

OREGON HEALTH & SCIENCE UNIVERSITY

ORAL HISTORY PROGRAM

INTERVIEW

WITH

Marvin Rittenberg, Ph.D.

Interview conducted January 29, 2014

by

Mary Stenzel-Poore, Ph.D.

Narrator: Marvin Rittenberg
Interviewer: Mary Stenzel-Poore
Date: January 29, 2014
Transcribed by: Teresa Bergen

[Begin Track One.]

Stenzel-Poore: My name is Mary Stenzel-Poore and I'm interviewing Marvin Rittenberg for the OHSU Oral History Program. The date's January 29, 2014, and we are in the BIC Building at OHSU.

So, Marvin. Let's just start with some facts. Where were you born and raised?

Rittenberg: I was born in Los Angeles and raised in Los Angeles during World War Two.

Stenzel-Poore: Wow. I think a lot of us would like to know how you got interested in science. What were some of the beginning steps that got you there?

Rittenberg: Well, it's difficult to say. I started off as an econ major at UCLA. And I hated it. I didn't know anything about it. And I managed to get an A in the final grade. And I thought if I can get an A, knowing what I know, which is nothing, this is not for me.

And I got a job at the UCLA Medical School when I was an undergraduate, washing glassware in the microbiology lab and cleaning animal cages. And I made pretty good friends in that animal place. And I just got interested in what was going on. Finally I got to wash dishes and make media. And so I knew what, found out what was going on. It was interesting. And that's how I started.

Stenzel-Poore: So it was that experience of really helping in a lab and seeing what people do in a lab—

Rittenberg: Yeah.

Stenzel-Poore: --that turned you on to science as a career.

Rittenberg: It did. At least, I think so. I blame it on that.

Stenzel-Poore: Some people find that it was their childhood that fostered that.

Rittenberg: Yeah.

Stenzel-Poore: But it can be an experience just like—

Rittenberg: Yeah. I knew nothing about science when I went to UCLA. And I had written an article in the high school, in, I guess it was a literature class. Anyway, the teacher thought it was terrific. So I thought well, I can be a writer. That didn't work out too well, either, when I got to UCLA.

Stenzel-Poore: Well that's kind of a great perspective because you are a very good writer. And—

Rittenberg: Well, I realize that now. But that's because I deal with facts, more or less. But I really thought I was good. And my very first experience in English literature class, which was beginning English at UCLA, the guy who was a teaching assistant didn't like me and I didn't like him. Anyway, I knew I wasn't going to be an English major.

Stenzel-Poore: So let me ask you a question about that. Because I think in science the ability to write can trump a lot of the other things that people think are important in science. Because you have to convey what you're thinking about.

Rittenberg: That's right. And I made a note about that. About grant writing, which we can get to later. But in fact, if you can't write a good grant, in addition just to the ideas, if you can't put them down on paper so the reviewers can understand them and appreciate what you're saying, it's hopeless.

Stenzel-Poore: Yeah. I agree. So just continuing on that little trajectory that we were on, you went to college at UCLA.

Rittenberg: Yes.

Stenzel-Poore: Tell me how you got to deciding I want to do a PhD and where.

Rittenberg: Well, I did it at UCLA. But in between, I got married and was drafted. So I was in the army for two years. And I was a much better student when I got out of the army than I was when I went in. I was sort of flighty, I guess, in being a student. I would never claim I was a good student as an undergraduate. I was much better as a graduate student.

Stenzel-Poore: So did the military influence that?

Rittenberg: Well, not directly. I worked at the Food and Container Institute for the armed forces in Chicago. And the guy I worked for was a very nice guy. He was a civilian. And we were studying fat oxidation, which I knew nothing about. And I learned a little about it. But it's a tragic story, in a way, because he was fired. It was during the McCarthy era. And he was fired because his brother or his father, I don't know, had somehow or other had some association with communism. According to my boss, he didn't have anything to do with it. But he was fired. I came to work one day and this man was gone.

And his boss came in and said, "We need a progress report from you on what you've been doing, the lab has been doing, for the last year."

Stenzel-Poore: And you had to—

Rittenberg: I wrote it. Yeah. And he said it was very good. That's all I can tell you. But it was traumatic.

And I once ran into that guy. I remember now, his name was Morrie something. But he, of course he left working for the military and opened a private laboratory in Los Angeles where I saw him once. He was a very nice guy. That was a shame. That's my one experience with McCarthyism.

Stenzel-Poore: Yeah. Well, it had a penetrating effect, didn't it?

Rittenberg: It did. Join nothing.

Stenzel-Poore: So you went on to get your PhD. Let me ask you this. Was there a particular scientist then or later in your career that had a significant impact on who you were yourself as a scientist? Or what you decided to do in science?

Rittenberg: Well, Eric Nelson was my mentor in graduate school. My second mentor. My first mentor was Meridian Ball. And she wasn't a great scientist, but she was okay.

And somehow I got the idea, and I've been trying to remember how I got onto it. But I decided if I went out into the Los Angeles Harbor and sampled bottom fish, I could find out something about pollution in the bottom of Los Angeles Harbor.

Stenzel-Poore: Hmm.

Rittenberg: So, yeah, it was interesting. I went out on, she arranged for me to go out on a fish and game boat in Los Angeles Harbor. I got immediately seasick. And that was the end of that project. I just couldn't do it.

So I worked with her for a while on leptospira, which is a pathogen which is now an important one. It started out in rodents and rats. But people get leptosporum infections as well. Excuse me, leptospira. You better edit that. It's leptospira, not leptosporum. I don't know what that is. Probably a mushroom.

So I worked on that. I published one paper with a guy named Dean Linscott who went on to, he was a post-doc, a graduate student ahead of me in another department. And we collaborated on showing that a particular kind of filter could filter out leptospira. And at the time I guess it was important, I don't know why it was, but it was.

So I worked with her for a couple of years. But then I had made friends with a guy named Eric Nelson, who was a faculty member in the department. And I had met him because when I first got out of the army, I didn't immediately go to school. I worked running the chemistry supply department or office or whatever it was for the biomedical department at, I think, I'm trying to remember what it was called. It was biochemistry department. It had another name. It must have had a medical associated with it. I don't remember. But I worked there. And somehow or another I met Eric. And when I started graduate school, we just hit it off. I liked him. He liked me reasonably well. And he was in immunology. So that's how I got started in immunology.

He eventually left the medical school and started a company called Nelson Research, which made him a multimillionaire. But he didn't hire me. I didn't ask him to either, but that's beside the point.

But I do remember him saying, "I'm leaving in six months. If you're not done, that's your problem." So I got done.

Stenzel-Poore: You hurried up.

Rittenberg: Yeah. Yeah.

Stenzel-Poore: So that was probably in the '60s?

Rittenberg: Yeah. In, I think I actually got my PhD in '62. You must have all these dates down.

Stenzel-Poore: I don't. I guessed.

Rittenberg: Well, you guessed right. I took a job at the VA Hospital in San Fernando Valley, where I worked, I stuck it out for about three months. But I realized that the VA and science didn't hit it off.

So I had written a review while I was a graduate student that was published in a national journal. And a friend of Eric's, Werner Braun, who was a bacterial geneticist, also interested in immunology, at Rutgers, and he expressed an interest in having me as a post-doc. So I went to Rutgers and was a post-doc for a couple of years.

And then I ran into Dan Campbell. Dan Campbell was professor of chemistry at Cal Tech, who had been recruited by Linus Pauling. And Dan Campbell and I hit it off in a conversation at the federation meeting, I believe. And he asked me if I, or I asked him, I don't know, if I could be a post-doc. And he agreed to it.

So I went to Cal Tech and was a biochemist, or a chemist, as they called it, or immune-chemist, for three years. And from Cal Tech, I came here. I think I could have stayed there a few more, there were a lot of senior scientists, or world famous, who had been at Cal Tech for 20, 30 years. Never had an academic appointment. They just lived off of grants. So I assumed I could have done that. Dan liked me. But I decided I needed to earn a living and have a regular appointment. So I came to Portland.

Stenzel-Poore: Well, that was a pretty important period for you, though, I think. Those three years with Campbell at Cal Tech.

Rittenberg: Oh, they were. Yeah.

Stenzel-Poore: And at that time period in immunology.

Rittenberg: Yeah. And I always used to say that you could be the dumbest guy at Cal Tech, and people would think you were a genius just because you were at Cal Tech. So it was really very good. And I could have stayed on, I don't know how long, but for a while.

I even got to meet Linus Pauling once. I shared a lab with one of Linus Pauling's research fellows. And the only thing I remember about that was he once said that he would go in and show Linus Pauling his data, however he had it. And Linus Pauling would say, "Oh, this is what it says." And rearranged it all. And suddenly it made sense to everybody.

And I've never forgotten that story, because in a way, when you're a mentor of a student, that's sort of what you have to do. You have to help them see what they've got. And hopefully you see it first, but not always. You get good students, they see it long before you do. You can tell them that. But I won't go on.

Stenzel-Poore: So I want to ask a couple of questions that have to do with being a scientist in that time period. Because I think you particularly have an ability to comment on what it was like to do science, essentially from 1965 until 1980. And if in your mind it was actually different from '80 to '95. How was it different?

Rittenberg: That's a good question. Because everything was phenomenological, at least in immunology. You know, you described, what we published about was the antibody response to a particular trinitrophenyl but you know, you would speculate about how the response came about and what was involved in it. And that went on for years and years. And in T cell immunology, cellular immunology was a field at that point. And, well, it still is, I suppose, but didn't mean much.

And then the molecular revolution occurred. And when I started, I mean, we knew about DNA. But we didn't know anything about how DNA acted. At the time, I think I was still a post-doc at Cal Tech when the structure of DNA was described by Watson and Crick. And that just changed everything. Once we knew that DNA was the basis of all biology, really, and what its structure was, that just opened up a whole spectrum of things that could be done.

And when I started out, I was known as an immunochemist. And then I started to be called a cellular immunologist through my first couple of grants. And then I realized I needed to be a molecular biologist. And that's where Mary came along. Because she said she wouldn't work in my lab if you couldn't do molecular biology. So I sent her to someone else's lab to learn how to do molecular biology. And that changed everything. Because then we had a structural basis for knowing what was going on and questions we could ask. And the difference was amazing. So that began, I can't remember when you started. But it was early 1980s, I think.

Stenzel-Poore: It was. It was about '82.

Rittenberg: Yeah. Yeah. So that made a big difference in the kinds of things that were known about immunology. At that time, as I say, it was all cellular. You put T cells in and something happens. How does it happen? Who knows? You inject a hormone. How does it happen? Who knows? And the same thing in all aspects of immunology at that time. So just the beginning of knowing what was involved, structures that were involved, and how they might interact has transformed the whole thing.

I think immunology is still a growth industry. But the difference nowadays when I read something about immunology, I can barely understand it. And I've only been retired for ten years. So it's just amazing.

Stenzel-Poore: So that tells you something. So I like the way you describe that, that period when you started out was very observational.

Rittenberg: Yes.

Stenzel-Poore: And then in the '80s and '90s, I think we think of it as reductionist. We really did try to dissect every part of it. And now things are different and we're trying to assemble it.

Rittenberg: Yeah.

Stenzel-Poore: So, technology. I want to come to technology because I think of the way you approached sciences. As technologies became available, you were very prone to using them. To me, it seemed. And I want to reflect for a minute and comment on how you think technology could influence what you did scientifically.

Rittenberg: Well, it had all the influence of the world. Because I was, frankly I think I was running out of phenomenological things to describe. You can only describe them so long. I might have had to move in a different direction in immunology. I don't know. Because I'd never stayed in one place in that sense. So maybe I would have done something else. But thank heavens I was rescued by molecular immunology. And I've never regretted it.

I think it took me 20 years to learn how to be a scientist. And once molecular immunology became my tool and I could say I was a molecular immunologist, I really started to feel that I knew what I was doing, and that my research was better. And I've always been lucky in having good students.

Stenzel-Poore: So that's a great place to segue, actually. Because I have some questions about that. About, well, first I want to tie one thing, I want to ask you, before we go to students, what do you think was your greatest contribution to immunology?

Rittenberg: Well, I could say building a better mousetrap. Because Cathy Pratt and I developed a plaque assay for trinitrophenyl (TNP). Which is a small chemical molecule that I'd worked with at Cal Tech with another post-doc, Alfred Amkraut. And we published one or two papers on it, I don't remember. But I worked with that.

I came to Portland. And about that time, another scientist whose name I can't recall published a paper where he could take—well, there were two papers published. One was a plaque assay. They showed that if you put red blood cells in a gel of, some semi-impermeable gel, and then put lymphocytes on top, the lymphocyte would release antibodies, and the antibodies would lyse the red blood cells. So around each lymphocyte that was producing antibodies to red blood cells, you could identify it. This is the cell that's producing those antibodies.

And about that time, as I say, I can't remember his name now. But this guy published a paper in which he said that arsanilate, another small chemical molecule, could be attached to red blood cells. And he could do this plaque assay with lymphocytes that were producing antibodies to the arsanilate. And I recognized two things at that point. One, that not a lot of people worked with arsanilate and a lot of people worked with what I was working with, TNP. And so if I could produce antibodies, or produce lymphocytes that would produce antibodies to TNP, I'd be ahead of the game.

So Cathy Pratt and I, Cathy was my technician who had worked before in the hematology labs and knew something about red blood cells. I only knew that they were round and red. And concave.

So we spent a long time, but we developed a technique for attaching trinitrophenyl (TNP) to red blood cells. And then we could put those TNP red blood cells in an agar matrix. And then put lymphocytes on top. And find antibodies that were producing antibodies against TNP. Not against the red blood cells, but against the trinitrophenyl, small molecule.

So that, I suppose that's my biggest contribution, simply because it was quoted thousands of times. I really don't know how many.

Stenzel-Poore: You think it has been. It was an enormous contribution.

Rittenberg: Yeah. So I built a better mousetrap. And people had plenty of mice to catch with it.

I was complaining to somebody that well, they didn't cite me because they claimed that they had developed this. They hadn't.

He said, "Well, we would have developed it if you hadn't done it."

And I once heard that same argument used in an NIH study section against some guy whose grant I had reviewed and thought was terrific. And this guy on the sidelines said, "Well, if it was that good, everybody would already have done it." It was a nonsensical argument, but that sort of thing goes on in science.

Stenzel-Poore: Well, I think that that discovery of how to do the plaque assay and be able to quantify responses in that manner contributed to your thinking a lot about how immune systems and immune memory responses happen.

Rittenberg: Well, it did. And my first graduate student, Wesley Bullock, published a paper in which we said that we could follow the anti-TNP response, the anti-trinitrophenyl response, using this plaque assay, through the course of immune response in mice. And it was a very nice paper. And we published it in the *Journal of Experimental Medicine* at the time. So we could do that.

And at some point along the line, we were doing more cellular—

[End Track 1. Begin Track 2.]

Rittenberg: --immunology with TNP. But I realized that the antibody response of phosphorylcholine, which is another small molecule, could be a better tool. Because the antibodies, the majority of antibodies to phosphorylcholine in the mouse are called T15. And they're all encoded by a single gene, VH gene, and a single light-chain gene. And since those genes have been cloned and were known, I figured this could be a powerful tool in itself.

And at that point, you have, I assigned Sandra Chang, who was another graduate student, to develop an immune response to phosphorylcholine, which she did, and a plaque assay to go with it. And Sandra published several papers, quite a few, in fact, about the immune response of phosphorylcholine in a mouse. And among other things, she found out that some different light, different heavy chains were being used. And one of those was called VH1. And Mary Stenzel-Poore appeared on the scene.

And so I was able to clone that gene. Indirectly. And I still remember when you showed us on a screen at a lab meeting in the department that you had cloned the damn thing. And it went right over my head. I didn't know what the hell you were talking about. Until you pointed it out to me afterwards.

But that was a big step. Because eventually you cloned the VH gene of VH1. And we studied that for a lot of years with a lot of stuff.

Stenzel-Poore: So, Marvin, you've referred to this a couple of times that you've had bright students to work with. I think there's a magic formula. And I want to know what kind of thoughts you have about what the magic is.

Rittenberg: Well, I don't like to say it, but they have to be girls. And she knows that. I had a couple of male graduate students. But really, by and large, my female graduate students were really super. And I think they found me because of my magnetism. But none of them ever indicated that that was the real reason. But I think it's because I treated them as I would treat anyone. I made them work really hard. And I expected them to always be working as hard as they could and as smart as they could, and to use their imaginations. Because imagination is part of being a scientist. If you don't have any imagination, you're doomed. You just can't do much.

And they all had good imaginations. They all worked really hard. And they only would hate me on occasions. It was fairly brief. I would say, "Well, you're not working hard enough." They hated me for a minute. But it didn't mean anything.

And I think that's the reason the graduate students that I solicited didn't turn out so well. It was the ones who found me and what I was doing was of interest to them. And I think that's the reason that I was successful.

Stenzel-Poore: I think that, speaking from my own experience, I think that's true. That what students could see in you was that you believed that they could make a contribution. You expected people to make a contribution. And that combination is, it's very inspirational. And you get that. That's what you get in return by expecting it and empowering it. So I think that's what you just described to us.

Rittenberg: I think I may have said that, yes.

Stenzel-Poore: That's my perspective. So I have a couple of questions about the history of the department, just to get a perspective. Who recruited you to OHSU?

Rittenberg: Bernie Pirofsky. And Bernie was an immune-hematologist in the Department of Medicine. And he was in charge. I don't know if they really called them chairs, if they only were chair of a division. But he was the chair of the Division of Immunology in the Department of Medicine. And he was the one that I went to work for.

It was Art Malley who, and Alfred Amkraut, who were two post-docs at Cal Tech when I got there, who came to Oregon and went to work at the Primate Center. And they knew, I think they had joint appointments in the bacteriology department. And they knew that Bernie was trying to hire someone.

Stenzel-Poore: And that was in--

Rittenberg: 1966.

Stenzel-Poore: 1966.

Rittenberg: Yeah. And Pirofsky only told me two things about why, well, one thing about why he hired me. I seemed to be relatively smart. And the other guy he interviewed only wanted to do what Pirofsky wanted him to do. And I didn't want to do anything that Pirofsky wanted me to do.

And it worked out very nicely. Bernie was a very nice guy. The day I arrived, he and his wife had already left on sabbatical in Mexico for a year. So we moved into their house, which was a beautiful, big house. More than we could have afforded. And we lived there for a year.

At the same time, he turned over his connection at the Oregon Crime Lab to me because he used to do the blood typing in criminal cases or parental custody cases. So I started doing that. I didn't know anything about it, but Cathy Pratt was able to teach me enough about red blood cells to get started. And it was interesting.

The crime lab would come and ask me to investigate red blood cells they found as blood stains, and what not. And I testified, I may have told you this, in two murder trials. My record was 100 percent. I lost them both. In one, I testified for the defense, and the guy was convicted. In the other, I testified for a prosecution and the guy was acquitted. So those were the two I did.

Stenzel-Poore: Maybe that's why there were only two.

Rittenberg: I think it was. Yeah. They made me feel good by saying, "Well, it could have happened to anybody."

But I remember going down to Klamath Falls where, that was the one where I testified for their prosecution. And some guy had, I don't know exactly, I don't remember the details, but somehow or other the blood from the victim was found in his truck. And I identified the blood in his truck as being from the victim. And I thought oh, this is simple.

And they brought this defense lawyer out. And I was sure he was this hick from the sticks, he wouldn't be able to shake me on anything. He demolished me. He knew every right question to ask about the frequency of blood tests being correct, and just all kinds of things like that. It was a good lesson for me in humility. I don't know that I needed any more humility, but I certainly had got it then. That was pretty good.

I was a joint appointment in bacteriology, it was called, when I started. But when Jules Hallum came in 1967, no, seventy-something or other, I wrote it down, because I couldn't remember it. In 1973. I came in 1967. In 1973, Hallum was hired to replace the original chairman, Chick Frisch. I should tell you, his name was Arthur. But his wife, everybody called him Chick. And his wife told us this story once that his nephew from someplace in the Midwest met him and thought his name was Chicken. So he called him Uncle Chicken.

Stenzel-Poore: And it became Chick?

Rittenberg: Yeah. Yeah.

Stenzel-Poore: That's good.

Rittenberg: Yeah. So at the time I joined the department, I wrote down some names because I knew I'd forget them. I think there were some that Alan Meyer told you about. Lyle Veasey, Evelyn Oginsky, and also someone named Ted Joys, who was a bacterial geneticist and Charlie Gardner, who was a virologist. And the other ones you heard of from Alan Meyer: Frisch, Lyle Veasey, Evelyn Oginsky.

And then, about that time, well, when I joined, shortly thereafter, Barbara and Wally Iglenski came. And we hit it off very well. We got along and we had fun together as a group. My wife and I and our kids and Barbara and Wally and their kids.

And then Frisch resigned. And something sad about Chick Frisch was that he once, they had a get together for him at the library to give him a toast and whatnot. And he said to me it was the first time in all the time he'd been here, which was maybe 15 or 20 years, first time he ever felt he belonged. And I thought that was really pretty damn sad. And said something about the school. I don't know what. I never felt that way.

Anyway, they hired Jules Hallum, who was a virologist from Tulane. And when he came, he brought Gerrie Leslie, who was an immunologist, with him. So there were two new faculty. And at that time, they were building a new basic science building. Which I think is now called the Jones Building, or Dick Jones Building, or something like that.

Stenzel-Poore: Richard Jones Hall. That's right.

Rittenberg: Jones Hall. Yeah. And I remember at the time, I was complaining because I didn't think it gave enough space to the microbiology department. And the guy that was in charge of that committee came to visit me in my lab, I was still over, my office, I was still over at Mackenzie Hall. Still with the Department of Medicine. I said I was supposed to go on vacation, but I'll stay so I can be here for the site visit.

He said, "Don't bother." He didn't want any, he didn't know what I would say to a site group.

Stenzel-Poore: [unclear]

Rittenberg: I might have. Who knows?

So then Hallum came. Charlie Gardner had already gone. Lyle Veasey had retired. I don't know why Charlie Gardner resigned. I can't remember if I did know. And he got rid of Ted Joys. For some reason, he didn't think Ted Joys fit into the kind of department he wanted. He was a very smart bacterial geneticist. But at any rate, he left.

At that time, the new basic science building was built. And I moved over and in the process got promoted to full professor, which was good. And then we started recruiting new faculty. And that was Leslie Hallick, Steve Morris, who's a bacterial physiologist. Katie Richardson who was, I guess, a bacterial physiologist. George Crosa, Miles Wilkinson, Scott Landfear and Eric Barklis. So they all joined the department.

Katie Richardson left during that time. And I think, I think Miles Wilkinson replaced her, although he wasn't really a bacterial physiologist. I'm not so sure that she was, either. But at any rate, so she left.

And the department existed like that until Maggie So came. And in the, during the interim, Jules went on two sabbaticals. And I was acting chair. Which was, I did as little as possible and it worked out well.

And then when Jules retired, he went to NIH as the ethics czar, which was bizarre. And I again acted as acting chair until Maggie So came. And when Maggie came, she brought with her Fred Heffron, Jay Nelson, and then recruited Ann Hill, David Johnson and David Parker. So the department was really good.

She only made one mistake, in my opinion. And that was that she let, she forced Miles Wilkinson to leave. And he went to MD Anderson. Had a very successful career for a while. And he's now an endowed professor at University of San Diego, UCSD Medical School. I mean, he was a terrific scientist. And I never understood why he was gone. It was never explained. But he wasn't happy and I wasn't, either.

And the department, I guess it has existed until George died. And you've recruited some new people, but I haven't met any of them. So I only know the old guys.

Stenzel-Poore: And Jay Nelson?

Rittenberg: Moved to the—

Stenzel-Poore: Right. Recruited individuals, also.

Rittenberg: Yeah. He recruited quite a few over at the primate center. I don't know what his division is called.

Stenzel-Poore: Vaccine and gene therapy.

Rittenberg: Oh. I looked up primate center on my Google. And when I got to primate center, I couldn't find that there.

Stenzel-Poore: They're separate.

Rittenberg: I see. Yeah. Anyway, so that's basically the history of the department as I knew it. I wasn't too popular with Lyle Veasey. Because I started off volunteering to be one of the faculty in their bacteriology laboratory. And I just wasn't very good at it. I didn't know a lot of bacteriology anymore. I'd been an immunologist too long. And I always had to stop and say, "Well, I don't know. Let me ask one of the other faculty members to answer your question." It just seemed fruitless for me to be there.

Stenzel-Poore: So you had an impact, though, in helping to advocate for building immunology as part of the microbiology/immunology focus.

Rittenberg: I did. Yeah, and I did have an influence on, I think I was the deciding vote on offering the position to Leslie. I don't remember who else we had as a candidate. And Jules was on, I think he was on sabbatical when I, George always gave me credit for recruiting him. So I guess I did. I suspect Jules had started it, because George was up in Seattle. But I guess I was involved in Crosa and Wilkinson and Landferr and Barklis. I know I was heavily involved in their recruitment.

And the people at the VA developed, eventually developed a strong immunology program. Originally, it was Dennis Burger. And then Hinrichs came over. And I can't remember his first name. You probably, you could.

Stenzel-Poore: Dave.

Rittenberg: Dave Hinrichs. Hinrichs. Hinrichs. That's it. And they recruited quite a few immunologists, a lot of whom are still there. And immunology has continued to do well at the VA. And those people do have, some of them have appointments in the micro department. And I was always enthusiastic about the more immunologists could be hired all over the campus, the happier I am. Because there are people you can interact with that get strength in your own program.

And I should tell you, I've got two grant stories to tell you. The first one was, the first grant I ever wrote when I got here was about the role of the FC in immunoregulation. And they said it was a good grant, well-reasoned, but everyone knew that the FC had no relationship to immunoregulation.

Stenzel-Poore: Things have changed.

Rittenberg: Yes, haven't they. So it was rejected. And I never resubmitted it. But I kept that in mind.

And the other one was when Barbara Iglewski and I submitted a grant. I can't remember what it was exactly. It may have been immunology. It may have had to do with bacterial toxin. I honestly don't remember. But I remember we got a review back that said it was a good grant, strong, well-reasoned. But there was no institutional support for it because it involved a lot of molecular biology. And we weren't molecular biologists, and they knew that. But as a result, it couldn't be funded. And that was the last time that ever happened. Excuse me. It brings tears to my eyes. Because right after that, the Vollum was built. And with the people they got at the Vollum, good molecular biologists, neurobiologists, we never got another, I didn't, I assume Barbara didn't, either, we never got a grant back that said there wasn't institutional support. It just made all the difference in the world, in the whole school. So even though I didn't publish papers with people at the Vollum, I could get them to write letters saying they could help me if necessary on whatever aspect was involved. And everyone does that nowadays. But when I first got there, that was impossible.

So I, you know, that's one of the ways that the place evolved. And I remember Jules and I met with President Laster. And we gave our undying support to the idea of creating the Vollum Institute. Some of the departments, they wanted to get rid of Laster right away. They did not want that institute built. And we were all for it. We got to interview, and I was on the search committee for the first director of the Vollum. And it was just, you knew that the place was going to be so much better once that Vollum Institute was built. And staffed with really good people. And it turned out to be true.

Stenzel-Poore: We're in the top five every year for funding in neuroscience at OHSU.

Rittenberg: That's amazing. I didn't know that. But it shows my wisdom when I suggested you just go work, or go down to the Salk Institute and specialize in neuro, whatever it was, neuro something or other.

Stenzel-Poore: Well you'll probably also be pleased to hear this, that we were number one in the nation in microbiology funding. At OHSU.

Rittenberg: Really?

Stenzel-Poore: Yes. Yes.

Rittenberg: Not just the department. The whole school.

Stenzel-Poore: Right. So it's really about how much funding you have in the area of microbiology and immunology. And they track essentially what comes to the department.

Rittenberg: Yeah.

Stenzel-Poore: That's how they track that.

Rittenberg: Yeah.

Stenzel-Poore: So grants flow through the department, even though they're [unclear] situated around campus. And this last year, we were number one. And it was a little bit surprising to me. Some of that is that we have several people who have just been stars in getting large grants in the area. People like Louis Picker, and Jane Austin, yours truly, Mary Stenzel-Poore, and Mark Slifka all were—

[End Track 2. Begin Track 3.]

Stenzel-Poore: [unclear]

Rittenberg: Way up there, yeah.

Stenzel-Poore: Way up there for big grants. And that's what influences those numbers. But it also speaks to the strength that's been built since you were here over that time. You built immunology, you built microbiology. There's this really strong substrate of faculty that work in this area.

Rittenberg: It's too bad, I'm sorry Jules isn't alive. He would have loved to hear that. Because he was a strong advocate for his department, his faculty. He really took care of his faculty. Made people all over the campus angry about one thing or another. But when it came to his faculty and the department, he was just really strong.

Stenzel-Poore: Yeah. That's what it takes. I'd like to ask a couple of things about academic life. And there's a very different feeling about academic life now where big problems are approached by teams of scientists. And that's different from when you and your colleagues were doing the bulk of their science.

Rittenberg: Yeah. In fact, my whole team of scientists, whatever graduate students and post-docs I had in the lab, nobody else. And I know that, I recognized that fact sometime before I retired. And I wrote something for somebody, I don't remember what, about the idea that it was necessary to develop cross-cultural ties between scientists in other departments, in other divisions, to help strengthen the school. But I don't know who I wrote it for.

Stenzel-Poore: Well that's an important point. And that is a transition that science across the nation is making. I think the recognition that their problems to work on are complex and technologic. And that requires a team of--

Rittenberg: Yeah.

Stenzel-Poore: --people who come from a variety of disciplines in order to tackle the problem. So the training of scientists is actually influenced by this because we have to train less about individualism and more about how you work on a team.

Rittenberg: Yeah. Yeah.

Stenzel-Poore: And my question in this is, did you have some insight into this? Because I believe you trained people to work in that manner in your own lab.

Rittenberg: Yeah. I think I did, in fact, I think people were impressed by the fact that I got David Peyton, who's an NMR spectroscopist at PSU to collaborate with us on part of our study on our antibody molecules. And it's clear that people in other departments can contribute if you know how to reach them, how to contact them, and let them know why their specialty, whatever it is, could contribute to what you're doing. In turn you could say something to them that might help them in some way. And I felt that for a long time. I always went out of my way to get people in various disciplines to help us, like David Peyton at PSU, and Hans Peter Bächinger at the Shriners Institute because they had a particular expertise in protein structure that I felt we could use. And he collaborated. We didn't do any studies together. But he wrote one of those important letters. And I always had three or four of those letters attached to every grant that I submitted. And I got to be pretty good at grant writing after a while because I learned what to do and how to do it. And it didn't hurt that I was on several study sections at NIH and American Cancer Society that let me learn how to write good grants.

Stenzel-Poore: If you could do it over, if you could do it over and pick your career back then, what would you—

Rittenberg: Way back when, at graduate school?

Stenzel-Poore: Yes. Would you have done something different?

Rittenberg: Yes. I would have done bacterial genetics. Because even at that time, they were just learning about the, well, they weren't learning at the time I started about the genome and the structure of genes, DNA and RNA, I recognized that they had a lot more solid stuff in their background than I did in cellular immunology. And in fact, while I was at Cal Tech, Lee Hood was just publishing antibodies with his boss, whose name was Dwyer, I think? Gee, I can't remember his name now. But anyway, they published several papers on the structure of antibody molecules, the sequence of antibody molecules. And I knew then that that was the place to be. But before that, just bacterial genetics. I thought, if I can work with, there was guy there named Bob Romig, who was really a good bacterial geneticist. And he was a faculty member. And we

developed a friendship because we used to have coffee together and talk about the old *New Yorker* magazines that were always in the coffee room. And I just thought he was a really smart guy. And the experiments that he did were good. So I would have done that.

Stenzel-Poore: Bacterial genetics.

Rittenberg: Yeah.

Stenzel-Poore: Well, there have been some Nobel Prizes that have gone in that direction.

Rittenberg: I know. Yeah. Probably not to me, but anyway.

Stenzel-Poore: Could have. Yeah.

Rittenberg: Who can say, right?

Stenzel-Poore: Okay. So if you could do it over, you would pick that area. What if you could pick now?

Rittenberg: Now.

Stenzel-Poore: And you were a young graduate student. What advice might you give, or what might you choose if you were in their shoes?

Rittenberg: Well, I still say that immunology is a growth industry. But I think really that structural neurochemistry is a very important field. And it's going to get better, probably. People are learning about the structures of a lot of neuro-hormones and other neurochemicals. And what they do and how they signal. So I'd say cell signaling is an area that I would stress, I think. Unless all the signals are done, but I don't think they are.

Stenzel-Poore: You mean brain synapses and how transmission takes place—

Rittenberg: Yeah.

Stenzel-Poore: --and the circuits.

Rittenberg: Gee, you always say it better than I do.

Stenzel-Poore: Well, I'm thinking with [unclear]

Rittenberg: Yeah. Yeah. So that would be my recommendation. That's it.

Stenzel-Poore: That's it. Okay. I want to ask just two more questions. One is, where did your family fit into all this? How did you balance the family—

Rittenberg: Well, in some ways, I balanced it poorly. I can't remember how many family vacations we had to cut short because I had something professional to do. Review grants for a study section. Do this, do that. I came back to work virtually every night, five nights a week. I came in on Saturdays. I frequently came in on Sundays, especially early in my youth. And I think that, in a way, impacted negatively. But every now and then, one of my kids will say, "I remember when you said such and so." And of course I don't remember it at all, but it influenced them, whatever it was. So I guess they're okay. They're both successful in their own way.

And my wife was amazingly both resourceful and independent. So if I wasn't around, she'd manage to deal with everything pretty darn well. And still does, because she deals with me every day of the week. And if there's anyone who deserves to be credited with my 82 years, it's my wife. I mean, she just sees to it that I do all the things I need to do to take care of myself.

So I'd say that it impacted my family, but I can't say that it was all negative. And certainly we took three sabbaticals. Some people don't take any sabbaticals, which I consider dumb.

Stenzel-Poore: Where did you go on your sabbaticals?

Rittenberg: All three of them, we went to London. The first time because I wanted to go to the University College with Av Michison, who was really a really brilliant immunologist and had a department of, I think it was called tumor immunology. Why it was called tumor immunology, I don't know. But there were a number of really good immunologists there. Really good. So that seemed like a good idea. And since I had salary money and he didn't have to pay me, and could make lab space for me, we went to London.

And after the first time, my wife had such a great time in London, she must have wandered every shopping street in the city for all kinds of foods, everything under the sun, she wanted to go back. And we both like the theater a lot, and the theater was always superb. We like museums. So it was hard not to go back a second and third time, although I did consider going other places. But in the end, we could speak the language. And it was just a good experience all the way around. And I found out, the first greatest discovery was probably fire. And then the wheel. And the third was underarm deodorants.

The first time we went to London, they didn't have underarm deodorants. At least, for the people who rode the tube. Which is what I rode. And on a hot day, it was horrific. It was pretty darn awful. By the time we went back, that was in '71, '72. By the time we went back, our next sabbatical, which was—

Stenzel-Poore: In the '80s.

Rittenberg: Yeah. I think it was '81, '82. They'd discovered deodorants. And it was a much more pleasant ride on the tube than it had been the first time. But London changed a tremendous amount. England changed a tremendous amount. Because the first time we lived there, they were just giving up their colonies. And there was a huge debate going on in the English papers. Because a guy whose name I can't remember now, who was a member, a conservative member of Parliament, I believe, who complained that if they allowed all these people from the colonies to come in as citizens, it would totally change the country. It turned out he was right. Whether you think it's a good idea or a bad idea is beside the point.

But by the time we went back in 1981, '82, the whole country was different. Because the number of immigrants was really large. And it continued to grow. And all the people from the colonies who wanted to be citizens could be citizens of Britain, and come and develop there. It's created all kinds of problems. Every now and then you read about a racial incident in England. They're much worse than they are here, I think. Probably because we're so dispersed. It was an interesting time. And we enjoyed it.

Stenzel-Poore: So your kids had the exposure, which was wonderful.

Rittenberg: They did. My daughter loved it. My son hated it because he couldn't play baseball in England. And he had to play soccer. At first the coach of the soccer team thought he was great because he could dribble. I don't remember which side, probably the right side, was terrific. But he got to the other side of the field, he couldn't dribble at all with his left foot. Or the other way around. I don't know how they do it in soccer. But I remember that. And he thought the class was too hard. It was a typical, at that time what they called a public school. And he had to wear a tie and a jacket and a white shirt, a uniform, to school every day.

My wife, not my wife, my daughter went to essentially a junior high school that was much different. And she loved it. She thought it was great. And developed an accent that was typically Lower End London. But they made good friends, I think. It was interesting.

After that, we went on our own. They stayed behind.

Stenzel-Poore: So I have these great recollections of your wife, Joan, being a fantastic cook. And you would have these wonderful weekly meetings at your house that you called Journal Club.

Rittenberg: Oh, yeah.

Stenzel-Poore: Can you talk about that from the perspective of your students coming? Your students and post-docs, and what that was as a learning experience or a teaching experience?

Rittenberg: Yeah. I think it was more, yeah, it was a learning experience for me. Because they could cover journals that I never got around to, and would tell me about papers that were of interest to the whole group. But it began with just two students. Wes Bullock and Karen Korsmo Vigeland, who's now a dermatologist in Vancouver. She went to medical school after she got a master's degree.

But they would come to my house every week and we'd talk about articles they read. And they'd eat cookies that my wife baked. And that was pretty nice. And we continued it then for a long time after you came. Until I had one post-doc who was difficult to deal with, who claimed that he was allergic to our dog. Even though our dog was never in the room, but the dog had been in the house. So we had to stop meeting at my house. Although, that was Tony.

Stenzel-Poore: Yeah, I forgot about that.

Rittenberg: But I think he was really allergic to discussions of molecular biology. Which he hated and didn't want to know anything about it. Anyway, I should have said, "Well, you stay home and we'll have our meetings continue." But I thought it was really worthwhile.

Stenzel-Poore: Wonderful. It was a wonderful experience. The students and post-docs learned so much from those experiences.

Rittenberg: Yeah.

Stenzel-Poore: And as you said, you learned. And it was a time to relax a little bit and be in a different environment that felt very comfortable. And it was great intellectual exchange.

Rittenberg: Yeah. I thought it was very good. And because it was so relaxed, it made it very easy for people to pipe up and say whatever they had to say. When we switched to having them in a regular meeting room, it was not nearly as spontaneous. And I felt it never accomplished as much as I wanted it to. But one moves on.

Stenzel-Poore: So one other small thing that I think is worth talking about, you alluded to having a leadership role in the department.

Rittenberg: Yeah.

Stenzel-Poore: Whenever the existing chair went away and you would play a leadership role. And you said you played it, you did as little as possible. I don't think that's entirely true. So I wonder if you could talk a moment about what your role was and how you took that role on.

Rittenberg: Well, I saw to it that we didn't have faculty meetings every week. We only had them, I don't know, maybe once a month. I can't remember. But I know I had served on so many committees at the university and found so many of them to be a waste of time. So my philosophy was that each person got to say whatever they had to say once. But you didn't go back and say, "Oh, he wanted to say more." And then they'd repeat themselves and repeat themselves.

And I remember Eric Barklis once congratulated me on the fact that our faculty meetings were succinct, and didn't go on and on.

And I tried to accommodate everyone. But I told them when I started each time that I can't change anything. If you don't like your lab, wait till the chairman gets back. If you don't like your office, wait till the chairman gets back. And don't tell me that you want this because some other guy has it, whatever it might be. Because that won't work, either. And if everyone's doing it and you're not, that's your problem, but I'm not going to deal with it. And people seemed to accept that.

Stenzel-Poore: So you, it was, you deferred the hard questions.

Rittenberg: Of course.

Stenzel-Poore: But you were democratic in the ones that you could actually solve.

Rittenberg: Yeah. Yeah, I think so. It's so often at faculty meetings people just want to say over and over and over again what they've already said. Whether everybody likes it or dislikes it is

irrelevant. They've heard it. And they'll go off muttering to themselves about whether or not it was a good idea or a bad idea, unless it's something you have to vote on.

When I was acting chairman, there wasn't much to vote on. Because I wasn't going to change Jules Hallum's policies. And while we were waiting for a new chairman, I certainly wasn't going to introduce new policies. I couldn't recruit anybody. So it was irrelevant.

Stenzel-Poore: Okay. So last question, you stayed at OHSU for how many years?

Rittenberg: I came in 1967 and I stayed until, I retired the first time when I was 65, which was, I can't remember the date. But it doesn't matter. I stayed until, well, if I started—hell, you add it up. In '65, or '67, rather. Then I retired the first time when I turned 65. And they had a big party for me and a dinner besides, and it was great. And Peter Kohler, the president, gave me my gold-colored watch. And almost the next day, my NIH grant got renewed for another five years. So I said, "This works out well." And I worked for another five years, until I was 70. And when that grant expired, I knew I had to go. Because I just, I didn't have enough good graduate students to write another grant for me. So it was time to go. I really, I had trouble keeping up with the literature. I had trouble seeing what I was reading for a very long period of time. It was tough. So it was good.

So now, I've been retired 12 years.

Stenzel-Poore: Twelve years. Wow.

Rittenberg: And I'm still grumbling along.

Stenzel-Poore: So do you still read science?

Rittenberg: I read science in the *New York Times*. I can't read the journals anymore. It's just too difficult.

Stenzel-Poore: Takes an active, you've got to keep up.

Rittenberg: Yeah. Yeah. Besides which, I don't like reading a lot of stuff on the computer. I find that very difficult.

So I do that. And I go to the gym five days a week.

[End Track 3. Begin Track 4.]

Rittenberg: Some guy told me I was a gym rat. I told him no, I was a gym mouse. But that's about it. It's a pretty good life. And I have no complaints.

Stenzel-Poore: Good. Well I think I don't have any more questions. I think that was wonderful.

?: Is there anything else you wanted to add, Marvin, or any questions you wish we'd asked?

Rittenberg: No, I don't think so.

?: Great.

Rittenberg: No. I get rumors, occasionally, down at the gym. But I'm not going to go into those.

Stenzel-Poore: Best not.

Rittenberg: Best not, yes.

?: All right.

[End Interview.]