

## **Norma Heaton**

### **Biographical Statement**

Norma Heaton was born on November 23, 1937, in Cleveland, Ohio. She received her B.S. in biology and chemistry from Bucknell University in 1959. Between 1959 and 1961 Heaton worked as an assistant pharmacologist in the Toxicology Department at Hoffmann–La Roche in Nutley, New Jersey. She joined the Laboratory of Chemistry of Natural Products, National Institutes of Health (NIH), in Bethesda, Maryland, in 1961. Heaton served as a chemist for Dr. Evan Horning. Two years later she was hired by Dr. Marshall Nirenberg, head of the Section of Biochemical Genetics at NIH. Heaton served as a chemist for Dr. Nirenberg and participated in the latter's groundbreaking work to decipher the genetic code which culminated in a Nobel Prize in 1968. Heaton retired from NIH in 1971 but returned to Dr. Nirenberg's laboratory two years later, working part-time until 2002 when she retired permanently.

### **Interview Synopsis**

Ms. Heaton begins the interview by describing her family background and formative training. Heaton walks through the events that brought her to NIH and describes her first impressions of Dr. Nirenberg. She then narrates her working relationship with Dr. Nirenberg and the competitive atmosphere of the laboratory. Heaton recalls the poly-U experiment and the technical challenges of the research. She then describes the research leading to the award of a Nobel Prize to Dr. Nirenberg. Heaton concludes the interview with a description of Dr. Nirenberg's personality and his outside interests.

**Digital Manuscripts Program Oral History Project**  
**Interview with Norma Heaton**  
**Conducted on November 18, 2010, by Jason H. Gart**

**JG:** My name is Jason H. Gart and I am a senior historian at History Associates Incorporated. Today's date is November 18, 2010, and we are in the offices of History Associates in Rockville, Maryland. Please state your full name and also spell it.

**NH:** Norma Damon Zabriskie Heaton.

**JG:** Do you prefer the full four names?

**NH:** I use two middle initials in my signature. Damon is a family name, so I was not about to drop it when I got married.

**JG:** History Associates has been contracted by the Digital Manuscripts Program of the History of Medicine Division at the National Library of Medicine to conduct a series of oral history interviews in support of *Profiles in Science*. The purpose of this oral history is to capture recollections for the historical record and to assist the staff at the National Library of Medicine in updating the *Marshal Nirenberg Profiles* website. We will be talking about the history of the genetic code and your relationship with Dr. Marshall Nirenberg. I want to start with your background. You were born in Cleveland, Ohio?

**NH:** That's correct, 1937.

**JG:** Talk a little about your interests as a child.

**NH:** Animals. At a very early age I wrote to the Humane Society and I became a junior member of the Humane Society. I grew up with dogs—only one at a time. I was born in Cleveland, but actually my parents were from New Jersey and I grew up in New Jersey. I read a lot. I liked the Nancy Drew mystery stories and I read the Cherry Ames nurse stories. I do not know if anyone even talks about those books now? They led me to an interest in medicine. I did not really want to be a nurse and I did not know that I wanted to be a doctor. While I was in college I worked in the lab in Passaic General Hospital in Passaic, New Jersey. I started out as a chemistry major in college and then switched to a double major in biology and chemistry.

**JG:** Talk about your family background. What did your parents do for a living?

**NH:** My father was an engineer with Gulf Oil, and my mother was a homemaker.

**JG:** Do you have siblings?

**NH:** I have one younger brother, John, born when I was twelve and a half.

**JG:** What was his career?

**NH:** He is also an engineer.

**JG:** Talk about your high school in Nutley, New Jersey. Did you have any high school teachers that were particularly influential?

**NH:** I really can't think of any, no.

**JG:** How about science classes? What attracted you to the sciences?

**NH:** I think I was attracted to medicine and thus biology from reading the Cherry Ames nurse books.

**JG:** You attended Bucknell University?

**NH:** That is correct.

**JG:** That was in 1955?

**NH:** Yes, September of 1955. I graduated in June of 1959.

**JG:** You decided to pursue a dual degree?

**NH:** Well, I started as a chemistry major because my father thought that would be a good career. It seemed very dry and dull to me so I switched to a double major in biology and chemistry.

**JG:** Did he want you to do engineering? Was there any push towards engineering?

**NH:** No, no. He just wanted to make sure that I studied something that was very practical.

**JG:** What were your aspirations at the time, entering Bucknell University in the mid-1950s?

**NH:** To get married and have children. I think that was the case for a lot of women of my generation. I do remember saying to my mother once that maybe I would like to be a doctor, and she actually did not encourage me, because she said that that would be a very difficult career once I was married and had children.

**JG:** So you found chemistry dry?

**NH:** Yes. I just felt that biology was a little more interesting.

**JG:** What interested you in biology?

**NH:** Its relation to medicine.

**JG:** What was Bucknell University like in the 1950s?

**NH:** It was an excellent school.

**JG:** Okay, so it had maybe 5,000 students at the time?

**NH:** Oh, no. I don't know what it is now, but I think it was about 2,500 students when I went. It was a small liberal arts school. It is a good school. It does have a good engineering school.

**JG:** Did you look at other small colleges?

**NH:** I also applied to Douglas College. It was the female branch of Rutgers. I think there was one other school I applied to. I know today that students apply to as many as twelve and fifteen schools. I applied to three. As I recall, I got into all three, but I just visited Bucknell and that was it. The campus is beautiful.

**JG:** If I am correct, this is the school that is on a hill, so there is a lot of walking?

**NH:** Oh, yes. The women's dorms are down below, so there is always a hike up the hill.

**JG:** Did you have women mentors in the program?

**NH:** There was no program as such. I simply started out as a chemistry major and then I switched my major. I guess we had advisors, but I don't ever remember talking to one.

**JG:** Was it strange to be a woman in the school in the 1950s?

**NH:** No, not at all. There were a lot of women. The women's dorms formed a little quad down at the bottom, so I never felt that we were overwhelmed by men. I felt it was like fifty/fifty between men and women.

**JG:** You graduate in 1959 and what are you doing?

**NH:** That summer my family took a trip out West and after I came back, about September, I started working for Hoffmann-La Roche, which is a pharmaceutical firm in Nutley. In early 1961 some sorority sisters of mine from Bucknell called me. They had gone to work at NIH in Dr. Evan Horning's lab, and they called and said there was an opening in the lab and was I interested? I said yes because I thought it would be nice to get away from home.

**JG:** One thing we did skip was that you were a medical technologist at the Passaic General Hospital. What was that like and what were your responsibilities?

**NH:** I enjoyed that, that was fun. I cannot remember how I actually got that job but I was one of the laboratory technologists. I would go to the floor with my little basket and take

blood samples from the patients and come back and analyze them. I also did the urine analysis and I did the chemical analysis. I also helped the pathologist with histology. I enjoyed doing that.

**JG:** This was during the summers while in college?

**NH:** This was summers, three summers. Actually, when I look back now, I simply had someone in the lab train me. I was not a trained certified medical technologist and they were sending me out to the floor to the patients. These reports were going to the doctors, who were relying on these to analyze what was wrong with their patients. I can remember a doctor standing over my shoulder, waiting until I had done the counts on the slide. I knew I was doing it accurately but I am thinking now they should never have relied on a summer student to do that.

**JG:** You would have been how old?

**NH:** Eighteen, nineteen, twenty. I knew I was doing a good job, but I am appalled now, thinking that the doctors were literally relying on summer students, who were not certified, to do work that they were relying on to analyze what was wrong with their patient.

**JG:** Well, it was a different world.

**NH:** I also did pregnancy tests on rabbits.

**JG:** You graduate and you go to—

**NH:** Hoffmann-La Roche. It is a pharmaceutical firm. I worked in the toxicology department and worked on long-range studies on Valium. It started as Librium. Librium had just come on the market and these were long-range toxicology studies on Librium. We were feeding and weighing rats, and then sacrificing them and taking out their livers and all their organs, and preparing them for histology.

**JG:** You mentioned earlier that you had thought about being a physician.

**NH:** It was during college. I remember mentioning it one summer while I was in college.

**JG:** You mentioned your father, and his interest in making sure that you pursued something practical. What were his goals for you after college?

**NH:** He just wanted to make sure that I was happy, that I was well situated. He didn't want to have to worry about me. He wanted to know that I would be able to support myself.

**JG:** You mentioned the call from your sorority sisters, and your move to Dr. Horning's lab. Walk me through the events.

**NH:** Well, I had a call from my sorority sisters who were working in Dr. Evan Horning's lab at NIH, in the Heart Institute [National Heart, Lung, and Blood Institute]. There was an opening and would I come down? I had to come down to be interviewed. I remember that I was supposed to fly down the day that JFK [President John F. Kennedy] was being inaugurated. There was a huge snowstorm, the day I was supposed to fly into Washington, and so I was delayed. But I came and I had the interview. I got the job, went back home, finished up at home, and then came down to start working there.

**JG:** What was Dr. Horning like?

**NH:** He was there but I did not work with him directly. I was working for another doctor in the lab.

**JG:** What were some of your assignments?

**NH:** It was a boring job, to be honest. The man I worked for really, I don't believe he really needed a technician. I ended up taking ODs and washing glassware. I was bored. Dr. Horning left NIH and went to Baylor College of Medicine, and he took my two sorority sisters with him. One had married, the other had not, but they followed him to Baylor, and I stayed at NIH. I was really bored. And so, Hank [Dr. Henry] Fales took over Dr. Horning's lab and I went to him and I said, "Please, I have got to do something." He said that he knew of two job openings, one was with Marshall, and the other was with someone else who I did know later.

**JG:** Before we get to Dr. Nirenberg, what was NIH like when you arrive? You had never lived in Bethesda, Maryland, or Washington, D.C.?

**NH:** No, never.

**JG:** Did you like Bethesda?

**NH:** Yes.

**JG:** You arrive, I guess, in concert with the new Kennedy administration?

**NH:** Yes. Well, I will tell you one thing. I think that for someone in her early twenties, coming down at the beginning of the Kennedy administration . . . . Let me digress for a minute. I come from a strong Republican background. Roosevelt was a dirty word in my family. My father wrote a letter to President Hoover, and I think I still have the letter, he saved it, saying, it was not your fault, or something to that effect.

**JG:** Regarding the Depression?

**NH:** Yes, exactly. I can even remember when Nixon was running against Kennedy. I think I even went and stuffed envelopes for him. But I came down, and there was a whole atmosphere in Washington of a fresh start, and I became a Democrat.

The whole atmosphere in Washington was just like fresh air and a fresh start, and I just thought Kennedy was wonderful. So I came to my own political affiliations on my own, not just from what my father always said. So my impression was, wow, Kennedy's in and look what he is doing. It was positive and it was good.

**JG:** How did your father feel about that?

**NH:** We did not discuss politics too much. He did not like it also that, when my husband and I married, we bought a Volvo. You know, he thought we should buy American.

**JG:** What was the NIH campus like?

**NH:** There was a lot of open space, you could drive in. There was no parking problem. Parking in the boondocks was parking now almost next to Building 10. You had no parking problem. It was nice, it was very open. It was like a campus. Which it still is, but now of course, with security, it is a lot different, and the parking situation is difficult.

**JG:** What was NIH like for a young researcher or technician? Was it different than Hoffmann-La Roche? Were there more activities?

**NH:** Hoffmann-La Roche was very much of an old boys' network. There were a lot of restrictions. It seems to me that men and women were not supposed to eat together in the

cafeteria. They gave you a five-pound box of candy at Christmas and a bonus. It was very . . . . I want to say Old World, or something. It is a Swiss firm. NIH was a lot more open and freer that way

**JG:** What about being a government employee?

**NH:** To me there was not much difference.

**JG:** From 1961 to 1963 you are officially a chemist?

**NH:** Yes. I was always classified as a chemist. You had to have something like forty-one hours of chemistry to be classified as a chemist, as opposed to a biologist, and I think I had forty-three hours.

**JG:** You move from the Laboratory of Chemistry of Natural Products. Does that laboratory still exist?

**NH:** I don't know. Hank Fales, I am sure, has retired.

**JG:** You are offered a position at the Laboratory of Biochemical Genetics.

**NH:** I think at the time it was still a section under [Dr. Sidney] Udenfriend. I don't think it was a laboratory at that time, but you would have to check on that.

**JG:** Walk me through the move, and your first introduction to Dr. Marshall Nirenberg.

**NH:** I knew about the genetic code, but I do not think I realized what Marshall had done and the whole impact of it. Because it was in 1961 that they announced that they had cracked the code. This was two years later—in 1963. Hank introduced me to Marshall. Marshall interviewed me in his little tiny office. Did you read the talk I gave?

**JG:** Yes, I did. I read it this morning.

**NH:** Well, the one thing that I remember him saying, he was talking about what he wanted to do. He wanted to work with a system that would use cockroach legs, and I am thinking, cockroaches? I did not like bugs then, I do not like bugs now, but I thought, “Oh well.” I took the job anyway, and we never did use cockroach legs, thank goodness.

**JG:** What was the background of Dr. Fales? He was a chemist?

**NH:** He was a chemist, yes.

**JG:** What were your first impressions of Dr. Nirenberg?

**NH:** Maybe because, I guess, I did not realize the full impact of what he had announced two years prior to that, I never felt intimidated by him. He just was very soft spoken, very kind.

I just felt very comfortable with him—I've always called him Marshall, not Dr. Nirenberg.

**JG:** He was described as over six feet tall?

**NH:** Yes. Tall, slender.

**JG:** Dark hair?

**NH:** Dark hair.

**JG:** I think Dr. Stetten described him as lanky, shy, quiet.

**NH:** Yes, all those.

**JG:** You have described him in your writings as modest, soft spoken, very kind. You wrote that he was a true gentleman, that it was in his nature to be generous with his praise. Talk a little about that. Also, what was your role in the laboratory?

**NH:** Well, we never talked about cockroaches after that. I was one of two technicians. Theresa Caryk was the other. Once I got into the lab, he had Theresa Caryk and Brian F. C. Clark, who was a postdoc with him, show me what we were doing and how to run the experiments. He had cracked the code, and they had already determined that,

numerically, it had to be a triplet code. All they knew at that point, to the best of my recollection, is that phenylalanine was a string of U's. That maybe others would be one C to two U's, or two C's to one U, but they did not know the order of them, and they were just finishing up that work.

The very first day, I think, I was there, Theresa was showing me how to run one of the experiments. She took a tube out of the freezer, and she and Brian were saying how it was hot. I am thinking, how can it be hot if they just took it out of the freezer? Well, they were referring to radioactivity, and I had never worked with radioactivity before, so my thought was, how could it be hot, they just took it out of the freezer? But anyway, Marshall let Theresa show me how to do the experiments. Then, to the best of my recollection, it was that November that we came up with what we called the binding assay.

**JG:** Who else is in the lab when you arrive?

**NH:** Theresa Caryk was the other technician. I joined the lab in July of 1963. She had joined January of 1963, so she had only been there six months earlier. We are still very good friends. She is a wonderful person. She was born in the Ukraine, immigrated to this country when she was about twelve, met another Ukrainian here and raised children and has grandchildren, and we are still very much in touch.

**JG:** Who else is there?

**NH:** The lab grew quickly after I arrived, but the people I remember when I came are Phil Leder, Judy Levin, Brian Clark, and Bill (William) Groves. I replaced Marshall's first technician, who was Linda Greenhouse. The room I worked in was 7D03; it was a single modular lab. The back part had been divided off into Marshall's office, and it was just as wide as a standard government desk. There were about two chairs. You walked in, the window was in front of you, the desk was to the left, and two chairs. The file cabinet was to the right. There was a blackboard on the wall that they had installed, and he had double helix models that always sat there. The room was very small and crowded, and you usually had to clear off a chair to sit down.

The layout of 7D-03 was: on the right was a chemical hood and the sink and some bench space. Theresa and I worked on the left-hand side. The right-hand side was sort of reserved for Marshall, if he wanted to do an experiment. We usually did not like it when he tried to do an experiment, because he did not clean up after himself and we always had to clean up after him. He didn't do too many hands-on experiments, but once in awhile he would like to get in there and try an idea he had.

**JG:** Let's talk about Dr. Nirenberg. He was a postdoctoral fellow for DeWitt Stetten, Jr.?

**NH:** Yes.

**JG:** He was born in New York, but he considered himself a Floridian?

**NH:** Well, a New Yorker, too.

**JG:** He studied at the University of Michigan at Ann Arbor. He came from a zoology and a chemistry background?

**NH:** I don't know that I can discuss his background as well as other people, or Myrna could.

**JG:** The lab is very crowded and you are working elbow to elbow. What was the culture of the lab?

**NH:** What do you mean by the culture?

**JG:** Well, what was the style of the lab? Was it relaxed—

**NH:** It was intense and busy.

**JG:** It was intense. How so?

**NH:** No, I would not say it was relaxed. It was crowded. Across the hall were Dick Marshall, Ray Byrne, and Fred Bergman. Next door was Don Kellogg, Fritz M. Rottman, and Mert Bernfield. Then the next room up was a double module, and it had Dolph Hatfield, Sid Pestka, Philip Leder, Judy Levin, Joel S. Trupin, and the dishwasher. I do not think there

was another lab. There was a little tiny alcove, and the Nuclear-Chicago planchet counter was in there.

I know I mentioned in the talk I gave, one of the ingredients we used when we did the binding assay was TCA [trichloroacetic acid]. You had it in a squirt bottle, and you always had to have that pretty full when you started, because you did not have time to refill it. I do not remember our TCA reagent disappearing, but it must have because Theresa finally said, "I'm going to label this in Ukrainian." I knew what it was. I do not speak or read Ukrainian, but I knew that what was labeled was our TCA. We did not want it to disappear—in other words, if someone else ran out when they needed the TCA, they might "borrow" our TCA.

**JG:** What was the atmosphere like? Was there a competitive atmosphere among everyone?

**NH:** I think it was. I do not know that I felt that it was that competitive, because I was working directly for Marshall, but I think the other fellows felt it was competitive there.

I did not realize it at the time, and I found this out years later from someone who was in the lab, that if one of the fellows came up with an interesting result, Marshall would have me repeat it to see if he trusted their result. If I did not get that result, then he would send them back and have them do it again. If I got the same, then he trusted it. I did not realize, I mean, I simply did the protocols he told me, and I did not realize that I was repeating what someone else might have done. But I did notice, sometimes someone

might come in and say, “Well, how are you doing on that?” I just followed Marshall’s protocols.

**JG:** Did he gather people together for lunch or coffee?

**NH:** I think he did. I do not remember ever being part of the lunch bunch. There were seminars. I did not usually go to the seminars. I usually ate lunch with friends.

**JG:** How did he explain the race that you were now a part of? How did he explain his work to you?

**NH:** I do not think he said to me, “Look, we are in a race, we have to be careful.” When you get to talking about the charts, there is something I can bring up in that regard. It was competitive. I knew there were other labs out there working on deciphering the code, but I do not think I felt competitive. I did not feel like, “Oh, we’ve got to do this before anyone else.”

**JG:** You mentioned earlier that he loved fast little experiments. He had an interesting name for them?

**NH:** He called them “quickies.” I hesitated at putting that in the talk because of the connotation it has today. He would come in and he would say, “Just do a quickie, just do a quickie.” What he meant was do a fast little experiment, and he wanted me to just kind

of throw it together, just to see where his idea might lead. I guess he figured I could do two or three quickies a day.

I was very meticulous about what I did and how I did it, and I finally said, “Marshall, it takes me just as long to do a quickie as a longer experiment.” It was frustrating to me, because he wanted to just throw an experiment together quickly.

**JG:** They did not use that language at the Swiss company that you worked for before?

**NH:** No, no. I never heard of a quickie experiment. [Laughs]

**JG:** This was also a reflection of his creativity?

**NH:** Well, that was the point I was trying to make in the talk, is that he had so many ideas, and he wanted to try them out really fast. “Let’s see where this will go or that will go.” That is why he wanted me to do these quickie experiments.

**JG:** You also mentioned that at the end of the week he always liked to have a successful experiment.

**NH:** Yes. He always thought it was a good way to end the week. The very first binding assay I did was just before the Thanksgiving holiday, so that was good.

**JG:** You also mentioned that he did not keep the same hours as the rest of the staff.

**NH:** Actually, I used to come in early and have breakfast in the NIH cafeteria. I guess I would get there 7:00 AM or 7:30 AM or something, have breakfast and be in the lab by 8:30 AM. Then he would come in anywhere from 10:00 AM to 1:00 PM, let's say. A typical greeting might be "What's up?" or "How goes it?"

**JG:** I assume you lived someplace in Bethesda at this time?

**NH:** I had an apartment with another girl in Bethesda.

**JG:** Where did he live at this time?

**NH:** He was living on Old Chester Road in Bethesda. He had a beautiful home there.

**JG:** I remember reading, and I want to quote Dr. Stetten, that Dr. Nirenberg "had no hobbies, except for his work."

**NH:** I think that was probably true. He had a lot of interests, though. I think initially he and Perola lived in the apartments that were across from Building 10, on the NIH reservation, but then they moved to Old Chester Road. I know he had pre-Colombian artifacts, so this was part of his interest in archaeology. But he just worked.

**JG:** We should mention his interest in archaeology and can talk about that later. He might arrive mid-morning or early afternoon?

**NH:** He would work very late. He was usually there when I left. I think he would eat and then he would come back because he always left the protocol, his instructions of what he wanted done, on the lab bench. I have no idea what time he went home. If he was not there when the results would come off the counter, we would call him, let him know, so that he could be planning the next day's work.

**JG:** This was the mess on the bench?

**NH:** If he worked at the lab bench, he usually did not clean up after himself. But Theresa and I cleaned up after him.

**JG:** His handwriting was difficult to read?

**NH:** Difficult.

**JG:** This was because he was left-handed but taught to write right-handed?

**NH:** That is what he told me. He was a natural born leftie, but I guess back in those days they forced them to write right-handed, and so that is why his handwriting was difficult. I got

so that I could read it. You sort of knew what he was going to be putting down there anyway. It was like learning a foreign language.

I have to say, he was such a nice person. He was a gentleman. I know of other people at NIH who yelled at people. He never did. In all the years I worked with him, I saw him angry only twice. Even when he was angry, he was quiet. He just did not raise his voice. You knew he was angry, but he just did not raise his voice.

**JG:** Sometimes big scientists, very successful scientists, can be prima donnas.

**NH:** Not at all. No, he wasn't.

**JG:** But there is the theme that people who have great accomplishments can sometimes be difficult to work for.

**NH:** He could be very difficult to work for, in that he was so focused that he wanted, what he wanted, when he wanted it. I cannot tell you how many times he needed to do an experiment, and he needed a piece of equipment, or a piece of glassware that was molded in such a way, and did not exist. NIH had their own glass shop and their own mechanical shop and could design what you wanted. But you had to put in a work order. You would put in the work order, you would send it over, and maybe six weeks later you would get it. Well, that was not good enough. He wanted it right away.

I cannot tell you how many times I would walk over and I had to plead with the glass shop. They would have had a whole stack of work orders that they felt they should get to first, because Marshall's was going down to the bottom, and I would have to plead, "But we really need it right away." If I came back and said, "Well Marshall, they said it is going to take six weeks," it was, "Well, why? Why can't I have it right now?" I would say, "Well, I think other people have things in." He would say, "But why, I don't understand." It was that focus, that he wanted it, he wanted it right away. He could not understand why he could not have it right away.

I am not saying that is a prima donna. He had this idea and he wanted to do it right away and he did not want to have to wait. It was not that he was saying, "My work is more important and all these other people can wait." It was that he needed it. I do not see that as being a prima donna. I see that as being focused and creative.

**JG:** You also mentioned that no detail was overlooked.

**NH:** That is right.

**JG:** How so?

**NH:** I really do not remember the experiments I did from the time I joined until about November. But once we got into the binding assay, the way I wrote out my experiments, everything was perfectly clear on what I did, how I did it, when I did it, what solutions I

used and so on. It was just because that was how I worked. I think he appreciated that. I can remember that you had to know which solution, who made it, when it was made, how it was made, the lot number of that reagent, because you don't know, maybe one lot wasn't as good.

When other technicians came in later, and I would train them, before they would go in to discuss their experiment with Marshall, I would say, "Now, you better know every detail of your experiment, because he's going to ask you this, this, this, and this." I can remember one of the gals coming out later and saying, "You were right, I never thought he would ask me that." It was like no detail of your experiment was unimportant or overlooked.

I was meticulous enough that when I did an experiment, I would have maybe twenty, thirty, sixty tubes. If I thought one tube maybe went ten seconds longer than it should have, I immediately wrote it down. I had a little list there, and I would write down Number 10 maybe went ten seconds longer, or maybe got one extra drop of something. I think he appreciated the attention to detail that I did, because that is what he needed, and that is what was needed to make these experiments work.

But even the solutions, you had to know who made it. Was it someone you knew was meticulous about the way they prepared it?

**JG:** This is 1960s biology so things are very different. We are talking about slide rules?

**NH:** Slide rules, no calculators.

**JG:** No calculators, and certainly no personal computers. There were probably mainframe computers with punch cards someplace.

**NH:** Yes. I remember when I worked in Evan Horning's lab, with the doctor I was working with then, he was using punch cards for something. We mouth pipetted reagents. I mean, you would never do that today. We even mouth pipetted radioactive material—that would never be allowed today. It was just different.

**JG:** You mentioned that he relied on you to confirm things that other scientists did. Was that the reason your relationship was so successful with Dr. Nirenberg?

**NH:** I think he appreciated and trusted the way I worked—the attention to detail, etc., so we clicked on that level.

**JG:** Let's discuss the genetic code. In August 1961, before you get there, he goes to Moscow?

**NH:** Yes.

**JG:** He publishes a paper in the *Proceedings of the National Academy of Sciences*. Walk us through what is going on, and what the genetic code is, and what this race is all about. This is Watson and Crick, after they discovered the double helix. There is a race to actually crack the messenger. Who is Dr. Heinrich Matthaei?

**NH:** Heinrich Matthaei was a postdoc. He had already left the lab by the time I joined it in July of 1963, but I met him once when he returned for a visit. He was working under Marshall. Marshall was telling him what to do. I think the very first experiment Marshall was out of town and Heinrich called him and said it had worked.

**JG:** This was the poly-U experiment?

**NH:** Yes.

**JG:** What is interesting is that Dr. Nirenberg had no formal training in molecular genetics. He had done some evening courses in genetics, but this was really a brand new area of research for him.

**NH:** Well, from what I have read and from what I know of him, he went to Bruce Ames and he talked about it. I think it is written that he was told that this was suicidal, and he thought that was a little extreme. But he said, "If you're going to study something, study something interesting."

**JG:** Who was Severo Ochoa?

**NH:** He was up in New York, and I think he was trying to do the same thing as Marshall, so there was a competition there. I would hear that name, but I guess I did not realize how much of a competition it was.

**JG:** Talk about the binding work and the binding assay.

**NH:** Phil Leder was with Marshall and he—

**JG:** He was a postdoc at that time?

**NH:** I am not sure what his exact position was, but he was a very prominent member of the lab. He and Marshall came up with what they called the binding assay, which was a very simply, elegant, little assay. You put in nineteen cold amino acids and one radioactive one, and then a small amount of triplet ribosomes with conditions to make it work.

If the triplet to be tested was, for example, UUU and the nineteen cold amino acids included phenylalanine and the radioactive amino acid was, for example, alanine, UUU would still bind with the phenylalanine, be precipitated out, and trapped on the filter, but because the phenylalanine was cold, not radioactive, it would not be counted. If, instead, the radioactive amino acid was, in this case, phenylalanine, then the trapped complex would be counted and give a positive result.

We made up twenty solutions with each one minus a certain amino acid so that we could add the missing amino acid in a radioactive form.

I do not know if the machine shop actually invented the single plater or if these were things that were already invented, but eventually they had the machine shop make what we called a multi-plater, and that is now in the display. Initially we used single platers. It was a little round, stainless steel tube, just big enough to hold the Millipore filter, and about so high, and it screwed onto a base. You had a glass Erlenmeyer flask connected to a vacuum, and then you had a rubber gasket at the top, and you plunked this thing down. Then you had the vacuum on, and you took one of the test tubes that had your experiment in it, and you would precipitate the complex with TCA. Then you would pour it through the plater and the precipitated complex would be collected on the filter.

Then you would unscrew it, take out the Millipore filter with forceps, and put it in order onto a piece of aluminum foil. Initially, we used what was called a Nuclear-Chicago planchet counter. You placed the dried filter onto little copper or aluminum planchets, about so big around, they had a little, tiny lip, and you would put the filter on that, and then you would stack them up and you would put them into the Nuclear-Chicago, and they would drop down and as they went across, the level of radioactivity would be counted. Eventually, we would put them into scintillation vials with scintillation fluid, and they went to a scintillation counter. But the initial results were counted in the Nuclear-Chicago planchet counter.

It all had to be timed. When you got good at this, you knew how many seconds it took you to unscrew this single plater, take it out, put it down, set it up with a new Millipore. I think I got so I could do it every thirty seconds, or maybe every twenty-five seconds. But thirty seconds gave you a little lag time. Eventually, you had a multi-plater, and it was a little easier. But initially, we used single platers.

**JG:** Dr. Nirenberg mentioned that it was almost like a factory.

**NH:** It was, sort of. Particularly when we got to the multi-plater point it was just, run through these experiments, with all different conditions, all different permutations. I did so many binding assays that sometimes I thought if I had to do another one, I'd scream.

**JG:** Describe Phil Leder. What was he like?

**NH:** Phil had a wonderful sense of humor, really smart. I don't know how to describe him. A very, very good scientist.

**JG:** Dr. Nirenberg would be working day and night?

**NH:** Well, I was working day, I was not working night. I do not know who was there all night.

**JG:** You mentioned that the lab work was intensive. It was exciting?

**NH:** It was. One of my memories, which I mention in the talk, is that I can remember being in the room, and Mert Bernfield and Edward M. Scolnick—Ed went on to be vice president of Merck and was instrumental in developing Vioxx. Mert went out to Stanford University; Mert has since died. They were both kind of, I don't want to say loud, but they were not quiet, soft-spoken people. One of my memories is, they would hang over the Nuclear-Chicago planchet, and they would have the tape and they would say, "Which one is this? Which one is this?" Then you would hear this shout, like, "Oh, we discovered a new one." It is one of the strong memories I have of that time.

**JG:** Talk about the significance of the charts.

**NH:** Well, I do not even know if the term spreadsheet had been invented then. It got to the point where we knew we needed to have some sort of means of seeing it all in one big picture. So I taped data paper together and drew the columns and rows with a pen and ruler. Actually, Myrna has all the permutations of the chart and she gave us copies. I started going through them, to see which are the earliest ones. This is a very early one, and down at the bottom it says, "T-9-1, T-9-150, N-6-173, N7-88." What that refers to is, Theresa, book 9, page 1; Theresa, book 9, page 150; Norma, book 6, page 173; Norma, book 7, page 88. In other words, data in this line comes from my book 7, page 88, which is the experiment on page 88. You could actually date this if you had the book. Anyway,

I started doing this, and this is pretty life-size. This is one of the earliest ones, because it goes back to Norma, book 6.

**JG:** I think this is the actual size.

**NH:** I think it is too, because this is the data paper. I am pretty sure. There were other permutations that I started. I mean, this was getting a little messy, and to the best of my recollection, I took the other data paper. It had wider columns, and I taped that together, and I think it was two sheets down, and maybe two or three across. There was a table set up in the hall, and I tried to work out in the hall. This is one of the things that I thought would be interesting for the interview. The lab benches, there was not that much room, because you had a shelf there, and you had stuff back here. Trying to work with a big sheet of paper on a lab bench was difficult. I taped these all together and I took them out in the hall. Marshall did not want me to work in the hall. I said, "Well, Marshall, these are big, I can't work on them in the lab." He said, "No, no, no, you can't work here."

Well, what I realized, and it never occurred to me, is that there was a competition. He was afraid that someone was going to come by, look over my shoulder and see my data, and then I guess go running off to some other lab and say, "Well, this is what Nirenberg's found." It never occurred to me at the time. Or even if I thought of it, I thought, well, if it is somebody new, I would see them coming down the hall. I would know they did not belong. We were literally at the end of the hall, and there was a stairway at the end. It was not like they would be passing by me. They would have to make a conscious effort

to come down and look over my shoulder, and I guess I figured I would throw myself over them and hide it or something. I guess I was young and naïve and idealistic. I could not believe that somebody would stoop so low as to do something like that. I just thought that that is not right. But of course, that is not the case, people would do it.

Anyway, Marshall insisted and so I moved my little spreadsheet operation into the lab and did the best I could on the lab bench. But there are two big charts and there was like twenty amino acids. The first one has ten, the second one has the other ten, and if you put it together, I don't know how big it would be.

**JG:** Where were these kept in the evenings?

**NH:** I folded them up and put them in my book, or maybe he took them home with him, I don't remember.

**JG:** Describe how long it took to fill in all this data?

**NH:** Oh, I don't know. I could not tell you. That is my writing along the top. It is my writing putting in the data, except for some. This is Marshall's writing, and this is Marshall's writing.

**JG:** What are the red notations?

**NH:** In other words, I have written in the data, and he has circled that because that is a positive result. In other words, he wanted it to stand out in his mind. This is aspartic acid GAU, GAC. In other words, anything that was elevated, that looks like a good result. Here is CGA with arginine.

**JG:** How long did you work on this effort? This went from 1963 through—

**NH:** Through probably 1965. Not this particular chart. But up until the end, when I did the chart that is considered the Rosetta Stone. That one I tried to be very neat on. What I remember about that one is that, when it was all folded up, it stuck up above the notebook and so the top of it was getting very frayed. That was missing for a long time, until Myrna found it.

**JG:** You used the term Rosetta Stone.

**NH:** I did not invent that, someone else called it that.

**JG:** Contemporary to it? Or later, in the 1970s or 1980s?

**NH:** I am not sure. I remember Joe Goldstein referred to it as the Rosetta Stone. I had just heard it referred to as the Rosetta Stone of the genetic code. I mean, I did not come up with that term.

**JG:** Was Dr. Nirenberg's handwriting easier to read than Greek?

**NH:** Yes. Easier than Greek, but it could still be challenging. I have to say that when I was doing this, I mean I knew what I was doing, but I guess the full impact of it did not really hit me, that I was creating the Rosetta Stone of the genetic code. I mean, you would think, why wouldn't I know that? But, I don't know. I knew what it was, but when you are right in the middle of it, I guess it is different than when you are outside looking in.

**JG:** The handwriting on this is you and the other technicians?

**NH:** No. The only writing on this is me or Marshall. All of this up here, that is Marshall.

**JG:** Did you fill out the chart each day?

**NH:** I do not recall having this out, and then as I got the result running back and putting it in. I think it was more periodically, I went and updated.

**JG:** The other interesting story from that period is the missing freezer?

**NH:** Missing freezer?

**JG:** I read that a liquid nitrogen freezer disappeared with all the ribosomes and enzyme preps.

**NH:** I don't remember that.

**JG:** Dr. Stetten talks about this period as NIH's finest hour.

**NH:** I think what they are referring to is that when he started doing this—and again, this is before I actually joined the lab, this is Maxine Singer and Bob Martin—they put aside their own work to help him. It is what NIH is set up to do, is to collaborate between the scientists for the purpose of the research, not to be competitive against each other.

**JG:** Do you know when you finally realized that you were going to win? Was there a point where you just realized that this lab was going to be able to make the announcement in 1966?

**NH:** Well, as the triplets were discovered, they kept publishing. It was not like they waited until 1965 or 1966 to say, "Okay, now we have done it all." They would publish them as they went along. I don't recall their being like, "This is the big announcement." It was just that, each time they were convinced of the accuracy of their work, they would publish.

**JG:** How did the project end?

**NH:** I don't remember. You mean like the last triplets to come off?

**JG:** Yes.

**NH:** I worked full time for Marshall until 1971, but at one point I was working with Tom [Dr. C. Thomas] Caskey, and he was working on terminator codons. Marshall, by then, had turned over the genetic code work to Tom Caskey and others. He was going into neurobiology. He felt like, "Okay, I have done this, been there, done that. I want a new challenge."

**JG:** He had already started switching over?

**NH:** He had already started switching over, yes.

**JG:** Let's talk about 1968 and the winning of the Nobel Prize. You were in New York City when you heard the announcement?

**NH:** There was another part in the talk that I didn't say. I eliminated it from the talk because it was getting too repetitious. Marshall was so exacting about what was published, it had to be written, it had to be accurate. And up until the last minute, he did not want to let a paper go. Of course, you did not have PCs, you did not have word processors, just typewriters, so you were literally cutting and pasting. The secretaries could be there really late at night, and they would use a lot of white-out and so on. I can't tell you how many times I made a mad dash downtown to hand in his paper to the National Academy

of Sciences. It would be due at 5:00 PM on a certain day, and I would be leaving NIH at 4:00 PM and we would try to get to the National Academy of Sciences by 5:00 PM.

What I was getting to is that, up until the last minute before it was due, he hung onto it, to make sure it was written correctly. One of the reasons I went to New York in April 1968 is, I came into work and he had left me a plane ticket to fly to New York. He had been working on the paper for the Lasker Foundation up until 3:00 AM the morning before. The secretary was Exa Murray, and she had stayed there with him, and they had worked on it. It was finally ready. Marshall said to me, "Here's the paper, Norma, you have to deliver it today to the Lasker Foundation." There was no e-mail, there was no fax, it had to be hand delivered, and you could not get it to New York other than by hand delivering it.

I had just gotten married six months before, so my husband took off from work, we bought him a plane ticket, and we flew to New York and we hand delivered his Lasker Foundation paper to the lady at the Lasker Foundation. We were given a tour of the building. It is right down by the U.N., and then my husband and I spent the day in New York. We went to a museum, we had dinner there, we flew back late at night, we got back really late, 11:00 PM or 12:00 AM, got to bed late. The very next morning, it was announced that Marshall had won the Nobel Prize.

Everybody had heard about it, and they had all gone to his house in Bethesda, to sort of wake him up I guess. My husband and I were kind of groggy from the night before, so

we did not go to the house. We got to work a little late that day. That was my memory of that. People just poured into the lab. By then, the lab had moved from the 7th floor to the 6th floor, and we had the whole 6D corridor, and people were just pouring in. It seems to me Marshall sent people out to get food, and I think they brought in champagne. There were people who I knew worked on other floors, who had no connection to our lab, but they were all coming up, and it was just very exciting. Then he took the whole lab to lunch at a Chinese restaurant in Bethesda. Some of the pictures in the display were taken at that lunch.

**JG:** He was the first federal researcher to win a Nobel Prize?

**NH:** That is right. To show how shy and modest and unassuming he was, within the next few days they had a big gathering in Masur Auditorium, and he was going to be up on the stage. He was going to be very uncomfortable being on the stage, and so to make him more comfortable on the stage, they brought in an oriental rug and a big comfy, leather club chair, so that he could be in this comfy, leather club chair with an oriental rug on the stage. They said it was because he was so shy and self-effacing.

**JG:** Do you think he ever thought he was going to win the Nobel Prize?

**NH:** I think he knew he had been nominated and that it was Nobel Prize-worthy work.

**JG:** Do you recall what your first few days, when you got some time alone with him, and talked to him about winning the Nobel Prize, what that was like?

**NH:** I do not have any recollection of that.

**JG:** Did the laboratory change after he won the Nobel Prize?

**NH:** Well, it had already changed because he was into neurobiology. Marshall had a lot of different ideas on how to pursue whatever questions he had in neurobiology, so he was coming up with a lot of different systems. There was work using neuroblastomas, a lot of the cell lines that Takehiko Amano helped develop. He went into nematodes for a while. At one point he thought he wanted to work with *Ascaris [lumbricoides]*, which I was horrified about. *Ascaris* are worms that live in the intestines of pigs. They are pretty horrible. He thought that would be a nice technique to use for neurobiology, so he made arrangements to pick up some *Ascaris* when pigs were being slaughtered. He had some young fellow from the front office go over and pick them up and bring them back, and it was horrible.

The smell was awful. The fellow from the front office, I think they gave him some sort of commendation for helping, because nobody else would get near them. He helped Marshall wash these buckets of *Ascaris*. Then Marshall had a dissecting board set up, and he would pin this *Ascaris* to the dissecting board, and he sat down there with a Coke

in one hand, just happy as a clam. He was going to dissect this *Ascaris*, and I guess get out the nerves or something, and everybody else is standing out in the hall and grimacing.

Dr. French Anderson came in and told him about all the dangers that *Ascaris* could pose to humans. Marshall listened, put on one glove. French kept talking, he put on another glove. Then he set aside the Coke. The long run is that we never did study the *Ascaris*. That just did not come about. Another thing, Marshall drank a lot of Coke. He always had a Coke.

**JG:** How about coffee?

**NH:** I am sure he drank coffee, but Coke was his drink of choice.

**JG:** In the glass bottles, I guess?

**NH:** Cans.

**JG:** Was Nobel Laureate Nirenberg different from Dr. Nirenberg? Did he change after the Nobel Prize?

**NH:** No, no, not at all.

**JG:** We have spoken a lot about success. What about a failure that he had—a scientific or professional failure?

**NH:** There were ideas he had that did not pan out, but I do not think he saw failure as a failure. It would be like, “Okay, that didn’t work, we go on to something else.”

**JG:** Were there disappointments?

**NH:** I am sure there were. I mean, there were ideas he tried that did not pan out. But that never discouraged him. He just kept going.

**JG:** Dr. Nirenberg changes fields, goes into neurobiology, and then you retire in 1971?

**NH:** I did.

**JG:** Talk about your stone farmhouse.

**NH:** Well, my husband I married in September 1967. We were not hippies, but that was back in the hippie days, you go out to the country and raise all your own veggies. In 1970 we started to look around to buy our first home, and we wanted to get a little land, we wanted to grow all our own vegetables, maybe have chickens or something. We found an old stone farmhouse which had suffered a fire—it was not livable—seven and a half acres, out in Washington County, Maryland. It was really a wreck, but we fell in love with it,

and we bought it in November 1970 and started restoring it, and we were able to move into the house in March 1971. And very naively, I thought we would be done in six months, and it has taken forty years. It is now on the National Register of Historic Places.

It is just a simple stone farmhouse. There is a stone barn, and we initially bought seven and a half acres, and we now have it up to about twenty-eight and a half acres, so we have added to it. The garden gets smaller each year. We did have chickens for a while, and a small herd of Scotch Highland cattle, and we are down to one aging steer now.

**JG:** How did Dr. Nirenberg accept your retirement in 1971?

**NH:** I don't know. He did not pressure me to stay.

**JG:** Then you return about two years later?

**NH:** Well, I had stayed in touch with Theresa Caryk, and I came down and had lunch with her. We ran into Marshall and he said, "Oh, Norma, nobody's kept the books up the way you had." I sort of jokingly said, "Oh, well, I will come back and take care of the books for you," or something like that. He said, "Really?" I mean, he really perked up. I thought, well, yes. We were into antiques, and I thought, well, I can earn a little extra money to buy antiques. So I said, "Oh, okay." It was set up for one day a week. It was a part-time position, it was not a full-time position, and I was paid WAE, which means when actually

employed. It was one day a week, and then it was two days a week, and then it was three days a week. It ended up four days a week.

**JG:** What did he mean by keeping the books?

**NH:** Well, it was the way I wrote things up. The way I kept the solution book, the way I had written up my experiments. Just the organizational skills that he needed to have done, I guess.

**JG:** I guess the other thing that changes in the 1970s and 1980s is that biochemical techniques start to evolve?

**NH:** Well, the technology had changed and, of course, computers were used. I did not get into that until I came back; I started using a computer.

**JG:** When you returned two years later, did you feel that you had lost skills during your absence, or did you feel comfortable?

**NH:** I did not get back into doing experiments so much because I was only working part time. I think one of the earliest things he wanted me to do is, at that point he had moved to Building 36, and he had two corridors. He wanted me to get everybody's reagents organized and computerized. He wanted to know who had what in what freezer. I had to go around to everybody and either inventory their freezers myself or just ask, "What do

you have?” Because he wanted to know what stocks he had on hand. I ended up putting that into a computerized base. If somebody had ordered a chemical that he thought he could use, but he didn’t know about it before, I put it in this computerized base, then he would know about it. I also computerized the cell lines he had developed.

**JG:** What are some of the other things that you are starting to do? You are also working on the cell lines?

**NH:** I did start doing some tissue culture work. I never really enjoyed tissue culture work. I am not sure why.

**JG:** Describe the type of work this is?

**NH:** Well, it was keeping the cell lines going in flasks and feeding them, culturing them, splitting them, freezing them. I did run some GABA experiments on them.

**JG:** How did Dr. Nirenberg describe his switch to neuroblastomas?

**NH:** He did not describe it, he just moved on. I mean, he did not say, “Well, now I am getting rid of all this, I am moving on.” He just moved on.

**JG:** Did these new directions require upgrading the lab?

**NH:** Well, you needed new equipment, and people with different interests. He got people who were more interested in neurobiology.

**JG:** I read that you did Foundation for the Advanced Education Sciences classes.

**NH:** I took those earlier. I took biochemical genetics from Bruce Ames and Robert Martin. I took biochemistry from John Finlayson. Even though I was a biology and chemistry major, I had never actually had a course in biochemistry, and I had never had a course in genetics.

**JG:** Let's talk about Dr. Nirenberg in the public arena. He has been described as having a social consciousness. He spoke about the ethical moral implications of genetic research?

**NH:** I do not even remember when that paper was published.

**JG:** I think 1967. The one in *Science*.

**NH:** I never got into a discussion with him about that, or at least I don't remember.

**JG:** In the *Science* editorial we are talking about, he writes that the expansion of scientific knowledge could lead to man's "power to shape his own biological destiny." He continues that "Such power can be used wisely, or unwisely, for the betterment or detriment of mankind." This is still a conversation that we are having forty years later?

**NH:** I go along with what he said. I do not think you can stop this research from going on. I do not think you should put your head in the sand. It is going to happen, but I think you do need some ethical guidelines to it. When I asked Marshall once if he had the opportunity to go back in time to see and relive the past or to go forward in time to see into the future, which would he choose? He immediately said he would want to go forward, and I think this was typical of his thinking and insight—always looking ahead to the future, to the next experiment, to the next scientific problem, and to the big questions both scientific and ethical that would arise in the future. For myself, since I am interested in archeology, anthropology, and currently am obsessed with genealogy, I would want to go back in time to see and relive the past of my ancestors.

**JG:** Did the lab have holiday parties?

**NH:** I remember there was usually a Christmas party. There was a fellow in the lab who had an apartment close by, and we had the party there once.

**JG:** Did your spouse go with you to these?

**NH:** That was before I was married. After I got married we moved out to Hagerstown, so I did not socialize with most of the people in the lab. Well, I should not say that. Some of us did start a little wine-tasting group.

**JG:** What about the challenges of being a woman at NIH in the 1950s or 1960s?

**NH:** I did not have any problems, but then I wasn't trying to make a name for myself in the sciences, or have my own research project.

Marshall always acknowledged and thanked his technicians for excellent technical assistance, but he did not make them co-authors until much later in the lab. I just accepted that that was his position, although technicians in other labs at that time were routinely made co-authors. He said later that I should have been a co-author, but it was too late then.

**JG:** How about your relationship with some of the other people that came through the lab, the postdocs and the other people?

**NH:** I have stayed in close contact with a lot of them. Last November, about a year ago, the lab was declared a National Chemical Landmark. You knew that?

**JG:** Right.

**NH:** Okay. Well, the American Chemical Society declared, not the physical lab, but the work as a National Chemical Landmark. There was a reception at the Cosmos Club the night before, and then the presentation and a symposium the next day. I had been retired at that point. I retired in 2002; this was six years later. Dolph Hatfield called me up and asked

if I could help get out the email invitations to that? This is going to sound like I am digressing but I am getting to the point.

I said, “Yes, sure, I will help.” I had worked for Marshall on contract two years before, so I felt like I was still part of the lab. So anyway, I tracked down people and I got them there. Then when the symposium and the memorial service were coming up this year, I had a lot of the names already in my Microsoft Outlook folder, so I tracked down even more people. Some of the people I e-mailed and I said, “I don’t know if you remember me, but . . . .” They would say, “Oh, yes, I remember you.” Then they would share little reminiscences, so it was nice.

They all remember me, and sometimes I have to remember, well, so and so was here then, and they don’t remember so and so because they came later, and they came later. I mean I sort of knew the whole gamut of them, but they all remembered me. As I said, we had a little wine tasting group that got together, and I still have lunch with some of the former lab members.

**JG:** What is your assessment of Dr. Nirenberg’s long career?

**NH:** My assessment of it?

**JG:** Yes, what is his legacy?

**NH:** Other than the work he did, I mean, he cracked the genetic code, he deciphered the genetic code, he went on to do research in neurobiology. They described him as a titan in science. He was just a wonderful, wonderful person to work for. And not just to work for, but as a person. He was truly one of the nicest and finest people I have ever known, both as a person and as a scientist.

**JG:** We talked before about his interest in archaeology, and you told a great story right before we started taping?

**NH:** My dirty little secret. He had me come back to work on contract in 2008, because he wanted to turn all his cell lines over to the ATCC, and I had been the one who had set up the entire computer system of them.

**JG:** The ATCC is the—

**NH:** American Type Culture Collection. I came back to work on contract. At that point, I felt like I could have more of a personal relationship with him. I said, “Well, Marshall, I am going to tell you my dirty little secret.” I told him about how I started as a chemistry major, then switched to biology/chemistry, but that I really was not that happy in my major. That by the time I was a senior in college, I am looking through the college catalogue, like, what would I really like to study? And I thought it was anthropology and archaeology. He went, “Oh,” and his eyes brightened up. He said at one point he thought he would have liked to study archaeology.

Then he went on to describe a visit to Petra, which I believe is in Jordan. He described how you walk through this long corridor-like canyon to get to Petra, which is a city carved out of solid stone. He went on about it. He was very interested in archaeology. As a matter of fact, after the memorial service, Dr. Uriel Bachrach, one of the scientists who used to come to work as a visiting scientist from Israel every so often but who was unable to come to the memorial service, e-mailed me back after listening to the talks on the NIH website. Uri listened to it and he said, "Oh, yes, I was absolutely right about archaeology." He knew Marshall was interested in it, and he described taking Marshall to some digs in Israel and talking with him about it.

I do not know if his interest in archeology was something that a lot of people were aware of, but he was very interested in it. He had pre-Colombian artifacts in his home.

**JG:** Last question. If you have one piece of advice, or one lesson learned from your association with Dr. Nirenberg, that you would like to pass on to a future researcher or technician, operating ten or twenty years in the future, what would that be?

**NH:** I would say to study something you find interesting—and then pay attention to every detail, be honest and accurate. But enjoy the process of experimentation as well as the answers and results.

**JG:** Thank you very, very much.

**NH:** You are welcome.

[End of Interview]