern California. We had no love of California, because Pasadena is not a place that you really want to go back to, even though Caltech was a fabulous place. If Caltech offered Len a job, we probably
would have gone back there, but there was no great lure in going back to California.

Instead we said, “Well, this is really great. It’s certainly an honor to be invited to join a department that Lederberg is starting.” We had known Lederberg. We knew that Linus Pauling and Lederberg both were stars in the firmament at that time. While we were in Paris, both of them actually had come to visit the labs there, to visit Monod in his lab.

Josh was famous in the Monod lab, actually, for having given a talk in French, rather halting French, but he gave the talk in French. The joke of it was that he wanted to say, “On the one hand, I would do this. On the other hand, I would do that.” There is no such saying in French, but he didn’t know that, and so he said, “sur un main,” and he put his hand up, and he said, “sur l’autre main,” and everybody who was in the room kind of went with their hands the same way in this gesture, and you could see them translating into English knowing what he said, because everybody there was fluent in English. The lab language was actually English, although Jacques was always telling us to speak French. And I think I may have said last time, that he was always saying, “Why are you speaking English?”

But in any event, Lederberg had definitely distinguished himself that way. Len had spoken with him when he was there and, I guess, made a favorable impression. Then when Len was at Harry Eagle’s place, Josh had come by again and had a rather long talk with Len, which Len really enjoyed. They just free-ranged over all the kinds of science things they would have free-ranged over at the time, and Len was very impressed. It
was just wonderful to talk to him, because Josh was just such a fertile mind, incredibly interesting to talk to always, always.

So here was this letter sitting on my lap. We got to New York and saw the parents. At some point, the two sets of parents were together, and Len said, “Oh, you know, we got a very big compliment. We got a job offer in California.”

And Len’s mother’s first statement was, “You’re not going, of course.” [laughs]

To which he said, “No, no, we decided we’re not going to go.”

We’d come also to New York at that time, besides Thanksgiving, because there was a meeting of the Federation of American Scientists. We’d both always been politically involved, and Len actually was running the Federation office, actually we both were running their office in Washington, D.C. at this time. I say “running the office.” I mean we were the young people who picked up the mail and cleaned up the office. The people running it were Hans Bethe and other very well-known physicists. Einstein was probably a part of it. I can’t remember that.

The Federation of American Scientists was a kind of leftist organization. It was peopled by very well-known physicists, including those who would not sign the Loyalty Oath that universities compliant with Senator McCarthy’s wishes were making a requirement for continued appointments. Stanford took a whole physics department from Berkeley at one swoop because none of them would sign the Loyalty Oath. So the
atomic scientists were physicists, were very clearly—they were anti-
McCarthy, anti-regimentation from the government, anti-fascist.

Actually, we had done a serious lot of work in establishing that office. We actually got the organization to recognize that they could get a general counsel, whose name was Daniel Singer, to be the general counsel and to really keep the organization together, because he was based in Washington. Danny Singer was actually Maxine Singer’s husband, and Maxine Singer’s a very well-known scientist who, I think, eventually headed the Carnegie\footnote{Maxine Singer was the president of the Carnegie Institution for Science from 1988 to 2012.}. It was a very sweet little circle that was there, and we were quite happy with being part of it.

Len went to the FAS meeting, and came home very late. We were sleeping in my old bedroom at my house, which meant we were sleeping on a three-quarter bed for the two of us, because there was no room in that bedroom for a full bed. The house my parents lived in had room for their double bed in their bedroom, and that was it. The other bedrooms were my brother’s and mine, and they only had room for singles, with barely a closet. So we were sleeping in this crowded little bed, and it got to be two o’clock in the morning.

Actually, I wasn’t sleeping because Len hadn’t gotten home, and I began to think, “Could something be wrong?” He went to a political meeting. They last late, but here it is two o’clock in the morning and he’s not home yet. Finally, he came home and crawled into the tiny bed and said he was late because he had been talking with Martin Kamen [Manhattan
Project physicist]. They had walked all the way down from Columbia University, which is 121st Street or so, down to the 34th Street subway, and just talked the entire time. He said, “We talked about the job in California.”

And my answer was, “We’re going, aren’t we?”

And he said, “Yes, we really should.” He started explaining to me why Martin had said this was the job of the century and you could not walk past this offer, that Stanford was going to be what it actually became in the years that followed.

Martin Kamen himself is very well known both as a scientist and because he was one of the few people the State Department denied a passport to. This was when Linus Pauling was denied his passport and Martin was denied his. They were leftists, not communists, but were targets of the House Un-American Activities Committee.

Although there were all these nice things and interesting political things to talk about, Len had actually talked his heart out to Martin, and Martin had said, “You’re crazy. You have to go.” So the decision was made, and we decided to come here to Stanford.

Josh was pleased and organized for Len to come out to visit that fall to see the campus and make a final decision, to decide if he wanted to live here, because Len had told him we didn’t think we’d want to live in California. And Josh said, “Well, come out and visit.” But the offer was already made. It was not like, “Come out and we’ll look at you and you look at us.” The offer was made. It would have had to have been retracted, or we would have to say no. We were pretty sure we were going. The object of
Len’s trip was really to make plans for how we would get here. I couldn’t go because I had to stay home with my daughter.

Josh had to cancel Len’s visit on the originally arranged date—I think Len was going to go in November or December. He said, “I have to make a detour, and so I won’t be coming to campus until January or February, and we’ll do this then.” That was OK. We found out afterwards the detour was for Josh to go and collect his Nobel Prize in Stockholm. He couldn’t say, because it was beforehand.

So there we were. Len went out to visit right around my birthday (mid-February). It was snowing, it was horrible in Washington, and Len came out to California and took pictures. The grass was green and everything was beautiful. Even just for the weather, we were happy to be able to go. But by that time, we had realized who was out here. The Kornberg department was already here. Henry Kaplan [radiologist] entertained Len. At that time, Henry Kaplan was still living in Marin, because the school hadn’t moved down yet.

**Kiefer:** So the Stanford med school was still in San Francisco?

**Herzenberg:** [00:11:49] Well, part of it was always here. The preclinical was here, so Sid Raffel was here and the basic sciences were here, and [Arthur] Kornberg had already arrived with his department, and I think that the building was open enough for Kornberg to be in it by the time he arrived, which was shortly before us, but there were parts of the building that still weren’t open.

**Kiefer:** Kornberg was the head of genetics?
Herzenberg: [00:12:20] No, Josh [Lederberg] was the head of genetics and Kornberg was the head of biochemistry. Kornberg came with an intact department that he had in St. Louis, and then he added people to that, but his primary department came from St. Louis.

Kiefer: From Washington University?

Herzenberg: [00:12:39] Right. If you remember, I told you a story about how Bruce Ames had gone out to St. Louis and that he’d gone out to the Kornberg department. This was a Caltech and Paris axis because people in Kornberg’s department, Dave Hogness and Dale Kaiser, both came from Caltech, from the same department we came from. Both had also preceded us in Pasteur. By the time we got to Paris, we were the third Caltech invasion. We even took Dale’s apartment in Paris.

Kornberg had also brought Mel Cohn with him from St. Louis. Mel was Jacques’ closest colleague in many ways, certainly closest American colleague. Mel was a kind of immunologist, but also a molecular biologist. He was one of the real attractions for us coming here because he was very creative, is still a very creative guy, and very interesting. We realized this when we first met him when he visited Monod and the laboratory in Paris.

Herzenberg: [00:13:57] So the Kornberg department was one of the key attractions here. We were pleased that there was a closeness in the two departments. Arthur helped get Josh appointed and the Department of Genetics established. It was just a very exciting place to come to. There was no question but that Len would take the job and we would come here.
Kiefer: So then just to place you in the proper place, you were part of the med
school and then biochemistry and then the Department of Genetics. Is that
the order of things?

Herzenberg: [00:14:45] Well, biochemistry and genetics were equivalent departments;
both sat on the dean’s whatever committee. They were just separate
departments within the medical school. Josh had a very different concept of
department than Arthur had. Arthur’s was biochemistry, E. coli, and what
we would now call molecular biology, but at that time was DNA. So they
were very much into genes and chromosomes, and Josh was into very
different things. Josh had a very, very broad and very fertile mind that he
dealt in-depth in anything that he did. It was not that he was superficial, but
that he was voracious for new things. And where Arthur really saw that we
had to really understand DNA and genes and how this whole thing was put
together and how the DNA was synthesized and what controlled all of that,
the biochemistry of genetics, if you like. Josh was even at that time probably
interested in epigenetics as well as genetics, meaning the ways in which
genes were integrated with one another, and much else.

Josh was a scientist, as opposed to a biochemist with a specific
mission. Josh was interested in all of science. To be in his department was
either incredibly awful if you wanted to have your nose to one grindstone,
because there was so much going on, or wonderful if you were like Len and
were interested in whatever made things work, how genes were expressed
and interacted in mammals and other complex organisms. It’s a very
interesting question. I don’t know how to describe this.
Kiefer: But you and Len felt that this was the atmosphere that you wanted to work in.

Herzenberg: [00:17:13] We were happy with the genetics department. Len had enough eclectic interests, if you like, that we were happy with the direction we were going. Len wanted to do mammalian cell genetics. He wanted to do the genetics of mammals, but not coat color or other genetically controlled traits. Rather than simply working out any single mechanism, Len wanted to understand the whole interactive genome from a genetic standpoint.

Len wanted to do gene transfers between and into human cells. Much of what has been done over the last fifty or sixty years was stuff that he wanted to do and envisioned being done. To do this, he was much better placed in Josh’s genetics department than he would have been in the Kornberg department.

Actually, Arthur liked Len a lot, and he gave him fatherly advice, which was, “Why are you wasting your time with mammalian cells? You should be working on E. coli because there are really important things to be learned.”

But Len said, “May I respectfully disagree? I know that my tools are nowhere near as fixed and I don’t have as many colleagues that I can bounce things off, but I really want to focus on mammalian cell genetics.”

Arthur just did not agree. Now, many years later, Arthur came back and said, “You know, that was good advice I gave you, but it’s just as well you didn’t take it.”

Kiefer: I love it. [laughs]
Herzenberg: [00:18:57] That was the gist of the conversation.

Kiefer: Now, how were you working with Len at this time?

Herzenberg: [00:19:02] We drove out to California together. We left the kids (our second daughter was born that spring) with my mother, put the dog in the car and drove our little Volkswagen across country.

When we first arrived, we stayed with Gladys and Lew Aronow. Lew was in pharmacology. They were very kind to offer us (and the dog) a bedroom until we could find a place. I still see Bruce, their son, who is now a molecular geneticist.

Anyway, we stayed with the Aronow family for a couple of weeks down in Sunnyvale area, which was all orchards. From there, you had to drive here to Stanford by what became 101, but was just at that time a typical two-lane-with-shoulders road, no houses, and just lovely orchards alongside. The Aronow house was in a tract that had been built in the middle of an orchard, so you kind of drove down all these orchard lanes. It was very sweet that they invited us to stay with them, but we very rapidly found a house that we could rent.

Herzenberg: [00:20:20] We rented a house in Menlo Park and arranged for my mother to bring the kids out. I have some cute pictures of the kids sitting on the lawn of the house at that time. It must have been September or October. And life began.

When we got here, Josh had a fairly chaotic office and there was no place for the department yet. That part of the school had not opened up. So biochemistry was where it was, and they gave up some offices. I guess Josh
was in the biochemistry wing, and we were put out in a physics building that was a kind of temporary building. It was like a two-story wooden structure that was meant for physics offices, and they kind of created a lab out of that.

It was just chaotic. I mean, everything was crazy. Josh, of course, had recently won the Nobel Prize. He had all this communication that was coming in and had no time to organize to get things done for the department. End result, although we had sent lists of things that needed to be ordered months before so we could set up the lab when we arrived, nobody had written up the orders, no one had typed out the purchase orders for this stuff, nothing was ready.

But we had cultures coming from the NIH. We had to have an incubator, we had to have glassware and media to feed the culture. We were floored. Len and I weren’t planning on my coming to work for Len, but I said, “Well, I’ll write the purchase orders and get all these things together just to help you get things started.” That’s how I got started working with Len again.

Once everything got organized, I just continued on. Len had to hire a technician, but I had to help train the technician to feed the animal cells. You know, we just were doing it together, “I’ll do this, you do that,” and just trying to get things together in this new environment and set the family up. By that time my mother had brought the kids out.

So I wound up working for Len part-time until it became clear that I really didn’t want to go look for a job for someone else, and he didn’t want
me to. We just decided to figure out how I could just work with him. So he said, “There’s something that I really want to do and I can’t spend the time to do it, so if you’d take that part of the project over, that would be a good thing. You’d be working more or less on your own, but with me.

I said, “What do you want to do?”

“Well, we need to make antisera,” he answered, “and to make antisera, you have to…” Essentially, he had gone to see George Snell, who later got a Nobel Prize for the MHC [major histocompatibility complex]. It was Len’s idea that I should make antibodies to the MHC, which everybody knew you could, and then we could use those antibodies to select cells in culture. Len realized that the MHC provided natural genetic markers normally expressed on cells. With antibodies to these markers, you could do genetics with them, because you could select for loss mutants, you could select for gain [gain of function mutations] mutants, you could do all these things with a natural antigen rather something that you artificially put into, or onto, the cells.

So Len asked me to make the antibodies that we would need in order to do this. George Snell said that this was a good idea and he offered a little bit of antisera, but not enough to be able to do a project. So we had to just immunize and make our own antisera, and that led me to our next-door neighbor over in the temporary building—Gus Nossal, who later became Sir Gustav Nossal, world famous head of the Hall Institute in Melbourne. There was me, “Gus, could you teach me how to immunize animals?” I can still remember Gus sitting there and showing me how to bleed a mouse,
how to take out a spleen, and all the things that you need to learn to do immunizations against animal cells. Amazingly, Gus just patiently gave me lessons. In about a week, maybe five or six days, I came back to Gus with a plan, and then I was off and running. I had my own project in Len’s (now becoming our) lab.

Next, I had to set up an assay to test the antibodies to know if I’d gotten antibodies, and so I did that. That required interacting with somebody who was working at Bar Harbor, Jack Stimpfling, who was working with George Snell, and he showed us. He had a new assay for measuring these kinds of antibodies. It required using something called PVP [polyvinylpyrrolidone]. We would use that to dilute the cells in and then look to see if the cells agglutinated, which would mean there were antibodies present.

Once I got the assay working, I made the antibodies Len needed so he could do genetic selections. It all worked. Len and Howard Cann, Len’s first fellow and later a leading human geneticist in Paris, did the selections and published the first papers doing something like this.

**Kiefer:** So as you worked with Len, he didn’t need to hire an assistant. You were more than doing the job.

**Herzenberg:** [00:25:56] No, no, he did hire a technician who worked with him on the somatic cell genetics. He had cell cultures that he was growing and selecting drug-resistance mutants, and he was doing a lot of classical what would be bacterial genetics but doing it with the mammalian cells. So he had gotten that started at the NIH, and he brought it forward with him here.
But I took this other part of the project, which was to make these antibodies to mouse cell surface antigens. I could help Len with the cell culture, and I did. However, he had that under control and never really wanted me to be in that part of things. That was his thing that he was enjoying doing, and he didn’t really need the help. Besides, he was more interested in me opening up new areas and me doing what I wanted to do.

Making the antibodies for Len wound up opening a new project that ultimately resulted in my first first-author paper. What happened was that, strangely, the non-immunized pre-immunization mouse serum I was using as a negative control turned out to show a very positive response. I then realized that the mice I was immunizing were retired breeders and that they had been bred to a male in a different strain, which had the antigen I was testing for. When I tested virgin mothers, they showed cleanly negative responses. Therefore, the outbred mothers that I got the pre-immune sera from were becoming immunized through the pregnancy. This was then a mouse model for Rh, which was known at the time, that the mother became immunized through the father’s Rh antigens.

So I started working on this immunization mechanism, on how all this was happening. That led me into working on maternal immunization, just understanding maternal fetal and uterus and all the kinds of things that go with that, which at the time is seemed very appropriate for a woman to work on, which was funny. [laughs]

There was a very, very nice guy in OB/GYN named Bob Goodlin, and he was interested in this kind of stuff, and so I wound up working half
for him and half for Len, doing much my own stuff on this maternal fetal thing. At this point, Len noticed some papers indicating that there was a maternal fetal immunoglobulin story going on in immunization, and he suggested I look into that. It was an immune suppression story that he’d noticed. He picked it up in a paper, and said, “Here, did you look at this?”

I said, “Oh, I don’t know,” although ultimately it became something that I really got interested in.

I stayed working with Len, and we worked very closely together. Mel Cohn left Stanford after a couple of years, and Len took in one of Mel’s students, a wonderful guy named John Wunderlich, to do a different kind of immunogenetics project. I started watching what they were doing. They were making antibodies to an immunoglobulin molecule, and I started working on that.

I had very good eyes at that time, and we had these little agar plates, in which you could see little lines where antibodies and antigens met up. I recognized that in several animals, immunization was stimulating antibody production to antigens on two distinct immunoglobulins, not just one. Two different proteins were being recognized. And from that came the recognition that the genes that encoded these kinds of immunoglobulins were very closely linked on a chromosome, and this became the immunoglobulin chromosome that I’m still working on. [laughs]

Kiefer: Wonderful.

Herzenberg: [00:29:51] It’s kind of fun, but, yes, it’s turned out to be a very interesting—we more or less predicted what it turned out to be, but, of course, it’s much
more interesting now than it was back then. And I'm still working on it, as I say.

**Kiefer:** As your lab developed, you had graduate students, post grads? What was your student component?

**Herzenberg:** Let’s see. Well, we were able to move from the temporary building a few months after we set up the laboratory. That temporary building was a disaster. When it got up to 110 degrees outside, which it did one day, the cultures were in an incubator that was supposed to be maintained at 98.6—37 degrees—so we were putting ice in the incubator to keep the cultures down to temperature. It was not possible to work there. So they got us over into the main building as rapidly as possible, and with that came all sorts of colleagues, and the whole environment changed.

We moved in. Gus moved over with us, and so Gus and Olli Makela were in the lab next door. Josh kept inviting immunologists to work in the department and introduce several other people interested in immunology. One of them he put downstairs near us, a guy named Avrion Mitchison—these were the top names in immunology at that time, and we were all there.

George Klein was here, Av Mitchison, Gus, and all these made up a new view. It was more of a genetic view, because we knew Gus proved that one cell makes one antibody. But we still didn’t know how that cell was stimulated or how it made the antibodies. Josh’s concept was that immunization selected for those cells that made the right antibody, one that reacted with the immunizing antigen. That’s true. He was right about that, and that was his basic tenet. Josh didn’t mind what the details would be; that
was the concept of how it was working. We didn’t know about T-cells at that time. We only knew about B-cells.

We were all living in this basement corridor, and the animal facility was put across the hall from us and I ran the animal facility. We had an animal caretaker, but by that time I learned all about breeding mice. We bred new strains of mice and did all kinds of things. So I became the chief animal caretaker. I was in charge of that.

We had Gus’ mice in there and we had our mice in there, and Av kept his mice—Av was so funny. [laughs] He liked to grow his mice in battery jars rather than cages. They were these round jars. It was his belief that the mice were happiest when they were piled way up on top of one another. He’s probably correct. He said, “This is how they live in the wild. They’re really happy to get a hundred mice into a small space. They keep each other warm, they feed each other, and this is how they’re happiest.”

But our veterinarians have different ideas of this. They are really wrong. The way that animals are kept right now is not conducive to learning what we need to learn from the animals. It’s conducive to doing surgery in the animals if you don’t want them to get infected, but if you really want to learn about the immune system, you can’t put them in a single cage by themselves. You learn something, for sure, but it’s not what you should be learning. But anyway, that’s another whole area of how the world has evolved. I mean, we learn other things much better because we do this. It’s not as if everything is downhill, but there are things that are missing.
Anyway, Av he did keep them in there, but he had an incubator he worked in. We had to bleed mice. To take blood from them, you take it from the tail and you nick the tail. You want the mouse to be warm, because when he’s cold, his tail constricts, so you don’t get that much bleeding. So you usually put them in a warm environment, usually an incubator that’s, say, at like an 85-degree or 90-degree room, and you nick the tail. The thing about this was that Av would bleed his mice. When you’d come in there, he had the incubator open where he was bleeding the mice, so the mice were crawling all over Av and up his arm. [laughs] They’re lovely. Mice are very nice. Captive mice are quite lovely pets, and Av just loved them. He’d put them all in the jar, but then he didn’t put the lid on, so the mice would jump out. [laughs] It was very funny. But we kept ours in the animal room because we had cultures going in the room, and you can’t get the cultures dirty with the mice.

**Kiefer:** Did you have some doctoral students and postdocs working with you?

**Herzenberg:** [00:35:19] At that time, Howard Cann met us before we came to came here. He met us in Washington and came and asked if he could be a postdoc in Len’s lab. He later went from our lab into pediatrics, then was here for a long time, and ultimately moved to France and ran the France Genome Center, Dausset’s—Dausset was the Nobel Prize in France, and Howard was the person who ran the center under Dausset. So he was medical.

Then we had a postdoc who was a classic postdoc, this funny guy, a little bit strange. [laughs] He was so funny. Somebody spilled a cup of coffee, and Ben came into the lab as we were wiping it up. He said, “You
didn’t have to do that. I would have done it for you,” because he was always spilling his coffee all over the place. [laughs]

There were three students, three medical students. This was the first year of the medical school down here and the second year. The first year was the student that Mel Cohn passed over to us. He was in the very first year. His name was John Wunderlich, and he’s now at the NIH. John came in, and he’s the one who started doing the immunoglobulin genetics. We had Harvey Ozer and Robert Erickson, who were both second year. They entered in the second year of the medical school, and both of them went on to be professors one place or another. They shared a cadaver, I believe, with Irv Weissman and Herb Kaiser. Irv is now the head of the Stanford Stem Cell Center. He was in the lab for a while, but he wanted to work on some stuff that Len didn’t want to work on, and so he moved to Henry Kaplan’s lab, where they had funding to do that kind of project.

There were four medical students in that group, two actually were working in the lab, but were all part of the group. Then Halsted Holman was the head of medicine, and he had two medical fellows. They hung out at the lab—Bob Mishell and Henry Wortis—and they were very much driving forces. So there was this whole gang of people.

There were no women at that time. Helen, Olli’s wife, worked with Josh. She was a bacterial geneticist, and they were quite close friends. They moved over to Menlo Park, where we were living. They took the house across the street, and we had a great thing going. The grownups, us and Olli and Helen, would hang out at our house. Our cousin, a younger guy who
was living with us, and their babysitter got on quite well together. So they stayed at the other house. We’d send the kids over there to play and we’d do what we’d do. [laughs] It was really funny, but it was a nice little commune.

It was just essentially a house that stretched across the street.

**Kiefer:** So you were really the only woman there in the lab?

**Herzenberg:** [00:38:49] In our labs there was a woman who was a technician—Joyce—but there were no women scientists at that time.

**Kiefer:** Was there any difference in attitude towards you versus towards others, or did they accept you because of Len?

**Herzenberg:** [00:39:11] Well, remember that I had no bachelor’s degree, I had nothing, so I didn’t have a right to standing. I had to earn my right to standing. We had a regular seminar that met every Tuesday night, and at that seminar I spoke up when I had something to say, which was not infrequently. I was doing all the reading that everyone else was doing, and I “put my two cents in” whenever I had two cents. At one point, Len worried about this a little bit. He asked Av, “Do you mind if Lee is doing this?” Because he didn’t want other people to feel that he was imposing me on them.

Av said, “No, no, no. Just let her say what she wants to. She’s very bright and she says interesting things. Don’t stop her. She’s contributing.” It was a very clear message, and Len was quite happy with that message. So from that time on, I was as much a part of that group as anyone.

So now I’m in a group in a way that was something like how I was with my father’s Boy Scout troop, where I’m the only woman talking up in this group, but it doesn’t matter to me because I know all the people and
they all love me. And even if they didn't, the structure of the group was set up so that it was OK for me to be there as well as everyone else.

That was a major thing. As I said, Len did feel constrained at the time to ask Av about my presence. Av was more senior by a few years. Gus and Olli were the same age as we were. But they all treated me as welcome. As a scientist, I was junior to them, and I recognized that, but that didn't mean that I couldn't ask graduate student kind of questions. So that was kind of like my graduate years, if you like, those first few years, and my professors were Av and Olli and Gus and Len. We had very, very incredibly interactive discussions about papers, taking the papers apart, what's wrong with them, what's right with them, what's been found, what do we accept, et cetera.

Let's move for a minute to the overall department structure, and how I was accepted there. Josh's wife had been his collaborator in the bacterial genetics studies for which he won the Nobel Prize. She was, depending on one's point of view, the person who taught Josh everything he knew about bacterial genetics, or a royal pain in the butt. [laughs] I mean, she was a very difficult woman, perhaps not unreasonably, because she was not appreciated and wanted at least part of the appreciation Josh got. But nobody was Josh's equivalent. No one in this world was ever Josh's equivalent. I think that was very frustrating to her. I think it was a complicated marriage and it eventually broke up. Esther was just this very difficult person that we all had difficulty with. It wasn't that she was nasty or anything—she would talk interminably about stuff, often not to the point that you were interested in. That's my recollection of her.
I could never get close to her. She was not somebody who would say, “Here’s another woman. I’ll—.” You know. Not even to be helpful to her, but she just wasn’t that kind of person. This was fine with me, because my way of being was I don’t care if you’re a man or a woman, but I’m used to being with the men and I don’t make a big difference out of it. But there weren’t that many women around. It was not just me. I mean, everybody had difficulty with her.

There was another woman who was a graduate student, Ann Ganesan, who was upstairs in Josh’s unit. She was working on something very far away from my interest. I knew a lot about bacterial genetics, but somehow we didn’t get together intellectually with her and the guy, a graduate student, who became her husband. We were good friends, we did social things together, but scientifically we didn’t have a connection. They were interested in bacterial genetics, and I had done bacterial genetics, but it wasn’t what I was really interested in. I was working on all these crazy mammalian things at that time.

Eva and George Klein came, and I did really relate quite well to Eva, but they weren’t there that long. They’re famous Swedish immunologists. Some complicated things happened and they left early, which I was quite sad about it.

Throughout life, I was always good friends with Eva. Once I gave a talk at a meeting, and she turned up and gave me a lovely necklace, she said “because the talk was so good”. A very feminine thing to do, and one with a great deal of meaning.
Eva was, in her own right, a very famous scientist, and she and
George were good partners. They were, in a certain sense, a kind of model
that Len and I knew you could be like this, and so that was great. We were
quite close, actually, through them, with the Swedish group. There were
several other people in that Swedish group. We went to Sweden. They came
to Palo Alto.

I think those were the women in my early world. Eva was clearly a
model, as was Esther Lederberg. I mean, these are women who held their
own in a science intellectual world, so I wasn’t alone. But in the area that I
was working, I was the only woman, but it didn’t matter. It wasn’t a big deal.

Kiefer: Now, didn’t your work evolve to or become involved with—FACS, I think
are the initials for it?

Len is the inventor of the FACS, the Fluorescence-Activated Cell
Sorter, and his invention was not the instrumentation that made it work. In
fact, someone else built a part of that machine before he ever got to it, and
they gave him the plans for that, and that’s very well known. But the reason
that he’s the inventor of the FACS is that he recognized that you needed to
put fluorescence detection on this and you needed to put size detection on
it in order to make the machine usable. Those are the things that make what
the FACS is the FACS, as opposed to some of the counters and other kinds
of machines that were around at the time.

I worked closely with him on this. He guided the building of the
FACS, he was in there with the engineers. What happened was that Av’s lab
was then cleaned out and was replaced. Lederberg now got interested in
exobiology. He’d finished with immunology, and he now got interested in exobiology and was involved in the first Mars shot. So he set up an exobiology laboratory in which he had a bunch of engineers, and they were physically right next door to us, and so we all played ping-pong together and we hung out together.

Josh had some other computer science people upstairs, who started coming downstairs and commandeered part of the ping-pong room. The ping-pong room that we used was renovated and now part of the current library. At the time, it was just an empty space waiting to become library. It was just a room with pillars and cement floor, a low ceiling and a couple of windows. It was just across the hall from our corridor, so we commandeered it and said, “OK, this will be our room to have lunch in and play ping-pong in.” We just used it as a recreation room, basically.

When Lederberg saw that, he put the computer group into a corner of that space. Once the computer group was in there, they joined our ping pong and lunch group and became close friends. So I learned to program, basically because I could learn and my friends were willing to teach me.

I learned to program on what I think is the earliest minicomputer, which Josh got from the MIT Lincoln Labs. If you say this and move your fingers like this, the people who did programming in the old days know what you mean. It’s when you’re moving your fingers, the two fingers together, you’re hitting the switches because there are ones and zeros. Up is one, and zero is down, and you get a bank of sixteen of those, and you set those into a certain pattern, and that’s an instruction. Then you push the
button and then you set them into another pattern, you push the button. That’s how you program the computer. You programmed it by visually setting the lights that meant that those were the addresses that you were moving to and those were the commands. You can say “jump to” or do things. It was a whole—I mean, understanding a computer at that level was—that was called—it was the Lincoln Labs was the first mini computer. It became Digital Equipment Corporation eventually.

I learned programming at that time when you could do this when there wasn’t even memory on the computer. You had tape. That was all the memory. [laughs] It was amazing what was not on that little computer. But nonetheless, there were engineers who were working it, and they were willing to teach me if I was willing to learn. My love of computers stems from that. Then they said I should go take the computer programming class, which was for the bigger computer. It was before Pascal. I went and took a real course in computer programming and learned how to make the big computer say hello or something like that.

At that time I had actually a woman friend, Henry Wortis’ wife. Henry was one of the people who were working in our lab. His formal lab was in medicine. He was a medical fellow, but his wife was doing psychology, and she and I would talk a lot. She was monitoring birds feeding, mothers feeding babies, and you had to keep pushing a button every time the mother did this, and you pushed a different time when the mother did that, when the baby did this, you did that, and she wound up
with these things, and she’d sit there and score them. And I said, “Why
don’t you use the computer for that?”

And she said, “Oh, that’s an interesting thing to do.”

**Herzenberg:** [00:50:38] As far as I understand, when Shelley went back and did her
dissertation, everybody at Rutgers was excited about this, because they had
not seen computers used to do this kind of thing, and this was very
innovative.

**Kiefer:** About the bird-feeding study.

**Herzenberg:** [00:50:52] Yes, yes, essentially. It was something that normally you wouldn't
even think about doing without a computer now, I mean, but at that time
she did it all by hand on paper tapes. So there were all sorts of things. It was
a very exciting—and her husband, Henry, was doing beautiful science.

**Kiefer:** And Henry’s last name was?

**Herzenberg:** [00:51:09] Wortis. He’s a funny guy. His high school girlfriend was Mary
Travers, from Peter, Paul, and Mary.

**Kiefer:** Really?

**Herzenberg:** [00:51:22] So he had introduced me to Mary. She and I have maintained a
very distant but nonetheless—when we’d see each other, I’d come back
stage every so often. She'd say, “Oh, how are you? And how’s Henry and
how’s everything?”

**Kiefer:** Oh, that’s wonderful.

**Herzenberg:** [00:51:36] She was very sweet. She was a lovely woman, absolutely lovely
woman.

**Kiefer:** She’s passed away, right?
**Herzenberg:** [00:51:41] She has, yes.

**Kiefer:** It sounds like there was a lot of interaction not just with geneticists, but with engineers and computer people.

**Herzenberg:** [00:51:52] Exactly, but this was Josh, OK. This was Josh’s department. He would bring these people in, and it was a small department, so we would get to know them if we wanted to. They were right next door at the lab sitting next to us. This is the value of mixing up departments. The mistake we have is that we make all these departments contiguous. It’s useful from an administrative point of view and you can draw the map, but the value of that cross-feeding was really important.

But Josh then went from the Mars shot and that technology, he went to computer technology, and he then had there in his group a guy named Tom Rindfleisch, who ultimately became head of the Stanford Libraries. I wrote a history of Josh and computing, as we remembered it, Tom and I, of the department and the kind of ferments that were there.

One of the guys who was in that department at the time was Larry Tesler, and I guess somebody needs to debrief Larry about the history back there. It really is important. Larry is the person who told Steve Jobs how to do windowing.

**Kiefer:** Really?

**Herzenberg:** [00:53:11] There’s a great *New Yorker* article about Steve Jobs, and there’s this whole section in there about how Larry told him about doing windowing. Well, standing behind Larry as he was telling Jobs, figuratively but maybe literally, was the guy who’s worked for us all these years, a guy
named Wayne Moore, who then came back and put up windowing for us as
the first. The first windowing I ever saw was on our computer. I'll come to
Wayne. I mean, I'm jumping way ahead, and I shouldn't, but those are the
roots that Josh planted this engineering group next to us. I was as
interested—my father was an electrician; my brother was interested, he was
a psychologist and headed up a Department of Psychology, but it was the
first computing department. He was always interested in computing, and my
brother and I always, always talked a lot and were always good friends.

Kiefer: So back to the FACS. I understand there's a patent connected with that.
Was it connected with hybridoma technology?

Herzenberg: [00:54:45] That's more than ten years' hence. So the FACS begins with
Josh's lab next door and with Henry Kaplan. Len's idea was to be able to
sort cells because there was no way of studying them and doing genetics
with them unless you could isolate a cell that was by itself. His idea of
making these antibodies to cell surface antigens—that all came into the fact
that we wanted to be able to identify which cells expressed those genetic
markers and then do genetics with them.

Len saw the FACS as a way of sorting cells to be able to do somatic
cell genetics and also to characterize the cells in lymphoid organs, because
by this time I'd gotten involved in immunology. Nothing that I was
involved in was he not involved in, and nothing that he was involved in was
I not involved in.

So Len eventually got together with Kaplan, with Lederberg's
blessing, and Kaplan put up 15k and Len had 15k, and they said, “OK,
we’re going to build a FACS.” And the joke is, OK, a million-five later, they had an instrument. But they did build the prototype.

I was always like a cheerleader in this thing, and very much Len’s consultant about what the machine should do to be useful. But I was never involved in the engineering meetings. Len had the engineering meetings himself with people, but he would come back and talk to me about it, and then we would discuss what could be done with the engineering meeting. So I was part of it but not involved in the direct engineering meetings. This probably wasn’t because I was a woman, or not mainly because of that. It’s just that I was busy being a biologist and running my side of the lab. For Len to be able to do all the engineering meetings that he spent time in, he needed me to keep all the physical stuff in the lab going.

The other thing is that Len was not an engineer at all, so when he went to those engineering meetings, he’d come home and talk to me, and if it was mathematic stuff, he would often bring it back to me, because between us, I was the one who did basically arithmetic. It was mathematics, but it was mathematics at a very, very simple level. I never became a mathematician as people would call a real mathematician, nor a real engineer as engineering did, but in terms of understanding the thrust of what was there, that’s what we had.

Then eventually we moved the lab upstairs. The department got bigger. They added the big mainframe that Josh put in that was right next to the department. It kind of lived in the breezeway between the two buildings once that building was built. I was always involved. I was helping to
troubleshoot that and debug it because I just liked the stuff. It was something that my father and brother taught me to do and it was my thing.

Len never had anything to do with computer programming. He never wanted anything to do with it. [laughs] But he didn’t mind my doing it. He liked me doing it except when it got in the way of him wanting—why isn’t something else done that needed to get done? [laughs] But computer programming was the ultimate crossword puzzle, so you just tend to love it if you love that kind of stuff. Anyway, that’s pretty well what the environment was like downstairs.

We all lived over in Menlo Park near Perry Lane. Walter Bodmer came. After Gus left, Walter took over Gus’ house, and he came with Julia. Julia at that time was not really speaking up very much. She was more of an Englishwoman, and she really didn’t speak up much as a scientist, but she probably was a good scientist at that time. I didn’t realize how much so. She was definitely contributory intellect, a good one, but it was in areas of population genetics, which is stuff that was not my interest, again.

**Kiefer:** I understand there was a patent involved that was and still is one of the most lucrative patents that Stanford has.

**Herzenberg:** [00:59:30] Well, actually several patents came through, and I can’t remember. I guess I should ask and become enlightened on this. I don’t really remember.

We moved upstairs. Len was moving a prototype of this cell sorter, which by this time had gotten to be a functional prototype, and we were beginning to actually look at cells through it when a guy who was a high-
level manager, engineering manager from Becton, Dickinson came by with his nephew. Couldn’t have been his nephew, but anyway, some guy who was like his nephew. He came because he was asking—should the company make antibodies to things that could be looked at? Mainly we were using antibodies to kill cells at that time. It was called complement dependent killing.

Bernie came by asking something about making antibodies and how would they make antibodies, and this guy was interested in how you would make antibodies. He’d heard that Len knew immunology and knew about antibodies, which actually was my thing, not so much Len’s.

So Len called me into the office with them. But then Len turned around and said, “Well, you know, if you really want to make a difference and you really want to make a lot of money and build a company, the thing you should do is build a commercial version of this cell sorter that I’m building.”

Bernie was a very smart guy, and he listened. So he said, “Oh, tell me about it,” and Len told him what he was doing. Bernie understood that this was a good thing to do. Bernie was also the kind of businessman who never invested money if he could help it. He was wonderfully tightfisted. It was good.

This was a little before Nixon’s War on Cancer got going, and Len was asked to sit on a committee that would review grant applications for the War on Cancer. Len turned around and he said, “No, I don’t want to do that, because I want to apply to that committee for funds.” And he did. He
applied for funds to build two cell sorters, which he said would do for
cancer more than anything else they could think of. Len was absolutely
right. It was an incredible vision, because you would not know how to treat
cancer if it was not for the FACS.

OK. That’s what the FACS does. When you hear “stem cells,” when
you hear “leukemia treatments,” when you hear “HIV,” where you hear any
of these things, these are medical uses of the FACS, and without them, we
would not even recognize that HIV resulted in loss of T-cells [laughs]

There was not a patent on the original FACS machine. That I know.
But I’ve heard that there was some patent that was issued for some silly part
of the FACS machine, and Becton Dickinson licensed that patent. It was
kind of a corner part, not important. I don’t really know. There was a patent
at that time, but more important was that Stanford actually kind of licensed
the FACS machine to Becton, Dickinson.

Now, this was prior to what’s called the Cohen-Boyer patent, and the
Cohen-Boyer patent was the beginning of licensing biotech, really. Before
that, it was not a big business. Cohen-Boyer was the first molecular biology
patent. But the FACS was already lucrative to Stanford before it was very
much of a patent position, because Bernie understood he should be
supporting Stanford in the development of this project. As much as he was
tightfisted, he was smart, and so he did provide support when it was needed.

That history is in the Office of Technology Licensing and ought to
be written out somewhere. I don’t think it has been. It was pretty loose,
because until the patent lawyers got into this, it was a much more personal
arrangement between Niels Reimers, who was the head of the office, and Bernie. And I don’t even know that Len knew what all the arrangements were. What we knew was that we were building the instrument collaboratively for this grant that we had. And then Bernie asked Len to travel around with him and to explain to people what the instrument did, and he did that, and that created the groundwork for this whole instrument and the market it serves.

The FACS instrument then grew, and more and more of them turned up. Becton, Dickinson built individuals, so it was kind of a custom industry in a way, until—this would be 1976 when we went on sabbatical to César Milstein’s science lab. César Milstein was the person who got the Nobel Prize for inventing monoclonal antibodies.

Kiefer: What years were those?


Kiefer: And his lab was where?

Herzenberg: [01:06:19] In Cambridge. Now, that’s an interesting women’s story. By this time, I’m well established. I have students, I’m well respected in our lab, and I’m publishing my own papers. Len and I run the lab together, and I’m accustomed to doing science and giving talks at meetings and doing the rest of this.

Len, during this time leading up to the sabbatical—this is early 1970s—chaired maybe a third to a half of all the immunology meetings that were held. It was a small field, and he was either chair or on the committee. He had by that time two fellows, Elizabeth Simpson and Patricia Jones.
Patricia’s a Professor here and has done important work in the provost’s office. She’s been here almost all her professional life. She’s been away, but she’s basically been here. Len’s statement was very simply that if he spends the time on a postdoctoral student in his laboratory, he doesn’t expect her to have less opportunity than him, and therefore he was either not going to train any women or he was going to see to it that the women got good places on symposia and had adequate careers equal to the careers they could have had as men, and pretty much he did this. He made sure that the positions were open for women to speak on these programs.

There were not very many women, but there was me, there was Pat, there was Liz. There was Marian Koshland who was married to the guy who eventually became editor of Science. She was a Berkeley professor and very strong in her own right. There may have been one or two other women who were that advanced, and then there were younger women who were coming up. Linda whatever her name is—she just got an award from San Diego—and some of the Swedish women, Erna Mueller, Eva Klein, et cetera. There were enough women that we could have women on all the programs, and Len just saw to it that we did. He said, “I don’t want to hear that there’s a man who can do that job better than Erna can. There isn’t. And Erna is very capable of giving this talk.”

So he built a space for women in immunology, so much so that when one year when we arrived for a meeting at Asilomar, and the microbiology meetings had been held just before us and were leaving our meeting was
coming in, this guy came out and said, “How come you guys have all these
women at these meetings? We don’t have any women like that.” [laughs]

**Kiefer:** Wonderful.

**Herzenberg:** [01:09:31] So, yes, Len did this. Liz (Elizabeth Simpson?) became inheritor
to Peter Medawar in England. She is the grand dame of immunology in
England, and there were a couple of other women there. And Av was a very
strong supporter—Av Mitchison, the guy we mentioned earlier at Stanford,
was back in England. He was a very strong supporter of women in his lab.
So if the women got there, they found places. It was hard. Women didn’t
know that they could come to these labs and have positions and would be
respected.

So I got to running much of the lab at Stanford. I decided what was
going to happen with this or with that, and I was just used to doing this. If I
said something was going to be this way, if Len wanted to argue with me, he
would --- and did, and we came to agreement. Mostly I didn’t feel I had to
ask him permission for anything and he didn’t have to ask me for
permission for anything. We were just running the lab together.

We get to England and César Milstein said, well, there wouldn’t be
place for me at Mill Hill, but he would make room for me at a different lab
about 20 minutes away [ARC Institute of Animal Physiology, Babraham,
Cambridge ], outside Mill Hill. It was a farm lab, basically. That’s where they
did the work on big animals. And he organized for me to be there. He said
his wife was at that place and that I could have a lab of my own there.
Well, it turned out, the difference was whether you were at Mill Hill, which was where real science was being done, or at that place. There was one real scientist there whom I really still adore, but otherwise there was no real science at Babraham. The guy Cesar sent me to work with was actually a fairly brilliant guy named Arnold Feinstein, but had no use for women. He was—excuse the expression—a pig. I mean his clothing was always hanging out—really a slob, and pretty crazy in a lot of ways.

One of the technicians who worked for him told me “if I can do something to sabotage Arnold’s work, I will”. I thought, “Oh, that’s great. I’m really good to be in this lab.” Here I am, working with the forefront people in immunology about how to measure responses and do all this. I’m the measurement freak. Remember I’m the mathematician, so called, the numbers lady, that none of them are. And then here’s Arnold coming and telling me to set up this very stupid non-quantitative experiment with sheep cells, which we never used. I said, “This is just not going to work.”

So after about two or three months of going out there—actually, I liked going there. It was a very nice place to go to work. There were sheep in the pasture and there was a sheepdog that used to work the sheep. My desk faced right out onto this beautiful pasture. The building was always icy cold, but if you put enough sweaters on, you could sit and watch the sheepdog work the sheep. It was totally lovely to watch.

But I was getting nothing done out there, and I finally just settled down to a different kind of work. My daughter came to live with us and offered to be my secretary. I just agreed to write chapters for *The Handbook*.
of Experimental Immunology, for which I became the editor-in-chief some years later, after we went through several volumes. But it started out there. I was just writing. These were methodology articles. I wrote some for the first volume and some for the second volume. My role grew. I finally did the final set of volumes as lead editor. They were wonderful. I would like to go back and do another set of volumes if I could. These days I could do this without much more easily. With online publication, that you can do almost anything.

But anyway, César plunked me out in that place, and Len went to work at Mill Hill. There were strange things in César’s lab. César had just discovered how to make what we ultimately called hybridomas. Now, hybridomas are antibodies where you take a tumor that is a tumor growing in culture forever, and you basically mash that together with normal antibody-producing cells in such a way that the tumor now produces the antibody that came from one of the normal cells that you mashed together. Then you clone these out and you have clones, each producing a single type of antibody. Each clone got the genes that define the structure of the antibody it makes from a normal antibody producing cell. The tumor donated its tumor apparatus, which enabled the clone to grow indefinitely and produce infinite amounts of one antibody.

In your antibody-producing cells, the important discovery of Nossal and Lederberg that starts this whole chain going was that each antibody-producing cell produces one and only one antibody molecule. By that we mean the structure, the sequence of that antibody molecule is defined by the
genetics. A protein is made because you have a gene that encodes the protein, and the gene gets translated and eventually becomes the protein.

So the advance that César made was to “hybridize” antibody producing cells with tumor cells, to get cells that grow infinitely and produce only a single type of antibody. Len, with his graduate student at Stanford, had actually done this before Cesar. The joke is that the guy who did it often says, “I left my Nobel Prize in my desk,” because he did the experiment but wouldn’t calculate the data. He [Roy Riblet] finally admitted to me a few weeks ago, he admitted to me he had actually calculated enough of the data to know the experiment was successful, but he wanted to do something else and he didn’t think this was important enough to allow Len that much direction of his life. He liked Len. He just wouldn’t twist himself over to that. He’s a funny guy, and he’s always been that way. He’s brilliant, absolutely brilliant.

Kiefer: And this is who?

Herzenberg: [01:16:34] Roy Riblet, his name is. He still works with me. We’re back doing some very interesting stuff again on genetics and antibodies.

But in any event, Roy had done this before we left for sabbatical. César had announced that he’d done it. He’d published it. Roy figured, “Well, then, OK, he published it. I don’t have to publish mine. This is second best now.” But he had had it first. He knew he did. It was their Nobel Prize if Len had published it. But Len wasn’t concerned with that. He knew what these antibodies could be used for and wanted to get on with it. César was more interested in using the antibodies as a means of studying
antibody genetics, which was very interesting. And antibody structure. Cesar was an antibody structure man, and so he really wanted it for that purpose, whereas Len had other purposes for it.

Len got to the MRC [Laboratory of Molecular Biology] in Cambridge and started working with Cesar. There was a discussion that Len and I had. I guess it must have been very important because I still remember where we were sitting. Len said, “You know, these antibodies could be used as FACS reagents.” Because our problem by this time was there were maybe ten FACS machines out, and we had a meeting of the people who had the FACS machines in Cambridge. The big thing at the meeting was, “I see this with the antiserum I made and you see something different with the antiserum that you made. We can’t really compare to figure out which of us is right. The fact is that the antisera are different, and we don’t know whether it’s the antisera, the technique, or the FACS measurements that are different.

Basically, the FACS was less useful because we couldn’t get reagents that would reliably identify cells. To identify them, you have to make the cell fluorescent, and so you need an antibody that you can put a fluorescent marker on. You could only get a certain amount of that out of an animal, so the reagent was priceless, because you could never reproduce it in quite the same way. All antibodies are different and what we had in antisera was a mixture of antibodies. So you would label it with the fluorescence. You would then take the fluorescent that you labeled and put it on cells and then
wash off what didn’t belong there, and now you could see here this cell was getting fluorescent.

But you couldn’t pass enough of that antiserum around to ten people so they could all reproduce what you were doing. So it wasn’t science. It was very frustrating for Len, who was purely a scientist who said it’s got to be reproducible. You have to understand that you’ve got to be able to do it. We were lost without this ability to really understand, to really define the reagents, to have reagents that I could give to you, that you could take home to your lab and use, so that when you studied an animal that was the same kind of animal as mine, you should see the same things. The FACS should show you this, but the clean reagents were missing.

Without such reagents, FACS was languishing. So Len said that now we can make these antibodies, these hybrid antibodies—the animal made the antibody for you, so you could take an animal that was making an antiserum, you could take its cells, and now you could use those cells to mix them with the tumor that could make them immortal, that could immortalize them, and you could isolate them and you could clone each one, and you could then test all the different possible antibodies that you made and find the antibody that was good or find the antibodies that were good, and find different ones because the animal made all these different things.

So it essentially immortalized individual antibody-producing cells by doing this, and Len and I said we could use these as FACS reagents. Now, this had not actually occurred to César, as far as we know. Whether it had or
not, it didn’t matter. Len understood that this was something that had to be
gotten out and had to be used as a source of FACS reagents, because this
was what we needed. César got his Nobel Prize and he got the credit for
making the hybridomas. But it was actually Len (and I) who knew what to
do with them, and that was really more important. César actually was doing
interesting science about immunoglobulin molecules, but he wasn’t
interested in this. He claimed afterwards that that’s why he had done this.
Maybe it was why. It’s between him and his maker. I don’t know what the
story was. But he definitely was not talking to us about this when we got to
Cambridge.

Len had agreed to chair a “ski” meeting with Eli Sercarz during the
coming winter. He and I talked about this, realizing that he was in a position
to introduce monoclonal antibodies as tools for FACS. He talked to Eli and
the decided to add a session and ask César to come and speak about
monoclonal antibodies, which Len said was the salvation for immunology
and immunologists should know about this as soon as possible. We were
running one of these ski meetings, Park City, maybe, and he set it up and
got César come speak. We really wanted to get people to understand that
monoclonal antibodies could be used as FACS reagents or in other assays to
really identify cell types. But for Len, the meeting was to introduce
monoclonals as FACS reagents, and he was very, very excited about it. The
monoclonal producing cells became known as hybridomas, and I was the
person who named them. [laughs] It’s just very funny.

Kiefer: Wow.
Herzenberg: [01:22:50] We were having a Christmas party at the Cambridge Club. César invited us to his club for this party, Christmas or New Year’s, I forget which, but a holiday party. We were all sitting at the table and I said, “César, this is ridiculous. We have no way of talking about these cells. We say they are hybrid antibodies. We call them tumor antibodies. We call them cloned antibodies. Very confusing. We need a single name. It’s going to be New Year’s very soon. We need to name it now.”

So he said, “Well, what—?”

Nobody came up with anything else. I said “Hybridoma.”

They said, “Yes, that’s good. That’s a good name.” So that’s how they came to be named hybridomas, to imply that they are hybrids between normal antibody producing cells and a myeloma cell line capable of producing and secreting unlimited amounts of a single antibody.

Kiefer: So the patent resulted from that.

Herzenberg: No, no, it didn’t. There wasn’t a patent on hybridomas because neither we nor Bernie Shoor (leader of the FACS effort at Becton Dickinson) believed patents should be allowed on biological products. Len finally convinced Bernie monoclonals would be a viable product for BD. It took eighteen months to get from the time Len proposed that BD build a Monoclonal Center till they finally got it moving. At my suggestion, they took one of my B cell fellows (Chuck Metzler) to become the head of the Monoclonal Center and build it. I actually had a lot to do with building that initial Monoclonal Center because Chuck would often come to me with questions and stuff.
Anyway, we didn’t patent it. Bernie didn’t feel it should be patented. It was a biological material, and we thought it was wrong to patent it. That cost Becton, Dickinson $5 million because other people patented stuff that we could have patented, but they patented it and we wouldn’t. We thought it was wrong.

Bert Roland, the lawyer who did the Cohen-Boyer patent, argued with me about this. He argued strongly that patenting should be done, and I argued just as strongly, saying, “Really, we don’t want to do this.” And Bernie didn’t want to patent either. He was a very moral guy. We just felt it was immoral to be patenting this biological stuff, and it is, because it resulted in the reagents often being lower quality. The patented one was the one that won. Even if somebody had a better one, they couldn’t sell it because the guy who patented the first one was the one everyone has to use. It was very badly patented (from the standpoint of science and medicine). It shouldn’t have been patented that way. It could have been patented to apply only to a single monoclonal antibody based on its structure, or to all monoclonal antibodies that recognize the protein structure that the original antibody recognized. Instead, the patent was granted broadly to cover any monoclonal antibody that recognized any structure on the whole biological molecule that had the structure the original antibody recognized.

It didn’t do the field any good to have this broad control over all monoclonal antibodies produced to a single biological molecule; it didn’t do medicine any good; but it did make a lot of money for the person and company that patented the original antibody. It cost Becton Dickinson a lot,
because they had to pay royalties for antibodies that got covered by the patent. But because Becton, Dickinson had built the first as the Monoclonal Center for FACS reagents, in the end, they were OK. Bernie said people would pay for it without the patent, and they ran for several years before the patent kicked in. They did pay lots of patent royalties, so ultimately Becton Dickinson and we started patenting our antibodies. We did so because it became the rules, basically. But we really did not, and I still do not, approve of monoclonal antibodies being broadly patented. I think there’s a very good argument against it. It is not in the public interest, because someone can make a better antibody, or biological, but still have to pay royalties. Because of this, companies are not interested, even if the antibody is really better. The economics are against it.

The patent we have that has made all the money for Stanford was a different thing. Len foresaw that antibodies would be good for therapy. He tried to get Becton, Dickinson to understand that, and Becton, Dickinson did not really. He was head of their advisory committee at that time. But they didn’t want to get into medical stuff, having to deal with the FDA, and also they didn’t really think monoclonal antibodies would be much in terms of therapy.

At the same time, there were problems with this because if you use a mouse antibody into a human, the human becomes immunized to the mouse protein and then the antibody doesn’t work anymore. So what was necessary was to humanize the antibody, and that’s what the patent is for. It’s a way of taking the mouse “business end” of the antibody molecule, the
thing that recognized what you wanted the antibody to bind to, and tying it
to a human “nonbusiness” end, so that you would have a functional like an
antibody molecule that has the specificity of the mouse antibody but the
overall structure of the human antibody.

Len, Vernon Oi and a woman named Sherie Morrison recognized
this could be done. This was their thing and they did it beautifully. I was a
supporter, but I was not involved. To my recollection, I was not involved
even in developing the understanding of why this was really important. I
probably was, but I don’t remember. This was stuff that Len and Vernon
led on. It took seventeen years for their patent to be approved.

Ultimately, this patent paid huge royalties to Stanford. It in no way
restricted what antibodies could be used with this technology so, unlike the
monoclonal antibody patents, we had no problem with this patent.

**Kiefer:** Wow.

**Herzenberg:** [01:28:04] The length of the approval process made this patent what’s called
a submarine patent. It was, I think, the last of the submarine patents. It was
there all the time, constantly being questioned and revised, questioned and
revised, and finally they granted it.

**Kiefer:** Is that what the term “submarine” means?

**Herzenberg:** [01:28:20] Yes, submarine patent means that it’s been there, it has the dates,
and the way the dating worked for the patent at that time, you got the patent
for a certain number of years when they issued it, rather than from the time
you applied for it. They changed that because of the submarine problem.
This was a prime example of it, but there were probably other ones.
Anyway, this patent and its underlying technology allowed people to make therapeutic antibodies. By the time the patent was granted, people understood how to make a therapeutic antibody that would actually help for disease. It is what’s called Remicade [infliximab]. This one was a therapeutic that will alleviate severe rheumatoid arthritis, a really, really important very broadly used reagent and a very important patent.

All of these monoclonal antibodies therapies are based originally on the hybrid antibodies. Now there’s some other ways of doing this, but it’s all basically the same principle.

This patent, because it was used to treat rheumatoid arthritis, has generated tens of millions for Stanford, so this was the big patent. There were other patents that generated lots of money as well, but this was the very big one.

**Kiefer:** Some professors and researchers have their own companies on the side that maybe they put together with graduate students or with colleagues. Did you and Len have anything like that?

**Herzenberg:** [01:30:09] We worked with Becton Dickinson. Len got a pittance. Actually, they originally made a deal with Len that was a beautiful deal, and we would have gotten, like, a penny for every reagent they sold, which would have been a fortune. Then Bernie, who represented us but also represented the company, came back and said, “No, they’ll break the contract. They won’t let you have it.” Len didn’t care, really. Neither he nor I really—we’re academics. We didn’t think we needed to make millions of dollars. We just needed to make enough to be able to do our research and live the good life.
We just were not in the business. It was a different time, maybe, or maybe just we’re old-fashioned, but it didn’t matter enough to Len to make us change our life style.

Of course, we could have gone out and started a monoclonal antibody company, we could have done other things, but it’s not what Len wanted to do, it’s not what I wanted to do. So we’re still in that same situation. I have software that’s under development right now—it’s taken us a long time to get there—that could make a lot of money for us. But I don’t really want to run a company and I don’t want to be involved. I have tried to run some companies just because I couldn’t get any company with traction to actually get behind this. So we did try and set up our own software company. I’m lousy at this. I don’t want to spend the time at it. Len was lousy at it. And all we did was lose money in those companies. That’s all we did. We put money into them and we lost it, because it’s not what we want to do. You have to really want to do this. You have to be willing to be nasty. You have to be willing to keep things to yourself and not talk about science that you want to talk about. It just was never what we wanted to do.

So with Bernie, Len built BDIS. That company is now a $2-billion company at least, and really he and Bernie built this. Bernie is not particularly appreciated for it. He’s still alive. He’s a curmudgeon, and he never had much use for me as a woman in this game. He always felt that Len gave me too much, because his wife was not treated that way. I think
Edith actually didn’t feel that women should be included. She could talk a good game, but she didn’t really believe that they should be.

I really don’t know. Bernie never wanted to recognize any of my contributions, and Len would get up and say that this was done by Lee, or Lee and I together. And Bernie would say, “Why are you saying that? You’re spoiling things.” Because the men in his company, who ran his company, were very conservative, and they did not recognize women.

They did not like to recognize somebody without a tie either, and Len went to meetings without a tie. Bernie just didn’t approve of all this. The world left him behind because he just absolutely refused to allow the world to let women in, allow the world to let people with ties in, the whole thing.

**Kiefer:** What was the first? Dickinson and what was the front?

**Herzenberg:** [01:33:52] Becton.

**Kiefer:** OK. What’s coming to mind is Beckman Instruments.

**Herzenberg:** [01:33:58] That’s right. Everybody’s very confused. Beckman, there’s another whole story there, not with Arnold Beckman, but Beckman Instruments eventually bought Coulter Instruments, and Coulter was the other person who made a cell sorter and made a business out of it. Wallace Coulter would hate me to say this, but it’s true: he was always too little too late.

Coulter eventually had people working with him (Mack Fulwyler, in particular) who had given Len the original plans for the drive train for the cell analysis and sorting part of the sorter. They essentially laid out how you
would do this, and built a particle sorter that Len realized could be used for fluorescent cell sorting. Len went to them and he said, “Would you put fluorescence detection on to this?”

And they said, “No, it’s not our mission.” They worked for Los Altos. They were doing fallout stuff, which to Len was anathema to begin with. But they were doing this stuff and they wouldn’t put fluorescence measurements in. Instead they said, “But we’ll give you the plans.”

They were good guys and they gave us the plans, and we came back and we built the sorter, starting with their plans. However, their plans showed a nozzle on it of the type that Coulter had. We built it first that way, but the nozzle had problems and had to be taken off and replaced with the nozzle that we invented here. So it wasn’t their instrument that became the prototype for the FACS that Becton, Dickinson eventually built.

**Kiefer:** As you worked at Stanford, did the university or the med school ever give you any problems as a woman involved in research?

**Herzenberg:** It depends on what you mean by problems. They never gave me any recognition as a woman involved in research. [laughs]

There’s a long story with this. There was a Women’s Committee set up at some point to try and recognize women. I somehow must have asked them to help me get recognized at some point because I remember that their answer was, “Well, you work with your husband, and we can’t tell what’s what.”
I said, “Gee, thank you. I’ve got papers. I’m the first author on them. You can tell what’s what.” But, no, I really never got any recognition from them.

In Josh’s world, he was happy to have me, but partly because of Esther, I guess. But he was very careful. “Len, you have to be really careful if Lee’s working with you to be sure that you don’t give her more than you should give. You don’t want it to look like nepotism.” We agreed, and accepted much more stringency in what recognition I was given because we didn’t want our working relationship to have any appearance of nepotism. So I didn’t get promotion and lived outside the system.

I was not allowed to write my own grants, but I wrote them under Len’s name and Len signed off and included a statement that I was actually leading the project. I wrote my grants, and in fact I wrote Len’s grants, because Len never wrote anything ever. He did not write. He told me what he wanted, I wrote it, I corrected what needed correction, we argued out any disagreements, and I corrected it according to the way he finally wanted it, et cetera. He never wrote anything—I mean, he wrote letters, but even those he’d give to me and say, “Put it into English.” Remember, I was an English major in high school, right? He always said, “You were an English major. That’s what you wanted to do, creative writing. Do it.” [laughs]

So there it was. Len and I worked together. I published my papers. I loved what I was doing. But I never got a promotion while Josh was chair. Maybe I was promoted to senior technician or something like that.
Then Josh left and Stan Cohen took over. There were some people in the department who pushed for me to get some sort of adjunct faculty appointment so that I could apply for my grants, and they felt that I should. One of those people wound up having to go elsewhere. He did fine. He’s not complaining that he had to go elsewhere, but it was in part because he was pushing on this thing. I’m sure Stan regrets it at this point and would say he didn’t really mean it quite that way.

After all, I didn’t have a formal degree. The, one day, Pierre-André Cazenave at the Pasteur Institute was talking about me with Len and Pierre asked something about my degree, and Len replied, “Well, she never got a formal degree.”

And he said, “Well, she did her work here at Pasteur Institute, didn’t she?”

And he said, “Yes, she was working with Jacques at that time, and she relates to that a lot.”

He said, “Well, she speaks French.”

Len said, “Yes, she speaks it.”

“Well,” he said, “she could actually get her degree at the Sorbonne, and I’d be honored if she’d consider herself my student.” So that got set up where Pierre-André became my advisor. He worked very closely in the field I’m working in, so that’s why this happened.

Kiefer: It looks like this was around 1981.

Herzenberg: [01:39:49] Yes, that sounds right. I eventually wrote a thesis. The date is in the thesis. Then it took three years for the thesis to get accepted because
Pierre-André kept forgetting to put the papers in. He was famous for this. It wasn’t just me. However, the students in his lab would keep tabs on him so that their papers would get in on time. So I finally got somebody who was in France who would follow Pierre-André around and make sure he put the papers in.

My thesis was in an area that was directly related to Pierre-André’s work. So it was straightforward for him to start the degree process in the usual way by constituting a thesis committee for me. The committee included Klaus Rajewsky, the foremost scientist in our field, Spedding Micklem, a renowned stem cell expert, and Sam Black. Sam was “only” just a beginning professor at Amherst, but he understood the work. I can’t remember who else was on the committee, but I am sure there was someone from the Sorbonne in addition to Pierre-Andree. I submitted the thesis, gave a formal thesis talk to my committed, and the thesis was accepted with honors. The only other thing I had to show was that I spoke French fluently, which I do. It is a requirement because you have to be able to teach at the Sorbonne. I think Pierre, with whom I normally spoke French, attested to my proficiency.

So I completed the requirements and then became formally doctorat d’Etat. So I had finally had a respectable degree. When I came back and told Stan this, he questioned whether it was real. I was kind of indignant and I said, “Of course it’s real.”

He said, “Well, I don’t know.” But he still was not willing to give me any promotion.
By this time, in order to submit this at Sorbonne, basically you’re submitting for what would be an associate to full professorship. That is what the degree actually meant in France. You could teach classes at the Sorbonne and you were a professor at the Sorbonne, or could be if you chose to join the faculty. They had a different system.

They had a different system there (it has changed again, now). Anyway, at that time I could have just decided, “I want to move to France” and I would have had the right to teach in France.

But Stan was not really willing to accept this. To my understanding—and this is only secondhand gossip—there was a committee that investigated the department. Josh had gone and Stan had taken over, and they had an investigation of the department, and apparently the one action that the committee insisted on for the committee was that I should be promoted to research associate, I think it was.

Anyway, I was appointed as a Research Associate, and later I was promoted to senior research associate. This promotion gave me the right to ask for permission to get my own grants, so that I could finally sign my grants myself.

Len had to always write a letter saying, “This is Lee’s work and she did this.” And people who knew us well said that the discussion at the study section was not whether this was Lee’s work. Everybody knew it was. They’d heard me speak. My friend sat on one of the study sections and told me this story said the question discussed at the study section was whether Len or I wrote the letter that said it was [the work in the grant was my own.
work]. [laughs] I laughed when I heard this and said, “Well, you know how we write in this lab.”

Kiefer: Eventually you ended up with the title of research professor, is that correct?


Kiefer: But that took about thirty years till about 1989?

Herzenberg: [01:43:33] I don’t know. There was no real counting. When David Botstein took over the department, he promoted me, I guess, to associate professor, and that’s when I got my formal right to apply for research grants. He was a good guy, David, and he basically appointed me co-terminus with my grants. My current department chair just said, “Oh, just forget about the co-terminus part. You’re in the department till you die.” [laughs] So I’m still sort of an anomaly in the department, but the current chair is wonderful. I love working with him.

Kiefer: And that is who?

Herzenberg: [01:45:02] It’s Michael Snyder. I’m working quite closely with him. We share a graduate student now. He’s wonderful. He had a close friend when he was at Yale (before Stanford) who was one of our students [Paula Kavathas]. He had a lot of respect for her. She was mainly Len’s student, but of course I knew her well (in part from helping to write papers with her). So Mike knew a lot about us when he came. We couldn’t understand why he just seemed to know us, but he was very welcoming and made us feel more in the department than we had felt for a very long time, because he just knew who we were and how we ran the lab and what we did.
Mike also appreciated that the department gets a good chunk of that patent money. But it’s not something we have a control over. It just comes from the school. That patent money comes in, goes to the school, and gets distributed out, but we get a big chunk of it. Years ago when there was patent money coming in or any kind of money coming in for stuff we had done, we’d all signed a statement,—Len and all of us,—saying that that money really was acquired because of work we did in the lab and therefore should be donated back to the lab. That was what we did in the beginning.

It then came to be that people would leave the lab, but they would get patent money, and there was really no reason for them to donate that back to the lab. Then if that were true, well, then there’s no reason if you stayed in the lab why you have to donate it back. So we went to the university policy, which is that the inventor gets a certain share and they can do what they want with it.

But the money we got from patents we have always fed back into the lab as much as we needed to. I just now, in this last week, committed to feed a fair amount back in again because we’ve just made a new finding that requires that there be some more people working in the lab doing this work, and grants are just awful to get. I’m going to be paying for several people for a while who I would hope I will not have to pay for indefinitely. But it is good to have this patent money to keep the lab supported with. Actually, the patent money will end soon so I have to be careful with support to make it last.

**Kiefer:** What are some of the sources of the grants that you’ve had over time?
Herzenberg: [01:47:27] Always the NIH.

Kiefer: The National Institutes of Health.

Herzenberg: [01:47:32] Yes. Some of it from the National Library of Medicine. A lot of it over the last few years has been the Division of AIDS, but mainly because it’s all computer stuff and they need what we’re developing, and that’s what we’ve just developed. That’s why now I need to make this stuff so it can go out to people. We’ve just developed this absolutely gorgeous stuff. I’m so pleased with it.

Kiefer: That’s wonderful.

Herzenberg: [01:47:58] Jumping up and down!

Kiefer: Back a while ago, when you talked about the neighborhood culture that you grew up in, you also mentioned that you were part of the Federation of American Scientists and that politics were important to you.


Kiefer: How important are politics to you in particular, but also to Len over the years? And when I say “politics,” maybe you could define that for me if it’s Democrat or if it goes beyond that, what would you say?

Herzenberg: [01:48:45] It goes beyond that. University politics are of no interest to us, have never been of interest, but then again, they’ve never been interested in us either. That probably started in the 1970s or the late 1960s, actually, because although we were older, our colleagues did not understand where we were, because we moved into the counterculture. We knew about smoking marijuana. We never, neither Len nor I ever did any other drugs but marijuana. We knew lots of people who were taking acid [LSD]. That
was OK for them, but Len and I felt that we had kids and we didn’t want to take any risks with there being a problem due to the drug. So we wouldn’t do any drugs other than marijuana. But we were in the hippie culture, if you like. It’s not really hippie. Hippie was the apolitical side of it.

So we helped to establish the Palo Alto Free University, which was a very, very important force during the late 1960s and early seventies. Len helped to establish a group called the Technology and Science Committee that ran coincident with the Free University and helped engineers who didn’t want to work for Lockheed get out and work for non-defense industries. In fact, the FACS engineers that we have in our lab are all from that time. One of them was kind of apolitical and he joined the lab because he was already an established engineer and ours was an interesting engineering project. He was working for Varian when Varian was interested in a project that included flow cytometry. Varian decided to close down the interesting, flow cytometry related part of the project, which this fellow [Dick Sweet] was working on. He had special skills related to flow cytometry and Len was devastated that Varian’s closing the project would disrupt his work with Len. So I said to Len, “Well, why don’t you ask him to come work for us?”

Len said, “Dick’s an established engineer. He’s never going to come over here.”


“All right, all right, all right. I’ll ask him.” [laughs]
And Dick [Sweet] said, “I’d be delighted to come to Stanford and work with you on the FACS.” He was the guy who invented the inkjet printer, which we use as a principle in the FACS instrument. We steer the thing the same way as you would ink to print letters. So that’s a central part of the FACS, and that’s how Len got him to come work with us.

**Kiefer:** Oh, is he the Sweet of Sweet Hall?

**Herzenberg:** [01:51:29] No, not he. I don’t think it’s the same family even. But Dick had been at Stanford. He has an interesting history. He was also part of this sort of radical-ish group.

The whole group of engineers that I was talking about earlier, who played ping-pong down with us in our “recreation” room, were all more or less leftish. We were all kind of leftish in one way or another. There were a few people like Elliott Levinthal, who was the head of that engineering group, who were straight Democratic, much more Democratic. They favored the war in Vietnam long after we had all become “anti-war” and were joining protests. Henry Kaplan himself took until 1971, but he then came out and made a major, very, very important statement against the war.

So the University itself was largely against the war, but that said, that’s not true. It was mainly the students and many faculty. The university was largely pro-war, and the engineering department was really gung-ho. But the medical school was more or less divided along “surgeon” lines, although a lot of the surgeons were counterculture. Mainly, it was just the more conservative M.D.’s, mainly older, who favored the war.
Oh, put down on this list we have to talk about: Medicare. I forgot to tell you about Medicare. That’s an important one. These are all interesting things that happened back there.

Politics was just important, a very important part of life. I mean, I grew up that. My family were Democrats, and a big issue was being for or against segregation. At that time, to be a Democrat meant to be anti-black. But we didn’t agree with that party view. My father was a Roosevelt democrat. That was different. We didn’t like the segregation part of the party. We were mavericks. My father was certainly anti-segregation, as were my family, my uncle, my brother. My brother hung out with the Pete Seeger crowd. My uncle hung out with that crowd.

So I was always brought up to be more or less counterculture and leftist, and it was not any great movement for me to recognize that I belonged with the counterculture. I was never hippie. I didn’t particularly approve of the kids who were going up and sitting on the street. So one of our engineers got the idea to teach them to bake nutritious bread, and we all worked with helping them to learn to bake bread so they could eat. I think it eventually got name “digger bread”. We did a lot of things.

The acid (LSD) culture and the Merry Pranksters were around Palo Alto at the time. I never really got tied into them. They regarded me as too straight because we were scientists. And we were straight, really. I mean, we had our brains to keep together. We had activities we were turning out. They were busily counterculturing because they were creative artists who could afford to counterculture. We needed to keep our focus on our work
Kiefer: Didn’t Ken Kesey live on Perry Lane?

Herzenberg: [01:55:30] He did, at least I think so. That was right where Gus Nossal (from our part of the Genetics Department) had lived. The Pranksters and their friends were all in this area and we were connected to them through the Free University. Everyone was connected. So we all knew each other.

I used to run rock concerts. I ran a lot of rock concerts around here as benefits, like for the Stanford Day Care Center. I was probably a key driving force because I was older and maybe more organized than most of the people. I was working with a lot of other people, people who recruited the bands and people who did this and did that. So I was just one of the pillars of running Frost Amphitheatre rock concerts.

We ran the concerts until a concert by Sly and the Family Stone, which made it clear that the culture had moved somewhere else, and we didn’t want to be in the business anymore. Too many crazies and too many people running around on acid and worse, doing all sorts of stuff that really wasn’t—it was the disintegration of the counterculture as we knew it.

But I spent a lot of time doing this work. We helped start the Stanford Nursery School [Day Care Center]. There was no childcare center before that, so we established that and we ran concerts, we collected money that way to pay for the nursery school and to do other things like that. My son was in the first class. So I loved it.

The music was very important to me. My daughter was into music. We used to run something called Free People’s Free Music. We were just talking about it the other day. We should reopen it again. There were a lot of
kids then—remember we were in the hippie era, and there were a lot of kids just sitting around on the street doing nothing. So we said, “Well, let’s run a concert every Friday night.” And we just used local bands and we got a sound system and we ran the concert and mad a constructive place for the kids to be.

My God, we had to fight the police on this. All we did was to put a concert downtown on Lytton Square that was had every Friday night. My daughters and friends had a place to go, and there was something happening on Friday night that was wholesome. “You’re bringing all the worst elements in here,” we were told. Anything that happened anywhere within a twenty-five-mile radius, practically, they blamed on that concert. But we did it.

The main guy who ran “Free Peoples Free Music” with me--actually I ran it with him -- Tim Gadus was about 19 years old at the time. I was the grandma of the group, and there were some younger guys who actually did much of the thinking and all the running. But I was there and I just saw to it that people were properly treated all the time and that we all kept our brains together.

Timmy eventually became an important administrator here at Stanford. He is still a delightful guy, a grandfather now. He just laughs when he remembers himself when he worked in our animal house.

Len offered him a job in the animal house because he was basically starving. He was a really smart kid with not much education and no family.
He came to our house one day and he just lay down across one of the chairs. He was just obviously wiped and starving.

Len looked at him and said, “Timmy, do you want a job?”

Timmy: “I’ll take any job,” Timmy answered

Len: “Even changing mouse cages?”

Timmy: “What do you have to do?”

Len: “It’s easy.”

Timmy, “OK, I’ll take it.”

In the end, Timmy became top animal care technician, then graduated to being top research assistant in the lab, and ultimately moved into the Stanford facilities administration group, where he is one of the long-term top people.

And we’ve done this. We’ve had a number of kids like this who’ve come in untrained and gone out as students or doing other kinds of responsible jobs. Essentially it’s a rescue route for kids.

We’ve got another one started just now. She’s coming through the lab, and she’s going to be a first-rate research assistant. So we just start them in the animal room, and then, unfortunately, they get too good for the animal rooms. [laughs] Then we have to move them to lab work, and give them a helper for the animal room. But then the helper comes up through the ranks, et cetera. So it all works out nicely.

**Kiefer:** Do you encourage them to get degrees?

**Herzenberg:** [01:59:15] If they will, they will. Timmy never did. He just grew up through the ranks. But he knows the facilities at Stanford. As I said, he became a
high-level facilities manager. He sits on all these big committees. It’s
delightful, you know. Every so often we get together for some reason or
another, and we’ll just be amused at how good everything is -- that the fifth
grandchild was just born and that all these kinds of nice things keep
happening.

**Kiefer:** Great. What was the Free University?

**Herzenberg:** [01:59:45] So the Free University was a concept that came from Europe, but
part of it began here in this [my] house in my back room. There were four
or five political science students, political science and history. We knew
them because one of them was the son of one of our professors at Brooklyn
College and got in touch with us. This has to be 1962, just after we moved
into the house.

I can still see the bedroom, the fireplace was not quite finished, and
they were all sitting in one corner. They were furious because they were not
allowed to have classes in Marxism, some aspect of Marxism, I think maybe
Marxism itself, but they weren’t allowed to have classes in economics of the
type that they really wanted to learn and have a class on.

**Kiefer:** At Stanford or—

**Herzenberg:** [02:00:52] Stanford. So they wanted to protest this, and then finally they
decided, “Hell, we’re never going to get this at Stanford. We’ll start our own
university.” So they started the academic side of the Free University.

The Merry Pranksters and their cohorts, they’re fringier people, not
they themselves, because that’s not what they did. Some of the fringe people
from their group started the what we used to call Sand Candle side -- people
who liked to go to the beach and make sand candles. They were very good
candle-makers. But they wanted to learn, do and teach more counter-culture
things than just candle making – things like artwork and photography and
running “encounter groups”. So we merged all this into the Free University.
There was always this interesting tension between the more academic side
and the less academic side of the “Free U”, but we all took part in both.

Len taught a course in domino theory. The domino theory, if you
remember, was that all of Vietnam and Laos and all these things would fall
if we pulled out. But as a joke, Len called the class “domino theory”. We
used to play dominoes all the time. So he gave a domino theory course, in
which people played dominoes and taught each other how to play well.

We, the Free U started a coffeehouse on the corner where Rudyard’s
Pub is now, around on the circle there.

**Kiefer:** On University Avenue?

**Herzenberg:** [02:02:16] That’s right. It was called the Full Circle, and that was great.

That’s where, actually, we first met Wayne [Moore], who works with us.

The Free U had its own printing press that used to print lots of
documents and stuff, and that was Marshall and Sherry. Before Sherry met
Marshall and “settled down”, she used to play guitar with Jerry Garcia
upstairs in the building that we turned into the coffee house. Wayne
wandered into town as a nineteen-year-old and started washing dishes at the
Free U at the Full Circle. Our daughter, Berri, was washing dishes there as
well, so she got to know Wayne.
Sherry and Marshall found him homeless, sort of wandering around town, and they said, “There’s this nice kid, he’s nineteen or so, and just floating around, doesn’t seem too bright. But very sweet. We gave him a room in our house. If you could give him a job as an animal caretaker—”

It just happened that we had no animal caretaker at that time. They kept turning over (this was before Timmy). So we gave Wayne a job as an animal caretaker. Best animal caretaker we ever had in our lives.

Wayne turned into one of our best friends, and one of the best software engineers we have ever met. We are still very close. He is just solid and totally brilliant. He went back to school, kept working with us and eventually wrote or directed the writing of all of the software we have developed.

Today Wayne is one of my closest friends. But he came in through the same route Timmy came. He was in Free People’s Free Music, worked at the coffeehouse, finally went to Stanford and has been doing high level software ever since. He is also doing very high level mathematical physics and my ultimately solve one of the key questions in that world. I talk with him about this all the time. I don’t understand it all, but Wayne makes it understandable, at least in part. I love following his progress.

The Free U time was a very interesting time. Part of me lived in that counterculture. I wore skirts to the lab all the time, come home, take my skirts off, put my jeans on, and then just be there, in the counter-culture. Len was there, too, a little less counter-culturish than me, but always there. He also played key leadership roles in that community.
We helped to publish what was called the *Research Guidelines for the Student Movement*, 1969 Student Movement, and through that I met Dave Parks, who’s another one of our very long-term friends. He was a Stanford graduate student in physics but is now also a biologist. He has worked in the lab since the early 1980s, or before. He took over the FACS project after Dick Sweet (the engineer from Varian) decided that he really would work on the project, but that he didn’t want to lead or manage it. David took it over.

So it was all this integration. OK. Political life integrated to lab life, integrated to political life. Neither Len nor I really made a distinction, but we had our own responsibilities. Len was sitting on study sections. He was traveling around the country. We were speaking at meetings. The lab was growing. We were teaching. It was consistent with all this. I think the only ones who suffered were our daughters. [laughs] They had to fend a lot more for themselves than, as they see it now, they really wanted to.

**Kiefer:** And that does beg the question, how did you manage with four children and all this activity?

**Herzenberg:** [02:05:10] Well, only three really. Michael has Down Syndrome, and we knew that when he was born. They thought he would die within a year, and I didn’t want my girls to have to live through a dying brother, so I put him into a foster home. His foster mother turned out to be a wonderful woman. She saved his life and he lived with her forever. He found her when she died. He now lives in a foster home with several other wonderful women who are really good to him.
But my two girls, they will tell you that some of our counterculture ways were very disruptive for them. We did things that now they will tell me, “Well, you know, that wasn’t the right thing.”

And I just said, “Well, you’ll tell me what was the right thing, but I don’t know.” [laughs] “I can tell you why I did them, I can say to you I’m sorry if it hurt you, I can say to you that I’m glad that it helped you, but it was what we did.” We did what we did it thinking that we were doing the best things for them. Len and talked about this a great deal at the time, and we talked a lot with them, too. We were always very open with the girls.

Kiefer: So we were just saying about you coping with the kids and how you were dealing with grass (marijuana) and them, and I assume they were teenagers.

Herzenberg: [02:06:39] Well, it’s not grass that was the problem. It was just the openness of what was going on here. When there were violent demonstrations, we were always on the nonviolent side of the meeting and against the violence. The rock-throwing and stuff was not something we approved, but it was something that was happening – a part of the movement. That kind of massive violence we could say we don’t support, but we understand why young people are throwing these rocks. They were throwing rocks at building windows, so, you know, big deal. Nobody’s getting hurt, but there is property damage. And given what was being damaged in the Vietnam War and what the fabric of the country was damaged, we could live with that. We could not live with any kind of other violence. Bombings, violence, stuff like that. We never knew anybody that was involved in that and never wanted to. Maybe I did know them, but I didn’t know who they were.
And it turned out that the most violent person on campus was an FBI informer, and that I did know, and that I did actual legal research on because there was a trial and we had to collect information. It turned out we wound up researching this one guy who then turned out to be an informer, and he was being run by FBI. He was a provocateur, basically, and they knew it. There was no question. He was the one who shot the holes in the president’s windows. He was the one who shot the holes in Dinkelspiel. He was the one who broke the glass at some sit-in where was a big piece of glass was broken. He did the violent acts that enabled other people to just do this, and he led the rock-throwing. There was no question that he led it and stimulated it.

I disapproved of it, I never went to it, but I supported the goals of the movement and I understood why nineteen-year-old kids would do this. We were a hospital section. At one point, this table [in our house] was a hospital section because kids were having teargas canisters thrown at them and all sorts of things, and they came here for help on that night.

So our kids were exposed to all of this. Their friends—they were teenagers -- their friends were in the movement—.they were close friends with some people who later were involved in much more serious stuff.

The part of the movement that I was in was opposed to the Revolutionary Union, opposed to Venceremos, opposed to any of the overt violence. The guy we more or less followed was a guy named Lenny Siegel who said, “I’m all in favor of violence when 51 percent of the people are doing it.” [laughs]
**Kiefer:** Lenny Siegel?

**Herzenberg:** Do you know his name? I just saw him. We just had a reunion of the April Third Movement. This was called the April Third Movement. How do you know Lenny?

**Kiefer:** My husband worked in in information systems at Polya Hall and that whole area, and he was around the day that Bruce Franklin led the charge in there.

**Herzenberg:** [02:10:24] Bruce Franklin didn’t lead that charge.

**Kiefer:** Really?

**Herzenberg:** [02:10:27] I’ll tell you exactly what happened, because I was there.

**Kiefer:** Really?

**Herzenberg:** [02:10:30] Lenny and Yale Braunstein and I had a meeting that was kept secret from Bruce. He did not know there would be a sit-in there, and he did not know that we organized a pre-meeting on White Plaza. Bruce was the last person to speak at the meeting because he was not invited to speak. He was definitely not invited to lead the sit-in, but Bruce being the Venceremos leader didn’t want to be left behind. He put himself in front of the crowd after they started walking and said, “OK. Let’s go”

Everybody was all ready. The sit-in was already started. Lenny and these people had mapped out Polya Hall. I was part of the discussion. I attended the meeting.

I had certain things that I did for the movement. As I said, I brought chicken soup to the movement all the time. Not literally, but I *was* chicken soup.
Anyway, I was on the corner. Bruce was here still talking at the microphone. White Plaza was there. I was walking out towards the front of the Union. We walked to Polya Hall, maybe by the other side of the Union. I can’t remember exactly. I can see myself and I remember Bruce standing at the microphone. It was the last speaker, and he said, “OK, come on, let’s go.” Everybody had gone. The police were standing there, thought Bruce was leading it, Bruce thought he was leading it, but the key people had already gone and were already over at the hall, sitting in.

Now, when the hearings were held, I told this story. You can find this in the testimony in the hearings. I told this story at that time, and I told them exactly who had planned Polya Hall, because we took responsibility for it. But Bruce himself was going to claim that he—he would rather be fired for something he didn’t do and say that he didn’t do it. [laughs]

He just did something very good, though. At Rutgers, they were going to have Condi Rice as their commencement speaker, and this was too much for Bruce and Jane. They and their friends told the students who Condi Rice was, and the students said, “No, we don’t want this person to be there.” They were right, this was the right thing to do, and they got another commencement speaker. So they were very proud of the fact that they’d done that. And I was glad to hear it.

Kiefer: To kind of wrap things up, I’m going to ask you were you ever actually formally involved in the Women’s Movement?

Herzenberg: [02:13:20] Probably not, probably not. I mean, the stuff at Stanford was run by—I guess Dody [Dora] Goldstein was the one who did most of it. I don’t
I don’t even know who did. I never really got together—was part of that. As I said, they told me that I worked for my husband, they didn’t know what to do about me, which I thought wasn’t sisterly of them. [laughs]

Kiefer: Right. Now, about the “they” that we’re talking about who said, “Well, no, you work with your husband.” Were these the people running a study of women in the med school who said publications with women as authors weren’t out there as much?

Herzenberg: [02:14:22] I don’t know who they were. What happened actually, which was very important, was Hal Holman was the head of medicine. Rose Payne and Judy Pool worked for Hal Holman. Rose Payne was a very good friend. She was an old lefty from the Union Movement, and her husband was a longshoreman. They used to take us down to the Longshoremen’s Hall to meetings. They were great people.

Rose was actually the founder of transplantation biology, of measuring what was needed for that, and Walter Bodmer worked with her. She wasn’t in our department, she was in medicine, but she was all part of this.

People recognized that Rose Payne and Judy Pool were eminent scientists, were well recognized outside of Stanford, but had never received an appointment. I was probably as well recognized as them. But I could go to all the key immunology meetings because Len was invited. I didn’t have to be invited. Eventually, we were both invited to the meetings. We were a different generation. They were a little older than me. So I was part of the group that was pushing for appointments for Rose and Judy.
It was kind of a very big shame because Hal Holman, who failed to appoint them, was the chair of Medicine. In other things, he was the big liberal, OK? “Liberal” is not the name he would like me to apply, but “radical” would probably be the right name. To his shame, he never thought it was important to appoint these women; or if he did think it was important, he never made it clear that he met enough opposition that he couldn’t appoint them.

I think that was the bottom line. But Hal did very important things in building the department, very, very important. He was also one of the people who were with us in the movement, and a very strong supporter of the Federation of American Scientists, which included our friends from Paris, Dale Kaiser and Dave Hogness. We had that group here. There was already a Stanford branch when we moved out here from Washington. Hal became part of that. He was part of the Physicians for Social Responsibility.

So there were a bunch of these little groups, but none of them focused on women. I don’t know who made the fuss about Rose. It might have been Walter. I don’t know who made that fuss, but I certainly was glad they made it and felt part of it.

But I’ve always been somewhat miffed that there’s a lecture that honors women every year here, and I have never been asked to talk at that. I usually don’t go to the lecture. This is my “bad”. You could say if you don’t go to the lecture, you won’t be asked to talk at it. OK, it kind of goes both ways.
But I’m too radical, my science is too radical, for a lot of the women.

Over this year, we have been proving a theory that I put forward that Len joined me in signing—but it was my stuff—that the immune system evolved as a set of layers in which each layer did all the different things in the immune system, but each layer was more advanced and more protective than the last. The current idea that you have a single stem cell—everybody talks about the stem cell—is incorrect. You have a set of stem cells that are layered on top of one another.

So we’ve finally, after all these years, proven that the two major branches of the stem cell go all the way back to the two-week-old embryo in the mouse, less than two weeks, ten days or nine days. And yesterday we found that, in fact, in the human the same thing is true. It’s just beautiful stuff. My fellow, he put it together. I have two really phenomenal fellows in the lab, probably because I could just use the patent money to support them when the grants ran out. So I could keep the team together.

Anyway, this is very radical science. It has parted me from the majority, and it parted me from the majority in another particular thing that has to do with regulatory T-cells. So I’ve always run science more radically. And what we’ve done is the FACS, which was mainly Len’s thing to build. I did science with what he built all the time, implementing and testing and changing features, et cetera. Len led the engineers, got them to build what we needed, my group implemented the science side, found out what more was needed, started another round of building, et cetera.
We’re just up for another layer of this now. We just finished reporting it out at the meeting in Florida, which I wouldn’t go to because I hate Florida. But we were a triumph there, and that’s going to come back and we’ll have a new layer of ability to do science.

But my thing is to hang out with the heavy mathematicians who can, in fact, put this stuff together. The immunologists are really not able to do this, so they all don’t like me. [laughs] It’s not that they don’t like me because I’m a woman. They don’t like what I do. I’m radical in what I do, I’ve always been radical, and my politics were radical when there was such a thing as radical politics. If there was again some kind of radical politics, I’d probably join it, but there’s nothing to join right now.

**Kiefer:** I know you’ve received a lot of awards, but what award or awards are you the most proud of?

**Herzenberg:** [02:20:33] I haven’t received a lot of awards.

**Kiefer:** No?

**Herzenberg:** [02:20:37] Not that I—I mean, you can count them. There are a lot of awards. But I don’t know. What have I received awards for? You know, Eleanor Roosevelt Fellowships, these are just fellowships. They’re awards, but they’re nothing so special. The Smithsonian Awards are very nice, and that has to do with the relationships that I developed with the SAS Institute, work I had done with them.

When Len received the Kyoto Prize, he argued with them, “Why don’t you give it to Lee as well? Because we really did this together.”
And they said, “No, we only give it to one person,” and they gave it to Len.

Len was almost going to turn down the award, to show people he really meant that we did this together, which we did. But I told Len, “Don’t be silly. How can you turn that down? You go. It’s OK.”

Well, when he gave his acceptance speech, which for once I did not write with him, he repeated that he felt I should have been awarded the prize with him. Then, he argued the point with the Americans group that presents the Kyoto Awards again in San Diego, a few months later.

Len won on this one. The group has a banquet and a meeting here where they normally re-award the prize to the people who initially received it in Kyoto. This time, they gave the award to both of us, had me give an award lecture in addition to Len’s, et cetera.

This was not actually theirs to do, but they did it. Len told them that he just did not want to accept the award here. He didn’t have to accept it here, and he said he didn’t want to accept it here unless they included me, so they did.

Kiefer: So the Kyoto Award is something that’s rooted in Japan?

Herzenberg: [02:22:14] It’s rooted in Japan, yes. It’s a very big award in Japan, and the American version of it—they did study it. It was Len who brought it up to them, and they studied it out, and they decided, yes, it was correct that we should be both given that award.

So, those are all the awards. To me, the awards are the science and the stuff we found. Len has always actually been on my case that I did not
seek out awards. He said, “Why don’t you join this committee? Why don’t you do those things?”

And I said, “I’m busy. Leave me alone.”

I love what I’m doing. I can’t tell you how much I love the science. Last night was such a triumph. I hung out with the guys till about nine-thirty, and then they said, “The rest of it is all boring.”

So Erin, who’s my youngest graduate student, said, “Come on, I’ll take you home.”

And Gigi was starving by then, so I just said, “OK, I'll take Gigi home and give her some food.”

Kiefer: And Gigi is that cute little white dog.


Kiefer: You were interviewed in the Annual Review of Physiology last year by one of your ex fellows…

Herzenberg: [02:23:38] Mario.

Kiefer: By Mario. And he was a postdoc of yours?

Herzenberg: [02:23:42] He was a postdoc.

Kiefer: Mario Roederer.


Kiefer: Towards the end of the interview, you said, “The purpose of all findings is to be overturned. Interactions among people, those will stay forever.” Can you elaborate on that?

Herzenberg: [02:23:58] Did I say that?

Kiefer: You did.
Herzenberg: [02:24:00] Oh, how beautiful. [laughs]

Kiefer: I thought it was.

Herzenberg: [02:24:02] That’s a great statement. Mario—that was a great interview. It was fun working with Mario. He has just put a thirty-color FACS machine. He’s made the next advance in the game. I want Mario’s machine, and I’m just going to get one immediately. [laughs] It’s that kind of thing.

Well, it’s true. Einstein’s theory: Wayne is now looking at things—maybe someone else will replace Einstein’s mathematics with something which is totally consistent with it, but will overturn it in the sense of being the final word. There is a better word that could be had. If Wayne doesn’t do it, somebody else will.

Every science finding is only as far as man’s mind can take it when you’re doing it. It can’t go further. As you learn new stuff, you get new perspectives. You’re always finding new stuff that goes on.

Human interactions also change, but they don’t overturn. They don’t go away because you have a new science finding. Maybe you can say that was wrong. I guess you could say there were interactions you have that were wrong, but the human interactions are for us now the thing that is the important part of our life. I think I’d still stay with that.

Kiefer: Is there anything in your time at Stanford that you would have done differently?

Herzenberg: [02:25:55] Oh, probably lots of things. [laughs] Yes and no. Maybe I would have agreed with Len a little more that I should have fought for some more stuff. I realized, something that he realized, that when he died or when he
stopped, I would lose a lot of stuff because we had balls in the air that we were both maintaining. There were a lot of people who wanted to say it was Len alone who did this and Lee just hung on. No. But you couldn’t say that it was Lee alone who did this and Len that—it was both of us all the time.

But, yes, things got lost that were honors that came to him. He regarded as being honors for both of us, and that was enough for me. But when he's gone, those honors are gone. So maybe I would have fought harder and done the things that you need to do to get the honors myself. As it was, I did them for Len to get the honors for him, because that was enough for me. We just took the most judicious trip. And it never bothered me not to get honors. It still doesn’t bother me not to get honors, but I see what honors are valuable for, and I maybe realize that more so since he died. Maybe I realized this before he died, but it didn’t seem important enough then to matter. [laughs]

Kiefer: Sure.

Herzenberg: [02:27:33] We’ve got this new stuff that’s coming in. It’s going to get us everything we need and more so. This is going to be a phenomenal year, and I just wish that Len were here to enjoy it with me.

Kiefer: Yes. So you’re the head of the lab now?

Herzenberg: [02:27:48] I’m the head of the lab now.

Kiefer: OK. And finally, you were going to say something about Medicare.

Herzenberg: [02:27:55] Oh, yes, of course. 1961, Bob Mishell was a fellow of Hal Holman’s. He came from a medical family but still a kind of a lefty family, Jewish left, in Los Angeles, -- not red but left-ish.
I don’t remember how we met up with Bob. But at some point, this was during the big Medicare fight, the original one, the argument of the Medicare opponents was, “Your doctor doesn’t want you to do this.” And Bob said, “Well, my father wants you to do this, and other doctors I know want you to do this. This is really terrible that they’re saying this.”

So think it was the AMA [American Medical Association]—I some branch thereof—ran an ad on the local classical music station. The ad said, “It’s really nice when a Boy Scout helps an old lady across the street, but if she doesn’t want to go across the street, he shouldn’t be helping her.” It was a really an obnoxious anti-Medicare ad.

Bob said, “You know, I met that guy who runs that station that is running the ad. He’s a friend of my father’s. I’ll call him up and ask him why he is running this horrible ad.” So Bob called up the guy at the station -- I remember him sitting in the lab and doing this. I can still see his back.

Anyway, after calling the guy at the station, Bob puts the phone down and he says, “He didn’t want to run that ad. When he heard the ad, he took it off that air, and it’s cost him $15,000” which at that time is like 150k or more now.

The Bob said, “This is really too bad that he’s lost money. We should get together and raise a little money so that we could put an ad on his station and show him and others that if they do something responsible like putting a good ad on for Medicare, that there’s a community that will support this.”

Len and I said, “Yes, that’s great. Let’s do it”
We got together and we went to see Dan Feldman, I think, who was a doctor of physical therapy at Stanford, I think. We went to see him and asked, “What should we do?”

In the end, we all decided we should have a lecture, and charge money and collect whatever we can from the house. We’ll pass the hat around.

Well, Dan put us in touch with Philip R. Lee, who was the son of Russell Lee. Now, Russell Lee started the Palo Alto Clinic. That was the first group-practice plan anywhere in the United States, as far as I know, and it was anathema to the AMA. They hated it because they didn’t believe in group practice at the time. Isn’t that funny now? [laughs]

It turned out that Phil Lee happened to be my physician, so it was kind of a small little world. We got together with Phil and told him what we wanted to

Phil put us in touch with another patient of his named Jack – I forget his last name. Len would remember his name. That was his job. [laughs] Anyway, Jack was the science reporter for the Examiner, and he was the third top science reporter in the country. He was really good.

We said, “Well, we just want to raise some money.”

And he said, “Phil is behind you. That means the Palo Alto Clinic is behind raising some money for this and for saying we’re in favor of Medicare.”

By that time, we had done what everybody did at that time, which was to start a petition and pass it around the doctors and the medical
school. “Are you a doctor who believes in Medicare? Because the AMA was saying the doctors don’t want you to support Medicare. We need to some percentage of the doctors here?” That was the BACMA, the Bay Area Committee for Medicare Acceptance, and in the end we got 25 percent of all physicians in the country to sign that petition.

Jack started it by saying, “OK, if you want to get this to go, here’s what you have to do.” He was a first-class publicist, and he said, “I need big names.” So we went to Josh Lederberg and Arthur Kornberg, and they signed the petition. Hal Holman and his parents, both physicians in the old Stanford Medical School. And we found there were a bunch of other friends, and they all signed the petition.

Jack ran. This big two-inch-high typed headline, “Doctors Revolt,” in the Examiner, and that headline spread through the country. We ran this whole committee, and for several months our life was getting this petition signed, getting more people to support Medicare.

Ultimately, Bob went to Washington with our petitions and brought them to Lee Bamberger, who was then working for the AFL-CIO, and she was running the Medicare thing. She took them to Congress, and those petitions were crucial in proving to Congress that doctors were not opposed as the AMA said. The AMA did not speak for the doctors. It was all over the country, and we got Medicare.

What we did was a very, very important part of the game, and it all started because Bob Mishell made that phone call. He was a great guy. He became a professor at Berkeley and an immunologist. He had a medical
degree and he had a medical residency, but he really liked basic science more, as did Henry Wortis, who was working in Hal’s lab at the same time. It was a really interesting place.

**Kiefer:** Well, I want to thank you so much. Certainly if you think of anything else that we have not discussed that you feel is important to the story, let me know.

**Herzenberg:** [02:35:01] I’ll think about it. I’m glad I thought of the Medicare. [laughs]

**Kiefer:** Yes.

**Herzenberg:** [02:35:06] It was important. It really was important.

**Kiefer:** It is, and I want to thank you.

[End of May 21, 2014 interview with Leonore Herzenberg]
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