Gallo, Maria 2008

Maria Gallo Oral History 2008

NCI Laboratory of Molecular Biology

Oral History Project

Interview with Maria G. Gallo

Conducted on November 6, 2008, by Jason Gart

JG: My name is Jason Gart and I am a senior historian at History Associates Incorporated in Rockville,Maryland. Today's date is November 6, 2008, and we are in the offices of the National Institutes of Health in Bethesda, Maryland. Please state your full name and also spell it.

MG: Okay. My full name is Maria Grazia Gallo. I use G rather than my whole middle name. It is M-A-R-I-A—G—G-A-L-L-O.

JG: Terrific. I want to briefly describe the interview scope. Established in 1970, the Laboratory of Molecular Biology, Center for Cancer Research, National Cancer Institute, National Institutes of Health, commonly known as LMB, currently has among its ten groups, four members of the National Academy of Sciences. LMB has trained many other prominent scientists and its researchers have contributed both to basic science and to novel applied cancer treatments. LMB has initiated this oral history project to capture recollections of prominent scientists currently and formerly associated with the laboratory.

You were born in Audubon,New Jersey, in 1946? Talk about some of your interests as a child.

MG: Well, I always liked reading. I was never athletic and I can't say that I had a particular interest in science or literature or anything like that. I enjoyed reading and I read appropriate to my age, of course, whatever I could get my hands on.

JG: Explain a little about your family background. What did your parents do?

MG: My father has a college degree from Villanova [University] in electrical engineering and he worked all his professional life for RCA. During World War II he worked on radio communications for tanks and was based in the U.S. with RCA. Later he worked on communications with NASA and on the moon program. He developed radio technology for the LM [Lunar Excursion] Module and that sort of thing. My mother was a housewife, homemaker.

JG: Did your father encourage your interest in the sciences?

MG: Well my father was sort of an interesting character. He thought that beyond high school girls went to college to get married, to find a husband. So I had an interesting time. One, getting into college, and then he dictated that I was either to go into medical technology or into nursing or teaching. I had to go to a college close to home. I went to Rutgers University in Camden,New Jersey. I started out in a med-tech program and the summer between my freshman and sophomore years I was fortunate to have a job in the microbiology lab of the pathology lab at the local hospital. After one summer I thought, "I can't do this all my life." It was too routine. I became a biology major, of course, unbeknownst to my dad. I graduated with a bachelors degree from Rutgers in biology. Of course I wanted to go onto graduate school which started a whole other round of things but I did get a scholarship, or a fellowship and stipend, at Georgetown [University]. I did get my microbiology Master's degree at Georgetown.

JG: Did you have any high school teachers that were particularly influential?

MG: Well, I did like my high school biology teacher, high school chemistry teacher, and one or two of the math teachers. By then I knew that I was not going to be in languages or literature or any of the liberal arts. I did not enjoy those kinds of classes but I did enjoy the math and the science in high school.

JG: How about mentors in the biology department at Rutgers University?

MG: I had a couple of good ones. In biology in particular was Dr. Henry Stempen who was really the force that drove me into what I was doing. Then in biochemistry I think his name was Dr. Sheiner but I am not sure.

JG: You attended Rutgers between 1964 and 1968?

MG: Right.

JG: What was it like to be there in the mid-1960s?

MG: Okay. First of all it was a commuter campus of the main college so literally it was a commuter school. There were no dorms. You could not stay there. There was no quad. Classroom buildings were in row houses that the university had bought. Before I left, I do not remember if it was 1966 or 1967, somewhere in there, RCA turned over a manufacturing warehouse that was along the Delaware River to the school and we had classes in there. There was a brand new science building and library that we had access to, but it was not a college campus per se. City traffic still went through campus. I rode to school with my father every day. He worked in the same city, in Camden, so we rode back and forth together.

JG: You mentioned that there were three different career paths—

MG: That he would approve of.

JG: Talk about why you choose biology. What interested you in biology?

MG: I don't know. I am not sure what got me into science but obviously in high school that is where my interest was. I do not have patience to be a teacher. If you do not get something the first or second time I explain it I just can't deal with it. Never could. [Laughs] Even helping my sister with her homework. Never could. If she did not get it right away I got frustrated. So teaching was out. Nursing again was not something I was interested in doing. Taking care of sick people, it just did not appeal to me at all. The biological sciences were a great place to be. Now, I am not an animal hater, I just am not really fond of animals. I do not like to feel them moving in my hands (like mice) which is why I went into microbiology. Microbes do not bite, they do not scratch, and they do not pee on you. [Laughs]. So that is how I got to microbiology.

JG: You mentioned briefly that you worked at the Cooper [University] Hospital in Camden. What was that like in the summers?

MG: Very interesting. I was in the bacteriology lab and there was a chemistry lab, a blood bank, and whatever. The woman who was heading the bacteriology lab, who was essentially my mentor, since I did not know anything about this, was completely deaf. She read lips and would explain everything. She had an absolute understanding of her field and was truly a teacher. So you enjoyed what you were doing—you were learning. She made sure you did it until you got it right and you understood all the principles. Her only rule in the lab was that you could not chew gum because she did not know if you were talking or chewing gum. [Laughs] She was a dear. She was a dear lady.

JG: How long did you work there?

MG: Four summers.

JG: You decide to go to Georgetown University?

MG: Yes.

JG: What caused you to leave Camden and move to Washington,D.C.?

MG: The fact that I got a fellowship and stipend. I applied to several schools, and Georgetown came through with the funding. It is a lovely area and I also had an aunt living in D.C. at the time. I was actually moving away from home for the first time.

JG: How did your parents take that?

MG: Well, it was okay as long as I had my funding and I was okay. My dad did agree to cosign the lease on an apartment for me. My first apartment was $90 a month in 1968 in Georgetown. The micro [microbiology] department was very small. There were only four professors originally and we were part of the medical school. We were sort of off to one side of the campus and did not really have that campus attitude. I lived off-campus. There was no graduate student housing at the time at least connected to the medical school.

JG: When did you decide that you wanted to go on to graduate school? Was it a conscious decision that you made?

MG: Yes, in the sense that I did not think I was finished learning for one. I wanted to specialize in micro and get out of the gross biology. Other friends were going on either to graduate school or to medical school or veterinary school. My circle of friends was all going on. It was sort of a prod to me to keep going. When I entered Georgetown I entered in a Ph.D. program but after two years I decided "I think I am sick of school. Let me get out; get a job for a while. If I want to come back fine; I'll see what happens." Also at the time there was a glut of Ph.D.'s in the biological sciences so I thought I had a better chance of landing a job with my Master's degree.

JG: When did you first learn about the discovery of the double helix by Watson and Crick?

MG: I am sure it was at college classes.

JG: Okay. So you leave Georgetown in 1971?

MG: I actually graduated in 1971, but I finished in 1970, and came right here to LMB.

JG: What brought you to LMB?

MG: I was looking for a job. The chairman of the micro department at Georgetown, Dr. Arthur K. Saz, knew Ira, called him, and said I have somebody who is looking for a job: "She is very good." At least that is the way I hear the story. I came out here for an interview and Ira and I hit it off right from the first interview.

JG: What was your first impression of NIH?

MG: I knew it had this world-famous reputation. I had not been out to the campus before. I knew Ira's name from some of the coursework I had done and reading within the field. I was amazed at how big it was. Are you from this area?

JG: I am from Philadelphia.

MG: You have not experienced the seventeen-year locusts that are down here?

JG: No, I have not. [Laughs]

MG: Okay, the summer I was here to interview for my job was one year they came out. My first impression, it is still with me, is of walking around campus. I had to go, Ira was in Building 10 at the time, from an interview in Building 10 with Ira, over to Building 31, where the old administrative offices were. I had to walk through the campus with these locusts falling out of the trees on me and clinging to my clothes. That is an image I will never forget, and I have been aware of it every time it has happened since then. [Laughs] I had never experienced it before coming down here.

JG: That would leave a memory with me too.

MG: Yes. It is a weird icky feeling. It was a beautiful campus and everything I had ever heard about it was top notch. It was a great place to be when you are in the area and you want to be in science.

JG: How did Ira describe what the lab was doing at that time? How did he describe what was going on here?

MG: You know I do not really remember that interview. One of the things I remember is I walked in and he said, "We are not formal. Call me Ira, not Dr. Pastan," and that sort of bowled me over. He was really asking more about the research that I had done for my Master's thesis. He told me a little bit about what he expected from me, that he had technicians in the past, that he was not sure he wanted another one. Apparently some of them did not work out. He said, "I'm willing to give you a try." You know, that sort of thing. He told me that he wanted a technician who was willing to do what he wanted done when he wanted it. He knew that I had been in the Ph.D. program, I believe, so he said I do not want you coming here thinking you have a Ph. D. You are not going to do independent research. That was sort of where things were and that was fine. At the time that is what I was ready for. A job with money coming in and somebody to direct me. [Laughs] I was impressed with Ira and his approach. He was very personable and friendly and welcoming.

JG: How did your professor at Georgetown describe Ira?

MG: I don't recall. I think Ira was the one who told me that the department chairman had called him. I do not believe I heard it from Dr. Saz at all.

JG: What were your impressions of the laboratory and the work that was being done there?

MG: Interesting. Shortly after I started in the lab Ira had me go to work with Max Gottesman to learn some new techniques, because I came from Georgetown with certain techniques, and he wanted me to learn others. I spent the first three or four months that I was here working under Max. I do remember that the first laboratory he assigned me to, I was to share with Benoit [de Crombrugghe], whom you just interviewed, and with Beatrice Chen. There were three desks in the room and lots of counter space. Benoit was the kind who would come to work mid-morning and work late into the night. I had to be at work at 8:30 am. I would come in the morning and my desk and work area were completely covered with all of Benoit's stuff. So I would spend the first hour cleaning it up and I would be getting myself set up ready to work and he would come in and never said a word. I would clean up at 5 o'clock when I went home and the next morning we did the same thing over and over again until I found a space in another person's lab that was empty and I started to set up in there. Then Ira kept trying to send me back in with Benoit and I kept moving out. [Laughs] Eventually we moved to Building 37 and so that ended that. I was in Building 10. I guess we came here, to Building 37, in February of 1972. So it was not that long.

JG: What was biology like in the early 1970s? What were the techniques like for you?

MG: Well, we did a lot more biochemistry back then, a lot more enzymatic assays. We did a lot of work with radioisotopes. One of the things I learned to do with Max was to radiolabel phage DNA. It was very hands-on. There was very little animal work going on. Certainly Ira was just starting to get into animal work and built an animal lab shortly after I arrived. Reverse transcriptase, which is an enzyme that really began the field of molecular biology, was not discovered until 1975. So it was a few years before the field of molecular biology took off. So biology at that time was really more biochemistry. The immunology part of it was very black box. You injected an animal with something and you got an antibody back out but none of the processes were known.

JG: Describe Ira and his research techniques, and Max and Benoit de Crombrugghe, and some of the other people that were there. Describe them as scientists.

MG: Well, they are all very intense.

JG: How so?

MG: Ira was in every morning at 9:00 or 9:30 am. He was there until 6 o'clock or later every night. He was in on Saturdays, sometimes Sundays, and he always wanted to know exactly what you were doing, what I was going to do for that day, and we would discuss specific experiments. If I did not have results by the end of the day when I went home I had to have them ready for when he appeared the next morning and we went over them. Incredible memory. He remembered numbers—raw data off of a scintillation counter—for years. I don't remember doing the experiments and he could quote numbers off the machines. Max also very intent. Benoit, very intent. They lived, breathed, ate science, as Ira did for many, many years. The others left the lab after a while and I lost track of them but Ira is still that way.

JG: What was the atmosphere for you and the other technicians? What was it like to be a technician for these intense people?

MG: Okay, how far back do you want me to go? [Laughs]

JG: Start at the beginning.

MG: Okay. I was the only technician in the beginning. There were about six or seven postdocs, Ira, me, and we had a woman who would do dishwashing and run the autoclaves and things like this. For the first few years it was just me as a technician. I must say Ira directed every minute. As the years went on and the lab grew to the huge size it is now . . . First we moved from Building 10, to Building 37 where we had part of a corridor in the building, and at that point Ira did set up a tissue culture lab and hired a technician. Again he was very directive of her work like he was with me. We got along well, she and I. We sort of kept each other going as far as keeping the lab going in supplies and whatever, although the tissue culture was very, very separate from the rest of the laboratory. Then as time went on more people were made permanent, more sections were made in the lab and section chiefs were appointed who got to hire technicians. It was pretty much up to the section chief as to how he/she dealt with his/her technicians but everyone managed to get along together. There were very few people that ever caused a ruckus in the lab. [Laughs] It was always a good group, no matter whether you were a technician, a secretary, whatever, you were always treated with respect.

JG: What was it like to be a young person at NIH in the 1970s?

MG: Well, I am really sort of an introvert. I never went out looking for people or things to do or whatever. I was really attached to LMB and the people in LMB. I was impressed and a little bit cowed by some of the Ph.D.'s, but I learned about that too. Some of them did try to direct me in what I was doing until Ira would step in and say, "You work for me, not for them. When they pay your salary, we'll see." That sort of thing. I really did not go out into the NIH community. I really do not know what else was going on. I stayed pretty much with LMB and into my own job.

JG: I guess it was interesting because you got to do research all day. Were there exciting "Aha!" moments for you in that period, during the 1970s?

MG: That is hard to answer because I feel like at least the first ten years I was in the job I was still learning. I might hear Ira say . . . I have to admit from the very early days Ira trusted me and my work so I would present him with data and he might be doing the "Aha!" kind of thing and explain it to me. Those first ten years or so I can't say I had the "Aha!" moment but he did. Then we would talk about it and talk about where to go from that point. I was not allowed to do individual research, remember this. I worked for him, I did what he wanted, when he wanted it. Many times I did not see the whole picture. I might be doing work along with another postdoc who was exploring a different aspect of the same project and all that data would get funneled to the postdoc. It did not always come back to me.

JG: You mentioned that he trusted your results. Speak about that. How did you earn that trust?

MG: Well, I think because he kept a close eye on everything I did. He examined every experiment and we discussed the experimental procedures to use. We discussed the data. Let's face it you never do an experiment once. It is two, three, four times just to make sure. Well you might alter it slightly, add something extra to it, but you are always sort of redoing your assays. After a few times you keep getting the same results and you don't look for excuses as to why this worked on day one and it did not work on day three. I think that automatically builds up the trust.

JG: What did you learn from Ira? I can imagine that his scientific skills were probably pretty impressive and the ways he constructed his experiments were very creative.

MG: Yes. He taught me the value of controls especially in running experiments. You can never have too many controls. Put in as many as you need, as many as you think you need, plus a few more. It was a matter of writing everything down. Don't try to keep anything in your head. Take columnar paper and write down that tube number one is going to get this, this, and this and how much. Tube number two is going to get this, this, and this. Make sure you number your tubes according to what your paper says. Really the basics so you don't get yourself confused. Apparently one of his earlier technicians before me was Israeli and she was labeling everything from right to left. He had another one who apparently did not label tubes at all and it was always very confusing about what she or he was doing so that Ira was not happy. I learned early on to do exactly what he told me. [Laughs] You both end up better off in the long run. I really felt in some way I was the student all along. I just got more and more comfortable with what I was capable of doing as time went on.

JG: You mentioned the change in technology. In the mid-1970s you have the recombinant DNA controversy. How does that affect the lab, and also, how do these techniques start to change your day-to-day activities?

MG: Well it is sort of interesting for me because actually into the mid-1980s most of the work I did was really biochemical, enzymatic assays, doing a lot of protein work, running gels. I sort of came into the molecular biology field from the side. Ira had a couple of postdocs working on projects in the field of molecular biology and they needed extra hands so I sort of learned from them the techniques that I would then take . . . Like if they were on the second floor I would be down there working with them but I might take everything upstairs to my lab on the fourth floor to finish up and run gels. It was a matter of protein gels versus DNA and RNA gels, learning all the various techniques that you needed to use. Proteins were a lot easier to handle than nucleic acids. You had to treat all your equipment, plastic boxes and tubes, and everything with sodium hydroxide to kill any enzymes when you were doing nucleic acid work. It wasn't an issue so much with protein work—the biochemical aspect. I kind of came into molecular biology from the side. I was never trained in it. I was already working when the reverse transcriptase enzyme was discovered. So the little molecular biology I know is from things I picked up in the lab working there directly with Ira or one or two of the other postdocs.

JG: You mentioned before the relationship with the postdocs. What was that relationship like for you? Did you consider going back for a Ph.D. or going to medical school?

MG: Well, yes. I did but late, late, late into my career with Ira. The postdocs were interesting and as I said in the beginning I was a little cowed. They were a little bit older than I was at the time. I would say that after about ten years I began to think, "You know, these young whippersnappers are coming in here and they think they know it all." After a few more years they were of an age where they weren't even alive when I was already working with Ira, and here they were trying to tell me what to do. In general, I think most of the postdocs respected me. I got to the point where I realized, "You put in your time, you got your degree, you might have a little bit of book knowledge, but common sense is what is needed here." This sort of thing. They just suddenly lost their luster, Ph.D.'s or even M.D.'s, for that matter.

It was in the mid-1990s or later then I started thinking about getting a Ph.D. That was sort of interesting. Sankar Adhya was on the adjunct faculty at George Washington Universityand he encouraged me. He got me in to see the head of the micro department at George Washington University. We worked something out and what Ira agreed to was that I was going to do all my courses part-time. I was continuing to work here in the lab. I would take evening courses, occasionally they would have to be in the daytime, and do research projects part-time as part of my duties. Ira just got impatient about how long the research projects were taking. After I changed my project two or three times he said, "Look you have to have enough by now to get a Ph.D. thesis." I thought I am not even going to talk to the people at GW because I know it was not enough for a Master's thesis. We just let it go. I have done all my course work. I have passed my oral exams but that was the end of the Ph.D. and I have no desire now to finish. I knew I could never go back and get my degree earlier and come back as a postdoc. I could not afford to live that way. It was more for my own benefit, and feeling good about myself, but it is okay.

JG: Talk about how LMB changed in the 1970s.

MG: From the 1970s?

JG: From the 1970s through the mid-1980s. It gets much bigger?

MG: Right. It gets bigger—

JG: And Max leaves?

MG: Yes. So many people have come and gone in my thirty-five years with this lab that I do not remember who left when.

JG: What are some of the projects that are going on in the early 1980s?

MG: You know I really don't know. Again, I was so into the work that I was doing that I do not know what other sections were doing. I mean I know Susan was doing a lot of work with bacteria, put it that way, also into molecular biology. Ira was now working more in tissue culture and animal work which is what I was doing. Very little molecular biology at the time. There was Susan and I don't remember when different people came. I think Benoit was doing molecular biology before he left. I do not really know what was going on in the different . . . Ira would have data meetings with his own lab group once a week, and those I attended, so I know a little bit what was going on there, but not so much with the other sections. I just could not follow it all and there were techniques I had not heard of, I didn't know anything about, and it was just too much. I needed to concentrate on my area of the world. [Laughs] By then I did not have the title, but even by the mid-1980s I would say I was sort of a lab manager for Ira's section, so there was a lot more than just science in my position.

JG: Talk about the weekly meetings. What are they like? How do they change over time?

MG: Okay. Well again as the group got bigger it got harder and harder to hear from everyone every week. So Ira might choose, like the last few years that I was working, two or three people a week to present what they were doing, which was good because then you could see—let's say John spoke the first week of the month and it was six weeks later before he spoke again—what John did over that six-week period. It was much better than hearing one or two days' experiments, at least to me. Ira always knew what was going on with everybody but it was good to hear a piece of work being presented.

JG: Ira has more than a thousand papers?

MG: It is a big number, yes.

JG: What was it like working for someone that was so productive?

MG: Well again, he had so many people. The lab just kept getting bigger and bigger and bigger and he is on so many papers as senior author, some just because they came out of the lab. His sections got larger and larger and larger. All his postdocs were expected to do work that would result in paper or papers before they moved on. So everybody worked intensely. Again everybody was here late in the evening and people were here on weekends.

JG: I mentioned it before and am not sure if we covered it. Did the recombinant DNA controversy impact the lab in the 1970s?

MG: You have to explain the controversy a little more.

JG: There was concern that people working with recombinant DNA technology could create things that could become harmful to humans. Some worried that bacteria could be constructed that might then escape from the laboratory.

MG: Like a superbug or something?

JG: Yes, exactly. As a result NIH banned certain types of recombinant DNA research and required the use of special rooms and other precautions.

MG: Right. Yes there was this recombinant DNA committee or section. Actually Susan Gottesman was either on it or headed it or both.

JG: Right. Did that impact your work at all?

MG: Again not early on. Later into the late 1980s and 1990s when the committee got going it had rules and regulations, they got tougher and tougher. We did not really have a problem in what we were doing. We had to start filling out all the paperwork and submitting everything to committees. If questions came up from the committee we would respond to them. It was not really anything that stopped us from doing what we were doing. What happened in the mid-1970s, I can't answer that. I can only talk about the mid-1980s on.

JG: How about a failed experiment in the lab? There are many successes but how do you deal with an experiment or hypotheses that doesn't quite work out? I assume you are always getting useful information.

MG: Well, what do you mean by don't work?

JG: If an hypothesis doesn't pan out.

MG: If it doesn't pan out?

JG: Right, you may have spent months working on something—

MG: How do I answer that? You can sort of keep doing the same experiments over and over again, and keep thinking that these results have to be wrong, these results have to be wrong. Sometimes you realize that you have designed the experiment wrong to show what you want to show. So just by redesigning it, I am not saying you are going to get the answer you are looking for, you could straighten out the cloudy answer or whatever. Sometimes you just have to throw up your hands and say, "Well obviously I am going down the wrong path. Let's back up five steps and start in another direction." Not everything is successful in that you have a new drug developed at the end. All of the negatives, or what you called failed experiments, are leading you in steps to the right direction. So are they really negative? Are they really failed experiments? They might be negative but are they failed?

JG: You mentioned some of the other technicians. Were there social activities among the technicians? Did you meet together as groups?

MG: Yes. Part of the problem was that by the 1980s and into the 1990s LMB was split up all over this building, Building 36, and Building 2. It was hard to get to know people you did not interact with daily. So, yes, I knew several of the technicians that were in Building 37. We often all had lunch together, some of us almost every day. We would wait for each other and have lunch. A couple of the other women and I got very friendly, and we would go out to dinner once a week together just to sort of sit and chat or whatever. We didn't want to discuss work. We might discuss people and people issues but never work. [Laughs] So, yes it was easy to get to know people.

JG: Your work also continued to change.

MG: Yes.

JG: You mentioned earlier that you started to take on technical support responsibilities. I also read that you generated a partial genomic DNA library.

MG: I guess so. Gosh, it was so long ago. [Laughs]

JG: And you have also published yourself?

MG: Yes. As part of my work with the lab and work with Ira.

JG: Talk about what it is like to publish in a scientific journal. When did you first see your name in print?

MG: Actually very early on when I did some of the assays in papers that were published. I was never a senior author and certainly never a first author, well maybe once I was a first author. It was fun. It was interesting, but again I was never given the chore, in the early days, of reading through the paper making my comments known. I might be given it for final proofreading before it was going off to a journal. That would be the first I know that there was a paper coming out with my name on it. Later on as I got more and more involved, I started writing Materials and Methods sections, and might write a little bit in the Results section. I would certainly look at all the data that was going to go into the paper. Again I do not know that I ever wrote a whole paper on my own from beginning to end but I certainly had input, and more and more input as time went on.

JG: How did the pursuit of biology change in the 1980s?

MG: I am not sure what you are asking.

JG: I'm sorry. The techniques and the methodologies—were there changes? The use of computers becomes commonplace.

MG: I am still a computerphobe. [Laughs] I learned to do on a computer what I had to do on a computer. I am still uncomfortable just turning one on. I am serious, I have never taken to computers. That is one issue. I did what I had to do and no more. I was never comfortable. Techniques changed some but only because the lab focus went from . . . Ira's sections within the lab went from protein biochemistry to molecular biology. It was still some protein chemistry going on in the 1980s but it was slowly changing over to molecular biology and again my main focus through all of this is with Ira's primary section. Things changed as he wanted to change and the molecular biology became more and more of it. He still had his tissue culture lab and had technicians working in his tissue culture lab who were growing tissues for inoculation into animals and this sort of thing. I was more working on protein as well as nucleic acids making recombinant molecules that were going to be injected as possible, future down the line, twenty years from now, drugs.

JG: This is the immunotoxin research?

MG: Right, exactly, exactly. So again there were a lot of negative experiments. I am not sure they were failures because they did lead to other things. I would not use failure for experiments—it was more negative.

JG: Ira switched in the early 1990s to immunotoxins. How did he describe that switch in his research to you?

MG: Well, we had been working with protein molecules making immunotoxins, working with the toxin portion, as well as the antibody portion, to bind onto the cells. There were problems because the molecules were large, and the chemical processes each had to be put through to have the two proteins joined were causing dysfunction, inactivating the various parts of the molecule. They were very, very immunogenic given their size. With all those problems in the way, and the fact that now we had these molecular biology techniques, we started doing recombinant work, and working at the recombinant DNA level on the antibody as well as on the toxin portion. Our techniques all changed to match what was needed to do that work. I can't say that I was in any way innovative about doing that because again I did not know the molecular biology. It was taught to me and then I learned to manipulate it after I learned the original techniques.

JG: How about funding issues. How did funding change between the 1970s and 1980s? I know during the Reagan Administration that if the NIH lost a position they sometimes could not fill them.

MG: Actually for Ira's lab I am not sure funding was ever a real problem because we were so productive. We had to watch our pennies. Each postdoc and technician was allotted so much money as an overall budget and we certainly had to stay within our budget. I am not sure we ever went over, but we never really had tight budgets to work with. I think Ira was always able to hire the people he needed and wanted. Ira was one of the most, is still one of the most, productive people on campus if you talk about productivity in terms of papers.

JG: Which they do in the sciences.

MG: Right, he and Tom [Thomas A.] Waldman. It has always been one and two, one and two.

JG: What was it like working for the most productive man at NIH?

MG: It kept you busy enough to stay out of trouble. [Laughs]

JG: He is extremely nice. I had the opportunity to interview him and he was extremely nice and very gracious.

MG: Yes. Very much so.

JG: But I guess he could—

MG: Well, you know I have been in the lab almost 35 years. Any long-term relationship has ups and downs. Obviously there were a lot more ups than downs or I would have left. I really can't say anything against him. He treated me well. He helped me get promotions.

JG: You did a lot of work assisting in the relocation of the lab. I read that three years of your life was spent overseeing the LMB move. Walk us through that.

MG: Oh, my. I think I have forgotten some of the more painful things. They renovated this building while it was occupied. They did one floor at a time.

JG: This was because space is such a premium here at NIH?

MG: Right. Building 37 was outdated in terms of its power plant and utilities and whatever. Something needed to be done but there was just no place to move us. At one time they had talked about moving us up to Frederick, emptying the whole building and sending everybody up there. It was just a zoo. They started from the top down. At first they had to move everybody off the sixth floor and then they renovated the sixth floor. Then they moved everybody off the fifth floor. We were on the fourth floor at that time.

JG: They just moved everyone someplace else?

MG: Yes. We fortunately stayed in the building. However I had to make arrangements for everybody. We were spread around the building as each floor was renovated. Where were they were going to go? Equipment had to be moved. All the personnel had to be relocated and space found for storage facilities, freezing facilities, whatever. I worked on this. I worked directly with the architects to design the fifth floor—our half of it anyway. I was involved in the meetings with the architects and with NIH personnel. Ira had done a lot of the preliminary work but I got involved in the nitty-gritty, what color do you want this, where do you want outlets, where do you want lights, what kind of water system do you need, where are we going to put this equipment, where we were going to put that equipment, do you need special outlet requirements. I was involved in all that. And then finally getting us all moved back up in here, and once we arrived trouble-shooting. Ira wanted to go to the sixth floor, and I kept trying to say, "No. it is the first floor they are renovating. You don' t want to go there." Well I can pat myself on the back and say I was right. Thankfully we did not go to the sixth floor because they had all kinds of horrendous problems. They got all new architects and designers in to renovate the rest of the building. We benefited. I think we were in good shape on this floor but as I said I think I have probably forgotten a lot of the painful stuff. It seemed like it would never end and I worked with all the technicians from the other section chiefs helping them design their space and what they wanted to do and whatever. It was not just Ira's lab. It was all of LMB. It was a job.

JG: What year was the move?

MG: I don't remember—

JG: It was sometime in the 1990s?

MG: The late 1990s because early 2000s I remember thinking I have done all this work I am going to stay here at least three more years and enjoy my new lab space. I had designed a cubicle for myself. One side was all desks because as a lab manager I was dealing with budgets, etc., and I had a computer setup for lab work. I had a computer setup for administrative work with budgets and surplus equipment and all kinds of things and then, behind me in my own little space, I had my work bench.

JG: Compare and contrast what it was like when you joined NIH in the 1970s to when you retired in 2005? What were some of the difference between when you first arrived and when you left?

MG: NIH or—

JG: NIH or LMB.

MG: NIH was pretty much the same physical area that it is now, but now has some new buildings. Building 10 has been renovated and enlarged at least twice while I was here. [Laughs] Many of the other buildings on campus have either been renovated or torn down and rebuilt. I was amazed when I came back today. I think I was here two years ago and when I came back today, I could not believe the change in the campus with new buildings, with this new visitor entrance, and the parking garage. I thought I could still get onto the campus even though I would have to go to inspection and come down and park in MLP8 which is right here by Building 37. Everything is up there on Wisconsin Avenue now. I am just amazed.

As far as the lab I can't speak of course for the last few years but we grew from about ten people to more than one hundred during my tenure with LMB. It has been a lot of fun; there has been a lot of hard work. The people have been great. As I said before there are very few that ever came through the lab that didn't get along with people.

JG: We are going to switch gears. Do we have a little bit more time left?

MG: Okay.

JG: I want your opinions on doing big science. Outsiders might think that science is just one person working on a bench. In fact it is not. It is a lot of people collaborating. What is that like for the technicians. Did you get to work with technicians from the other labs that Ira was collaborating with?

MG: Very little, very little. Again Ira might have twenty postdocs and we would all meet at least once a week and everybody would talk about his/her work. I would be included. I would say what I had done. We sort of all knew what was going on as a group. Let's say someone had a terrific result and suddenly Ira is interested and he wants to get a lot of data quickly, I would start working with this postdoc and start doing experiments and assays for him just to augment what he was doing. Or say a postdoc would leave and Ira wanted to pursue his work so I would get involved. I would repeat much of the work that he did to make sure it was all valid and we could build from there. So yes, there were collaborations certainly within Ira's main group. Again I was not involved so much in collaborations with other sections but certainly within Ira's main molecular biology group.

JG: As the lab expanded during those twenty years was there a shift in management style?

MG: Yes, a style of Ira being very much in control of everything and everybody. [Laughs] I mean after a while he dealt with only . . . The lab now has a hundred people and has six or eight sections in it. At some level he has to deal only with the section chiefs because there are just too many people. In his own molecular biology group, and a few of those associated with the clinical center and the clinical trials, Ira is still the man in charge.

JG: I read that you were one of five people that were invited to the Shanghai Institute for Biological Services.

MG: Oh, yes.

JG: What was that like?

MG: That was a blast. It was very, very interesting. I do not know if you know any of the build-up to this.

JG: No, I do not.

MG: Okay. So David Fitzgerald—

JG: Who is a lab chief here?

MG: Right. He brought the pseudomonas toxin with him that we started to work with in building our immunotoxins. He came to LMB in 1980 or so. David was good friends with someone at a company out in Washington state (and it seems that someone at the head of the Biochemistry Institute in Shanghai, the equivalent of the NIH, but in the Biochemistry Institute, contacted this other person) who contacted David and they picked up a third fellow. David was telling me one day that the three of them were going to go and teach this two-week lecture course in Shanghai on how to chemically construct immunotoxins. This is before the recombinant work really got started. This is 1988. I was just kidding around and said to David, "You can't do just a lecture course especially with people who don't speak English. You're going to have a terrible time. You need a hands-on lab course." He just laughed with me, and the next thing I know he came to see me a couple days later and said "Pack your bags you are going to Shanghai. You are going to do a lab course."

I had to put together laboratory protocols on how to do all the things that they were going to talk about in the lectures, and supply all the reagents and all the chemicals and all the accessories like pipettes, test tubes, and whatever. I had to get all that organized together and shipped over. I wrote up protocols for the lab work they were going to do and (remember these are Chinese, people with very little English) everything had to be as explicit as possible with simple words. We get over there and they do not have any equipment. They do not even have deionized water or distilled water in the buildings. So in the evening when I would get back to my room I rewrote the lab protocols based on the equipment they had in the lab to do things. So yes, it was fun. I mean it got to the point where David was actually teaching the lab as the instructor but I was there helping and providing supplies and whatever. It was a terrific experience and I never would have gotten it if I had not jokingly said something to David.

JG: This is still old China in a sense?

MG: Yes, you bet.

JG: What were your impressions? This would have been pre-Tiananmen Square I assume?

MG: Yes, it was the year or two before. We were there in 1988.

JG: So it was the year before Tiananmen.

MG: Right. So it was the year before. We were there at the end of September, early October of 1988. I mean literally I did not realize it at first but we would use pipettes (I brought over automatic pipetters with the little tips and everything for them to use) for something and I would discard them not thinking anything about it. Well, it turned out that they were pulling all these things out of the trash to wash and reuse. They really had to go to another building to get water. You would walk into a nice lab that had lab benches, but there was no equipment, all the cabinets were bare, no chemicals, no equipment, no supplies. I mean I do not know how they did any science at all but there were some very intelligent people coming out of the Institute. The person who invited us actually was Dr. Wong, who was with LMB until he retired. He came to LMB in the early 1990s into a technical position.

JG: During the time that you were associated with the lab a lot of postdocs and other researchers are coming from foreign countries. What is it like working with people whose first language might not be English?

MG: It depends. Some of them come with a great understanding of English. Some of them, their English is so bad and it is a shame. They will not watch TV to learn. They might bring their wives with them and their wives only congregate with other wives from the same country. They do not read the newspapers so the wives never pick up English at all. The researchers here would slowly learn English, but it was painful. I have to tell you, Ira may have told you this story about his first postdoc from Japan. He supposedly could speak English. Well, he arrived and we had a terrible time understanding each other. It turned out he had learned English from a Texan. We were speaking much too fast and, of course, we did not have that broad Texas "A." When his ear got adjusted a little bit he was fine but it was tough getting started. We have had people come from various places with very little in the way of English skills. I don't know if it was NIH or NCI that at some point started English as a Second Language [ESL] for some of these people.

JG: You mentioned—

MG: I have since visited some of them back in their own homes, in their own countries, so that has been fun.

JG: That is interesting.

MG: Yes.

JG: You mentioned this in passing earlier but what are some of the challenges for women in the sciences?

MG: Well, I do not know how to answer that because I have not experienced anybody trying to hold me back because I was a female and I attribute that to Ira. If I asked for something he might not say yes immediately but—at one point I asked for a promotion, and the way that the specs were written for the promotion one needed to do a lot of independent research and Ira said to me "I want you to do what I want when I want it." I understood this but unbeknownst to me he already was working on a way to get me a promotion and within six or eight months I had it. What he did was assign me to work with one of the postdocs and that postdoc was the kind of person who let me do a lot of talking and suggesting experiments, etc., and it worked. I mean I have never had a problem being in Ira's lab. I don't know if I were a Ph.D. now on my own, trying to succeed, getting grants, etc., I don't know. I have just never been in that position as a technical person.

JG: How about the other women that have gone through the LMB?

MG: Again, many of the technicians have stayed with him for years, or their husbands got jobs elsewhere so they went with them. I just don't know. I don't know what to say. Most of the women, as far as I know, who left after a postdoc here were going on to further postdocs or even better positions, and as far as I know, they worked out. There might have been a couple, especially foreign, that Ira could not do anything for because they were foreign, that might have had a problem. Some might have had a problem but in general I do not think there has been too much of the glass ceiling here.

JG: What about the mapping of the human genome? It is kind of a fascinating story. What was it like to be inside NIH and watching that unfold?

MG: I knew it was happening but I thought, "Well, the techniques are all there." They are just plugging the same techniques I know into a machine and letting the machine do the work. I guess it was one of the things that was bound to happen once the technology came out. People wanted their own genomes. Even now they want their own genomes sequenced and whatever. As a technical person I see it as a repetitive process that someone else has worked out all these different techniques for over the years, and whoever is going to do this genome mapping is just going to put them altogether and plug things in. Everything is so mechanized anymore. You plug everything into a machine and your results will come out. So, maybe it is my naiveté, I don't know, but it seems like the whole idea of the PCR [polymerase chain reaction] machine, when you think about it was so simple. Why didn't somebody think of it sooner? I don't know what to say in that respect.

JG: But the person that thought of it won the Nobel Prize? [Laughs]

MG: Yes.

JG: What about the health of science in the U.S. The media often reports that there are not enough students going into the sciences? What is your impression?

MG: Well, again I know what I read in the newspapers, and I guess I agree that is what they are saying. I think everything goes in cycles. When I was in graduate school I had no trouble getting a job as a technical person with a Master's degree. I might have had a hard time getting a postdoc because there were so many doctoral candidates in biology at the time and I think things are cycling. I really do. I am speaking of course in biological sciences. I don't know about the other sciences. Computers have certainly taken over doing a lot of work that was done by hand in science. I think you need the computer technology, as well as a scientific background, anymore to get far.

JG: You are now retired?

MG: Yes.

JG: What are some of your interests and hobbies?

MG: Well, okay. So I retired. My mother was living by herself in New Jersey and was at the point where she could not be alone anymore. We decided we would move together to Massachusetts. I have a brother and a sister in New England. It turns out that we moved to the same town my brother is in because he could be a help with my mother and my sister is only about a three-hour drive away. For the last three years, Mother and I have taken a big trip, one a year. Now she is almost ninety. Her last trip I think she realized she just can't do it anymore, so it was probably our final trip. We spent a few days in Istanbul and two weeks in Greece but she saw Greece from a bus. She just did not have the oomph anymore to do a lot of walking. So our traveling days are probably over. I still want to go to Alaska and the South Pacific and I will get there. Then one thing I always wanted to do while I was working, but never had the time or the space to devote to it, was quilting. So I have started quilting.

JG: Oh, my wife, she is not quilting but—

MG: Crocheting?

JG: Crocheting, yes, thank you.

MG: I started to watch the hand motions. [Laughs]

JG: Her and her grandmother.

MG: Yes, which is something else I like to do but I am really into quilting and I am really enjoying that. Again, mother lives with me, and she sort of decided that she is not going to do anything anymore, I am going to take care of her. I do insist she still get her own breakfast and her own lunch but I do all the cooking and the cleaning and the laundry and whatever. So that keeps me busy too.

JG: That is very nice. Finally, the last question. If you have one piece of advice, one lesson learned over your career that you would like to pass onto a future researcher or scientist operating ten, twenty, thirty years into the future what would that be?

MG: Okay. We need to define our parameters a little more. As a technical person or as an independent researcher?

JG: Your choice.

MG: If it was a technical person I can always say just keep your boss happy and the biggest thing you can do is run controls in all your experiments. That way if there is some point that is out of line or the results are not what you expect you can say the controls are here, these are valid results. I think that would be true of an independent researcher. Always run the controls with your experiment. It will save you a lot of time of repeating and repeating and repeating with experiments that either do not work the way you think they should or just do not work. You know because there are questionable points. I don't know, is that what you want to hear? [Laughs]

JG: That's fine. Thank you very much.

MG: Thank you. This was fun, this was fun.

JG: Thank you.

[End of Interview]