



Mary Jo Nye Oral History Interviews, March 26, 2015

Title

“From the Laboratory to the Archive, a Historian of Science Evolves”

Date

March 26, 2015

Location

Valley Library, Oregon State University.

Summary

In interview 1, Nye discusses her family background and upbringing in Nashville, her earliest interests in science and science fiction, and her decision to pursue undergraduate studies in the sciences. From there she notes her initial college experience at Vanderbilt University, her transfer to the University of Wisconsin-Madison, and the tumultuous social and political environment that was pervasive around the world in the mid- and late-1960s. She then describes the circumstances by which she made the decision to pursue graduate training in History, the atmosphere in Wisconsin's History of Science department, and the work that was required of her to develop a historian's skillset, including a memorable early research trip to archives in France.

The Nyes' move to the University of Oklahoma is the next focus of the interview, with Mary Jo reflecting on the environment that she and her husband encountered at Oklahoma at the end of the 1960s. She then recounts her progression in academic rank while on faculty in Norman, and also discusses several of her trips to work as a visiting scholar at other institutions.

The session next shifts its focus to Nye's second book, the topic of which was scientific communities and provincial leadership in late 19th and early 20th century France. In discussing her work on the book, Nye recalls her initial idea for the project and her experiences conducting research in a variety of French archives. She then reflects on her activities in 1988 and 1989, a time period during which she served as a visiting professor at Harvard and as president of the History of Science Society.

As the interview nears its close, Nye shares her memories of her tenure as chair of the History of Science department at Oklahoma. The session concludes with an examination of another of Nye's books, *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950*, published in 1993, near the end of her years at the University of Oklahoma.

Interviewee

Mary Jo Nye

Interviewer

Chris Petersen

Website

<http://scarc.library.oregonstate.edu/oh150/nye/>

Transcript

Chris Petersen: Alright Mary Jo, if you'd please introduce yourself with your name and today's date and our location?

Mary Jo Nye: Mary Jo Nye and today's Thursday the 26th, March 2015 and we're on the, I believe, third floor of Valley Library.

CP: We are, yes. So, we'll talk a lot about your career and your association with OSU, but we'll begin at the beginning. You were born in Nashville, is that correct?

MJN: Yes.

CP: And were you raised in Nashville?

MJN: Yes, I grew up in Nashville, with just a few trips down to Alabama and Florida until after I left.

CP: What were your parents' backgrounds?

MJN: My parents both were college graduates. My father was born in Tennessee, my mother was born in Alabama and they met at the University of Alabama where they both graduated and then he went into the Marine Corps, actually before the war began, but then he fought in the south pacific and was wounded badly in Guadalcanal and shipped back to the United States eventually. And then after the war was over, my mother was I guess what we call a stay-at-home mom, but she was a wonderful mom; among other things, encouraged me from the very beginning to read and to sort of develop myself. And same thing with my sister. My dad worked for various loan companies and he was doing real estate loans and mortgages and that kind of thing when he retired. So, both college graduates, he was in business, I guess you could say, and my mother was in charge of her daughters.

CP: What was Nashville like for you growing up?

MJN: Oh dear. Well, one thing I would say about Nashville is that of course it's known for country music, and I did go to the Grand Ole Opry once. I grew up in a home where, I guess it was WSM, my father's clock radio would go off. Maybe it wasn't a clock radio in the beginning, but anyway, I remember that there was music on in the morning and I could hear from the room when we'd get up. And a lot of it was sort of Grand Ole Opry, that kind of stuff, country music. But of course Nashville also is well-known for its universities, its colleges and education, as well as its churches and many church-affiliated schools.

And I sort of grew up in a community that was dominated by that culture of music of all kinds and of education and business, I guess you would say. And as a consequence, when I was very young, I was taking music lessons at a little college called Belmont College where my father's mother had—my grandmother who died when he was about sixteen—but she had gotten a college degree at Belmont College. And I used to ride my bicycle over at another college to which I could walk, it was called Peabody College, not too far from where we lived, and then of course Vanderbilt was not too far down the road towards town. So, I sort of grew up in this culture of education and going to the library, on the bus originally, before they had two cars, with my mother on the weekends to check out books from the public library.

And on the other hand, Nashville is very class-stratified. It took me a while to realize that. I think, initially, like lots of southerners, I wasn't particularly aware of segregation, I didn't think about the fact that my schools were segregated and that the only black people that I was seeing were people who were doing chores coming into the neighborhoods. And as I grew older, I became very, very aware of not just class distinctions, but of race distinctions that were tied into class. And that was hard. I remember going down to Alabama with my family for the family trips during the summer. When my dad would get a two-week vacation, we'd go down and visit my grandfather who was living in Alabama then, near the Mississippi border. And I remember just everything was segregated. I was more aware of it there than I was in Nashville, but in Nashville the movie theaters were segregated.

[0:04:57]

But in Alabama and on the Mississippi border of course, the water fountains were clearly black and white, restrooms at the gas stations were white only. I remember thinking as a kid, "where do the black people go?" Because they were all around us on those trips. And going to this little town and wanting to go to the swimming pool and asking about it, for some reason something came up and I was told that it was only white people went there and I said "well, where do the black people go?" and I was told they don't have—I guess we called them "Negroes" then, or "colored people"—I was told "they don't have a pool." So, growing up in that south, that was part of the national experience. And just gradually becoming aware of it and beginning to think about it.

So, it was mixed: education, music on the one hand, my family—well we ended up going to a southern Baptist church for years and I gradually sort of stopped going. And so, you make your accommodations with things.

CP: How about science, did you have interest in science from an early age?

MJN: Well, I was reading science fiction books when I was in elementary school. I remember that, I can remember some things about the Martians. I was interested in the idea of extraplanetary life and that kind of thing. I mean, I do remember the science fiction as well as other things. And then, of course, Sputnik went up the year before I went to high school and I was fascinated by space exploration and that notion, as so many people were in my generation. And then I benefited very much from the post-Sputnik expansion, or funding for scientific education. And so when I went to my high school, which was a segregated white public school, I got a very fine education there. It was a very good school, I was very lucky, and we had—I was tracked into sort of an honor's track and I was taking Latin, four years of Latin to—I loved English literature, just read, we did plays, acted and did all that kind of stuff. I did forensics and debate. But we had good math education, they didn't—I don't know, at our school you couldn't do calculus but I did the usual sort of algebra, geometry, that kind of stuff. And then I took science courses beginning the first year with, I guess, general science. And I did some biology, which I was a little squeamish about, which I guess means it was a good education. We must have been doing things.

But I became really interested in chemistry, so the two things that I was most interested in were astronomy and sort of space exploration and that kind of thing, a lot of stuff that was in the news about flying saucers, and I found that kind of interesting, which tied in with my early interested in science fiction. But then the chemistry class that I had was, I think, probably exceptionally good for its time.

And looking back on it, the one thing that I've mentioned to a couple other people when they've asked me about my education is that my chemistry teacher my junior year in high school was a woman, which that was a little unusual. And she is, I remember it—and I think my memory's accurate on this—had been the summer before I started in the fall a chemistry class, and I think it was advanced chemistry or something she had been teaching. I mean not teaching, she had been taking classes at Vanderbilt. And she came back to us with some of the stuff that she had been learning in her college. I mean, obviously she had a college degree but she was taking some advanced classes, which may have been necessary for the qualifying of teaching education, I don't know. But she was just really enthusiastic in explaining to us, as well as she could, what she's learning and sort of tying what she was learning into the exercises, what we were learning and into the lab exercises we were doing.

[0:09:52]

And at the same time, she was the coach of the women's basketball team. And the women's basketball team was quite good. They were very successful. So, here was this woman who had these two sides to her that I was observing. One is this, well maybe charismatic, but a very successful high school women's basketball team coach and the other as this chemistry teacher who was very enthusiastic. And it really made a difference to me, I think. And she must have been kind of an inspiration to me, and made me feel that in fact, when I went to college, I could do chemistry, I could do science. And so, I got interested in chemistry in particular at that point.

CP: Do you remember what it was about the discipline that captivated you?

MJN: A lot of it was theoretical. This was sort of one of my difficulties as I went on to get a chemistry undergraduate degree, because I lacked confidence, I think, in the lab. But I really loved the theory. And so, maybe I had a more literary approach to science than a sort of hands-on, technical, pragmatic approach. And I could do lab work and we

had participated in a science fair and that kind of thing when I was in high school. And we did experiments at home. I remember that in my mother's kitchen. And got...it was theory and one of the curious things that you'll find interesting is that one of the things that we were learning in theory was the sort of fundamental electronic theory of the chemical bond. And we were learning some of the really basic ideas that you would find in Linus Pauling's textbooks.

I mean, it was the, you know, and it was the sort of implementation of that chemical bond program that was introduced in high schools in the sixties and, well I was there in the fifties, the late fifties, but when was it introduced? The chemical? The electron chemical bond probing became the basis of high school curriculum. But anyhow, we were getting it and she was learning it, as I mentioned, having this course at Vanderbilt. And so she brought it in to us. And this would have been about 1961, I guess. But we were learning it. So, it was just a way of organizing material. I mean, we learned the periodic table but then we were learning it in terms of the electron structure of atoms and that kind of thing, and so it made sense and it was not just memorization, and there actually was a kind of order that you could figure out that didn't have to do with just memorizing, but learning some basic rules and theories that were attached to the rules and employing them.

But as I say, I was never, I never developed confidence in the lab. And when I was in college and I was, you know, made my way through from introductory and then analytic and then organic and physical, all this sort of thing, I did all the experiments and I did fine, but I just lacked confidence. And I would have nightmares about things going wrong in the lab. Of course you have nightmares about teaching too, but—and those continued to bother me until maybe fifteen years after college. And I would still sort of have a nightmare in which I lost the key to my locker where something was going wrong at the lab bench, that kind of thing. And I wasn't keen on—I loved the sort of smells and doing things where you were suddenly getting something new out of these things you'd put together, and I was sort of fascinated by chromatography and I was interested in spectroscopy. I eventually ran a mass spectrometer machine one summer and, but I just, I had this feeling that I just didn't quite trust myself.

CP: Well, you went to school at Vanderbilt but you didn't stay there, is that correct?

MJN: Yeah.

CP: You want to tell me about your Vanderbilt experience and the transfer to Wisconsin, of all places?

MJN: Well, the Vanderbilt, as it still is, was a very, very good school and I think I, well they had a very strong chemistry department and I was taking a curriculum there that was a core curriculum and history, and I took German for my language there and I took literature classes there and was also, I started out in calculus the summer after my senior year in high school. I went to Vanderbilt and began taking classes there and worked my way up through differential equations at Vanderbilt and then ended up going to Madison, which was, of course, entirely different, and liberating in so many ways. I mean I found it just a different world for me.

[0:15:39]

But, and I found too, which I hadn't been sure about, I could do just fine in Madison, and what I had learned in Nashville first at my high school and then at Vanderbilt had prepared me perfectly well for Madison, a big university and—or not as had been the case at Vanderbilt. But that I could manage. But what happened there, of course, is that I finished high school in '62 and I went to, I spent two years at Vanderbilt and went to Madison in '64 and I got to Madison and left Vanderbilt just at the time that both civil rights and the questions of atmospheric testing had been going on in the early sixties, and then the Vietnam war was beginning to escalate.

So, my college education, as it was true for people of my generation, both as an undergraduate and then as a graduate student, particularly then in the period from '65 till '70 down at Madison, was one that was really, really tumultuous politically, and exciting. When I was in Nashville, some of the sit-ins at the Woolworth five-and-dime stores were just beginning to integrate the counters, lunch counters which they had at the Woolworths. And when I got to Madison, that had been an issue in Madison about the same time. And then in Madison, for one thing, of course we were following what was going on in the civil rights, the marches in the south and demonstrations and then, under Johnson, the enactment of legislation to do something about voting rights and those kind of issues.

And of course I had, as I mentioned, spent time as a kid in Alabama, and so George Wallace was a very real figure to me and the whole south thing was very real to me, although I didn't go back and participate in demonstrations, I didn't do that. I guess that was a choice I made, although I don't remember actively making a choice not to do it. I don't remember anyone ever saying to me "Mary Jo, we're going to go down today and are you going to come with us?" That never happened to me. So, I didn't make a choice in that sense. But in Madison, I was there during the first Dow demonstrations and I was there when there were protests by students against CIA hiring on campus, as well as protests against Dow because of their involvement with napalm.

And of course the draft was in effect and so men I knew were all, as students, subject to the draft in principle, including Bob, my husband. And either the student deferment or the marriage deferment was a help, but we had some younger friends at Madison who made various decisions. One of them did go to Canada. He stayed there, he's still there. But people were having to make those decisions every day of their lives, those kinds of decisions to participate, not participate, to wear an armband, not to wear an armband, to try measures in order to avoid the draft, to protest the draft. So it was a kind of time of tumult—which is familiar to you from reading about it—that was just very, very real in the lives of us as students.

And we persisted. Bob and I got married and we met in Madison and we persisted in continuing to do our studies and finished our graduate degrees, but at the same time, we were very, very self-conscious politically. And we did do, take certain actions, none of them I would say courageous or...I would not say courageous, but on the other hand, we did make decisions to demonstrate or not to demonstrate, and of course to vote or not to vote. I always voted.

[0:20:21]

And we were in Paris in 1968. We'd left Madison to do our graduate studies just after Martin Luther King was killed, and then we were in Paris when Bobby Kennedy was killed. We were in Paris during the student riot days in Paris in '68. We arrived in May just as the subways shut down, and to our dismay as well, the libraries and archives shut down and the banks shut down. And we had no credit. That was a period when I suppose people had credit cards, but we didn't. And just imagine yourself being cashless, and we were supposed to have our stipends transferred into our account at a French bank and it closed. We literally had no money. I mean we had no money and no way to get money. And we were fortunate in that we became friends with the people who managed our hotel we were staying in. They just said "pay us when you get your money," which was lucky for us.

But it was, we were there when the Soviets went in to Prague and crushed the Prague—and a lot of our, we were part of a sort of a community in Paris that was a student community, the ex-pat kind of students, including some Czechs we met. And it was just all very, it was all very, very intense. It was very intense student life. But we were lucky because, in the sense that we can decide if wanted to do the graduate degrees. And we were able to finish and we did eventually get our work done in France and came back. And of course came back still with Vietnam war and the invasion and went to Oklahoma after we finished our degrees, and then continued to watch what happened over the next three years until, I guess the US was no longer involved in fighting, so to speak, and then Saigon in what, '75? So, and that was a period from '65 to '75 of just very, very intense political actions.

Including in Oklahoma where there was a demonstration during the Cambodian invasion. Bob was one of the people who was on the football field trying to hold back, keep the National Guard, which was doing—was it National Guard? Anyway, it was one of these things, or situations where the students were protesting the presence of some, I think it was National Guard troops, and faculty members had volunteers to act as a kind of boundary between the students and the guards. And it was tough. But of course the draft then was stopped and then things changed a lot and the students no longer were facing the draft. And I'm not saying that's the reason students became less politically involved, but it was less immediate.

CP: Yeah. Well, I'm interested in knowing, you went to Wisconsin and you finished your undergraduate degree in chemistry, correct?

MJN: Mhmm.

CP: But then you made a switch to history after that?

MJN: Yeah.

CP: What was the process there?

MJN: The process was that I made a decision when I was getting my chemistry degree in Madison that I had too heavy a load. I was taking—what was I doing, I was taking physical—anyway I had just too heavy a load, too much chemistry. And I needed one more credit. I think it was three credits I needed in chemistry, and I suddenly discovered that I could substitute for a one-semester course in inorganic chemistry, a course in the history of chemistry. And I thought to myself "well, that sounds fun. That will get me out of a lab and I can learn something about the history of chemistry."

And so I took that course with Bob Siegfried and I enjoy it a lot and it obviously, it was obvious in retrospect it changed my life because I, at that point, I was, I hadn't made up my mind if I wanted to go to graduate school in chemistry, which also shows you how things have changed now, because now there's sort of intense pressure to do your GREs early and do all your graduate applications and all that sort of thing. But either I was, maybe I was just naïve but I wasn't thinking along those lines. And I had been encouraged to think that I could be admitted to the graduate program in Chemistry at Wisconsin pretty easily, if I decided to go on to it. And so, I didn't feel uptight about getting it, I felt uptight about whether I wanted to do that path.

[0:25:54]

And so, I was working for somebody in the physical—Paul Bender—in the physical chemistry lab at Madison over the summer after I graduated, and I happened to run into Bob Siegfried, with whom I had taken this course on campus, as I was walking along probably in the area of Bascom Hill or South Hill, where the history of science department was then. And I saw him and we talked some, and I think he knew Paul Bender, who was quite a fine man and physical chemist and I guess I must have told Bob Siegfried that I was a little undecided about whether I wanted to do it, and I probably said to him, which was going on in my head at the time, that I just wasn't sure I wanted to be in the lab and that I knew that I couldn't get a chemistry Ph.D. without spending a lot more time in the lab, and I couldn't just suddenly become a theoretical chemist. And I didn't even understand then that there's not this strict division between theoretical and experimental chemistry, that you had—best theoretical chemists had had a lot of experience with during experiments or certainly know what's going on experimentally.

But in any case, Bob Siegfried said to me "well"—so and I said "well, I'm thinking maybe what I want to do is to be a science journalist, that maybe what I want to do is to write about science rather than doing science." And he said "I have a suggestion," and so he said "why don't you just take a year and come into the history of science department and see what you think of it?" he said "as it happens," this was July or so, I don't remember, but anyway he said "as it happens, someone who we had thought was coming to graduate school to enter our program in the fall is not coming after all. So at this moment we happen to have a little extra money, so we could help you out with tuition." And I thought—so, I tried it. And my idea was that I would go in, I'd spend a year, maybe two if I decided to get a master's degree, but I really liked it a lot, so I stayed.

CP: Wisconsin by then had a rich tradition in its history department. What was the environment like for you in history of science?

MJN: It was—at Madison, the history department and history of science department were separate. History department was huge. History of science department was, of course, small. I don't know, if I think back on it now and I try to count them, there maybe have been six or seven faculty members in the history of science department. And they were in a separate building from the history department. And so, it was a smaller program, although at that moment in 1965, it was probably larger than it ever had been. This had to do partly with the draft, because so many people, particularly men, were coming into graduate programs all around the country in order to get a draft deferral. And the history of science department at Madison—I need to check on this, because my memory is that there was something like forty people who were beginning classes in history of science as graduate students that fall. And that, I don't know, that's probably too large a number. Maybe it was forty people who were in the whole department, but it, as small as it was, it was a lot bigger than it later was.

But even so, it was small and my classes had maybe eight, ten people in them, the graduate level classes. And I liked the people who were my colleagues, one of whom, I mean several of them became friends but one of them is one of Bob's and my oldest friends. And then I began taking classes. There's a basic curriculum and so I was doing history, taking classes in history of biological science and history of physical sciences and ancient science and medieval science and all of that.

[0:30:28]

And I really did enjoy it. And what I enjoyed about it was that it was really giving me a context for the way in which the sciences had developed, and I was really interested in the sciences. The approach in the department at that time, which was typical of history of science programs around the country, those that existed then, was very much one of intellectual history, history of ideas, and changes would take place by the early seventies in which social history became more important to history of science. But it was very much the history of ideas. And I liked that and I loved writing. I mean, I just loved the writing. And I found that I really loved the research. I loved reading. I mean, I loved working in the library going down in the old stacks. And what do they call that system, the cutter system where you had to pull out boxes? It was a different organization but I just loved it.

And I had some very good teachers in the program here and I ended up working with Erwin Hiebert, who was just a wonderful person and one of the smartest, very, very smart, but a wide range in what he was interested in. So, in our classes, I mean he encouraged me, if I wanted to, to do some work that had to do with interactions, intersections between literature and science. And so, I think I wrote one of my papers that had to do with thermodynamics and the way in which—themes of thermodynamics and again, the sort of the end of the world and of the universe were used in literary works at the end of the 19th and the beginning of the 20th century. He had an interest in religion and philosophy, but also in both physics and chemistry, which suited me just fine. And Erwin was, his field was more German history and German science than others. He had worked in the Manhattan Project, by the way. He had been in Chicago during the Manhattan Project as a physical chemist and then had gotten his own Ph.D. after that and then taught it in Madison. And the year after I finished my degree at Madison, he moved to Harvard where he finished up his career. Died just about a year and a half ago now.

And his whole family's a musical family and he sort of opened up his house, he and his wife Elfrieda, to his students. We'd have seminars at his house, which was sort of in the old German tradition, and it was just lovely. I mean as a student, being invited into a professor's home and just, I remember he was sitting in this area with books all around us and these photographs. I remember a particular one of Brahms that was a print or something of Brahms that was on the wall, big grand piano in the main room where Elfrieda played. Erwin played the clarinet and all of their kids were musical and they played various stringed instruments or horns. And so, it was very inspirational.

And there was a seminar that I took with him in which we did all these various papers and I sort of identified three possible dissertation topics, one on J.J. Thomson and the electron, which would have been English, British, one that would have to do with Wilhelm Ostwald in Germany and physical chemistry and then one coming out of a paper, in the seminar as well, on Jean Perrin and French physical chemistry, say at the end of the 19th and beginning of the 20th century. And at that point, when I was making—trying to think about this, I had been, I had studied German, so I was not fluent by any means but I could read German and I'd worked through some German texts with Erwin. So, he had confidence that I could work with German texts.

[0:35:09]

But Bob and I had gotten married and Bob, my husband, was planning to go, I mean his research had always been France-oriented at that point. And so he needed to go to France and so that decided me to choose my French topic among my three, which meant that I had to sort of intensively study French in summer school, which I did, to get ready. It didn't do much good. When I arrived, I could barely speak but I could read a little bit, and then just developed both reading and speaking by doing it, essentially. But particularly, I mean the reading is just the doing it is what teaches you in the end.

CP: I'm interested in sort of the acquisition of the toolkit for a historian, and it sounds like it came fairly naturally for you. You were a science undergraduate, you switched to history, but was it pretty, did it feel natural to you or was it work?

MJN: Well Chris, I regret that I didn't have more historical, more background in history. There's only so much you can do. And most of my reading as a young person had been in the sciences, science fiction or literature. As a young person, I had not read much history just to pick up a history book and read it. And as I was becoming a historian of science, in the early years I more and more regretted that I didn't know more about the long historical context of culture, in France, for example, and then just in general in Europe and the US. And I knew the basic outlines, I mean I had American history, I had European history, that kind of thing, but it wasn't just sort of secondhand to me. So, that was a challenge.

And my initial work really focused on history of science as history of ideas, but overtime I developed more and more techniques of studying the history of science that had to do with political and cultural context and social conditions and institutions. But that really just came with time, with getting older and doing different things and branching out and learning more as I went along, rather than any initial learning education that I had as a student. It's a bit of a problem because, as I say, there's only so much you can do and so much reading you can do. But I think it's very important for historians like myself to have kind of, if you want to call it natural understanding, of the conditions in which the scientist worked. And that comes with time.

But was it easy? I think it was, because of what I just mentioned, that my approach initially was history of ideas and that was easier than doing something that really required more of a thorough understanding of institutions and politics, say in France. But I did develop those kind of interests over time.

CP: Did you do any teaching at Madison?

MJN: I was lucky—well, either lucky or unlucky, you can argue it both ways. I had National Science Foundation funding after my first year. First year I had university funding and after that I had NSF money. And at a certain point I thought that I was going to be leaving graduate school without ever having any teaching experience. So, I asked if I could teach and I taught as a teaching assistant for David Lindberg, who was developing—who had a wonderful course. And he was a fabulous teacher at all levels, but he was really masterful with undergraduates. And it was a real privilege to teach in that class. And it also had the advantage that I was teaching and listening to his lectures and preparing discussion sections with students at the very time I also was preparing, doing my own preparation for my preliminary exams. And so the teaching experience, in some respects, certainly complemented in very important ways what I had been learning in classes and getting me ready for the preliminary exams, because it gave me a lot of knowledge that I simply didn't have, general kind of knowledge. Because that's what I was teaching students. So, generally sort of elementary knowledge of developments in history of science over time. What we call "Plato to NATO."

[0:40:19]

And I taught "Plato to NATO" at Oklahoma for years, in two—we were on the semester system, so I did it sequentially. And I really loved it. I mean, I loved teaching a lot of things that had to do with ancient science, even though I'm no expert. And early modern science. And I think that too I learned a lot in that way. Which, you can learn a lot by teaching. I mean, everybody says that. And that's why teaching and research are so—they're complimentary to each other. I mean, they really depend on each other. Enthusiasm for teaching comes for many people in their ongoing research, just as I mentioned my high school chemistry teacher's enthusiasm coming from what she was learning in research. She was—and new research that she was learning at the time. And I've always found that the teaching and research go together in our work, and in the work of lots of historians whom I most admire.

CP: So, tell me how your dissertation experience kind of resolved. I mean, you mentioned you were in Paris, didn't have any money for a while and you didn't speak any French for a while, but I assume that you got the work done that you needed to get done and you wrote your piece?

MJN: Yeah, I was—both Bob and I were very lucky in what happened to us, because I was working on Jean Perrin and his son, Francis Perrin who had, at that time was—he had been Head of the French Atomic Energy Commission and he was a professor at the Collège de France. I contacted him. I can't remember if I asked Erwin to write him first or if I just wrote him myself as a graduate student from Madison, saying that I was going to write my dissertation on his father's work. Which is very presumptuous. An American, you know, this young American woman writing. And they were so kind to me, he and his family, and he realized that I couldn't use the library, I couldn't get in the Bibliothèque Nationale, couldn't use the Archives Nationale, and he actually brought over to me, he had brought over to me at the hotel where we

were living some letters and correspondence of his father's. And he loaned it to me while we were there so I could work through it. I mean, how amazing is that? And I was so very grateful to him for that.

And Bob had a similar experience that he was working. Also, his work was focused on a figure, Gustave Le Bon, and he had written a letter to introduce himself to a woman who'd had the legal right to Le Bon's papers. And she met us at a café, a patisserie near the—in the Bibliothèque Nationale, which was closed, in Paris. And we had this conversation over lunch and after that she gave him access to the papers. So, the families were, they were just very, very kind to us. And we were very grateful. That got us to a certain stage when we couldn't do anything else and then we just, as soon as the libraries—they were only closed about three weeks, four weeks, something like that, but when they reopened we just did nothing but work. I mean, people who were there for longer periods of time, Americans, we'd see them going out for like a two hour lunch. I mean, there were a lot of jokes about the two hour—but they were real things and people had time for lunch. We just never did. We just worked and worked and worked. We were there at the library the moment it opened and we left five minutes before the doors closed. So, we had to work hard in order to finish, because we had a schedule of just being six months in France and we had a certain amount of money and we managed to meet the schedule and then came back to Madison and wrote the dissertations pretty rapidly and then revised them with first books. But they were, Bob's was more ambitious than mine. Mine was fairly limited as a dissertation topic.

[0:45:18]

CP: A lot of work, but it must have been very exciting to have that experience with your husband in France.

MJN: Oh yeah. No, I mean it was life-changing. I mean Madison was life-changing and then Paris was just—the challenge of learning this new culture and of being on our own in a way we'd never really been before and of coping, but then also it was just exciting. And it was an exciting milieu, again politically, things were going on. But we were, again, we were incredibly disciplined. We didn't get distracted. We didn't go out at night and go to rallies and things like that. We just, we—both of us, I mean we had, we didn't feel like we had any backup, so we really had to get done what we needed to get done, because otherwise we wouldn't have finished. And that was going to be on our mind.

So, we went back to Madison and wrote up the dissertations within a period of about eight months after we got back. And Bob got a job at Oklahoma and I had a, again, National Science Foundation, bless their hearts, I had an NSF post-doc, which I took to Oklahoma when Bob got the job there.

CP: And what was the environment like at Oklahoma when you arrived?

MJN: Well again, it was tumultuous, because it was 1969. We went there in the fall of '69 and there was a lot going on. And it was pretty intense. We were also pretty—I mean I was, what was I, twenty-four, twenty-five, something like that, and Bob was two years older. So, we had finished pretty quickly and we were pretty young as faculty members, which gave us, as faculty members for a few brief years, a kind of relationship with the students that was one that had to be a certain distance on the one hand but also, even with the undergraduates, I mean we weren't that much older than the undergraduates. So, we were, we eventually, in fact, became quite good friends with some students who were undergraduate students of ours, some of who are best friends, some of them the best of friends among those ones who did actually did go on to graduate studies. And so, we became colleagues over time but had begun as undergraduate and teacher, and then in later years became close friends and colleagues through those ties.

But as the undergraduates, again there were political demonstrations, there was tension between the legislature and the university because, as in Madison, I won't say anything about what's going on there now, but there was tension between the legislation and students. And in Oklahoma just as in Madison, there was a tendency when there were demonstrations on campus, and this still, again now '69, '70 in Oklahoma, it's been true in Madison just a few years earlier and continued to be true. State legislators tended to think that the campus would be calm and that there would be no problems if only the outside agitators would go away. And so, the way of dealing with the outside agitators was to increase out-of-state tuition, which in both cases, Bob and I thought was a real shame. In both cases, we were out-of-staters, although we in one case weren't liable for tuition, so to speak, but we were out-of-staters.

But on the other hand, for the in-staters, when you increase tuition, as has just been done by this institution, twenty-eight thousand dollars a year for nonresidents, what you do is to really imperil the experience of students on campus. Because

for those students who are in-staters, Oklahomans, for example, to have limited exposure to students from other states with other experiences and not to be able to form friendships with people from places other than Oklahoma or other than Wisconsin is a real detriment to their education, because of course universities are not simply about books and labs, which they are, and about ocean-going vessels and research and internship experiences and that kind of thing; they're also about new living experiences and the people you meet and the ideas that you experience and experiencing people who are different from you and their ideas that might be different from yours. And if you don't have that experience, then your whole education experience in person is diminished.

[0:50:40]

So, that was one of the things, when we initially got to Oklahoma, it was a little bit more diverse than it was later through the years there. And now graduate education's a little bit different. That's—I'm talking about undergraduate education.

CP: Well, my notes say that you arrived in 1969 as a post-doc; in 1970 you were appointed a visiting professor, 1975 the "visiting" is removed from your title. So, I'm interested in the process of establishing yourself, and I think, if my math is correct as well, at some point along the way you had a child.

MJN: Yes. Our daughter was born in 1971, the fall of 1971 and I, when we got to Oklahoma I had this post-doc and I was overconfident that the history of science department would be overjoyed to have me there. That turned out not to be the case, initially. And one of the things that happened there is that I did do some teaching and in some ways, when Leslie was little, I suppose it was nice to be doing part time teaching, but by part time, I'd teach one semester and then I wouldn't be hired the next semester. But I was teaching and I taught big classes for the most part, and I don't mean hundreds of students, but classes of sixty students, something like that. And I enjoyed it but one of the things that I did was to decide that if I were going to be taken seriously at Oklahoma, that I was going to have to do more than just having, have a Ph.D. in hand.

And so, I really spent what time I could writing and doing research. And my post-doc, I think I, I don't know, I published two or three papers as a post-doc and then I worked on my book. And I was really lucky with the book because Erwin, my major professor at Madison, got in touch with the editor of a series. He was, and still is, in England. And so I had an introduction to send my book, manuscript in, and I got it published in '72, which is pretty quick. And then that brought me to the attention of some people and I was offered a post-doc at the University of Pittsburgh during '74, '75 academic year. And we worked that out so that I could take it, which I won't—I mean Bob and I have always tried to stay together rather than splitting up. I mean, that was a financial hit because it meant he couldn't be teaching, but we did those kind of things. And so we were there for a part of but not all of that academic year.

And it was great. I mean, it was another entirely new place to me and the program there was a history of philosophy and science program and they had some very good historians of science there as they do now, but some of the best philosophers of science in the world. I mean, it was just a world famous philosophy of science program. And I sat in on Adolf Grünbaum's classes and got to know Larry Laudan and Rachel Laudan, with whom we became very close friends, and several other people. And that was sort of pivotal, because when that happened at Oklahoma, a new person came in as chair of the department about the time that I had gone to Pittsburgh. And there was some concern that I might actually leave if I didn't have a regular appointment. And there was also a new dean. I think it was that early. I may be a little confused here. But I ended up being given a regular appointment.

[0:55:01]

But again, that's a little peculiar, because this is just about the time that Affirmative Action policies began to get introduced, and if I remember correctly, there was not what we had come to call an open search for my position. In modern parlance, I guess I was a targeted hire. And I was targeted at that point in 1975 for several reasons, but it did make a difference to the college to have me as a faculty woman. And so, whereas initially when I arrived, being a female I would say was a disadvantage, and I could tell you stories, but I'm not going to, that would indicate the ways in which it was a disadvantage. By the mid-seventies, there—it was becoming a bit of an advantage if you had people who would make an argument for you, or make an argument for hiring a woman, either for you personally or for a qualified woman. And I had the qualifications because of my publications. And I, if you compared me to the other people in the department

in terms of publications at that point, I looked pretty good in terms of comparable, that's to say, to other people in the department or people who might be hired.

So, the strategy of having done whatever teaching I could, but—and I enjoyed it, I'm not saying it was punishment to work and do research and writing, I really did enjoy it, but it also was a strategy to take that time and make that effort, but I think it really paid off for me. It doesn't for everybody, because it's a matter of luck. You never know what's going to happen. You get breaks one way or the other.

CP: Something else that, I don't know if the word "strategy" is the right word for it, but in looking over your career, you've been a visiting scholar at more places than I think anybody I've ever talked to.

MJN: Oh, I don't know about that.

CP: Well, you began at Berkeley in '77, went back six times, Institute for Advanced Study in 1981, and that's just the beginning of the list. I mean, you did a lot of travel, have done a lot of travel. You want to talk about sort of how that's worked out for you and propelled your career?

MJN: Well, I love it. I mean Bob and I have always—now Berkeley was sort of special because Bob's from the Bay area and his family was in there, living there. And we—I loved northern California and the Bay area, and Berkeley had, I mean at that time, about that time, the Office for History of Science and Technology was created at Berkeley. John Heilbron was running it and there were several people in the history department there who were historians of science who were really, really good. Berkeley department, history department for Bob's really, really good. The library is absolutely wonderful. And we could go there and make arrangements to live for the summer that didn't cost us money. Well, later on it did cost us money because we were renting somebody's house, but initially we'd go there for a month or so and split our time between staying with his dad, staying with his mom.

And then we'd then trade off, Bob and I, going into Berkeley and using the library, and you could check out books and bring them home or we could work up in the stacks. And so, we got into this routine of sort of going out there and staying for one or two months every summer, and two months if we could, and working while we were there on research and getting to know people in the area, which really increased the range of who we knew and what we knew and opportunities to meet people and hear new ideas and that kind of thing. There was one time I applied for, I don't know, I think it was NSF money again, it might have been NEH, and I said in my grant proposal that I wanted to go to Berkeley. I don't remember which time this was, and this was to go for either a year or eight months or something like that, and one of the referee reports said something to the effect of "this is okay, this is good," whatever, "but why does she want to go to Berkeley?" you know, "why not—there are lots of good libraries on the east coast, I mean why Berkeley? She doesn't really give a rationale for it." Well, it was a personal rationale, and also we had built up friendships and contacts there, so that was a natural thing.

But the other thing that we did was that we were very lucky at Oklahoma in that Oklahoma, which is on the semester system, had a policy. They did have sabbatical leave policy, which not every university does, for example Texas and Wisconsin don't have sabbatical leaves that are sort of you automatically expect. At Oklahoma, we discovered shortly after we'd been there that you could get what's called the mini sabbatical leave, which meant that you could go off for a semester at half pay or a year at one-quarter pay. So, if one of us could get a grant, then we can apply for the mini sabbatical and we could manage to support ourselves somewhere. And we usually went to France, although not always. And for summers we often, we would sort of—one summer we'd go to France and the next summer we'd go to Berkeley. We kind of split it up that way in terms of what we could afford and what we could manage.

[1:00:59]

And then the Institute for Advanced Study was just a fabulous, fabulous opportunity. One of the historians, one of the permanent faculty members at the Institute at that time was Marshall Clagett, a medieval scholar. And he knew me and he had been Erwin Hiebert's major professor when Erwin got his degree. And again, this is sort of the world of contacts, of, you know, I had published, Marshall had met me, he knew of me, and so he sponsored me for membership. I had letters, I applied and I had letters of recommendation and all that kind of thing. But on the other hand, he was enthusiastic about my coming, and that certainly had to make a difference. And that again sort of changed our lives. I mean we had to spend

time—well, we'd been in Pittsburgh but that's not on the east coast. So, this took us to Princeton New Jersey, southern New York, sort of Washington D.C. access.

And the Institute for Advanced Study is an incredibly special place. And of course I knew that was Einstein's institution. But it's truly international; there's a permanent faculty there, small permanent faculty, but then everybody else is visiting. So, some people are there for a year, a few people are there for two years. And Princeton University is just nearby. You go up to New York easily. And it's a very—it was when we were there—a very convivial place. We very often had lunches at the institute's dining hall, I guess you'd call it, and you'd just look around you at all these people, these mathematicians and physicists and a few historians and social scientists, and it was just like a who's who. And it was so exciting to go to the lectures there in colloquia, and we'd go over to Princeton University to the things, and it was just a fabulous, fabulous year. And it was privileged, a very privileged year, a very privileged place. But again, for those of us who over many, many decades have come in and left and whatnot, I think for most people it's just a very exciting experience intellectually. And there are just a few of these kind of places around the United States, and that's certainly one of them.

So yeah, we were there. And so, we found that whenever we go away, we meet new people and we meet new challenges and we use different libraries, and our sort of interests have changed over time so that—although we started going to England and then have worked in libraries at Oxford and Cambridge and elsewhere and have spent time in Berlin now and used their resources, and it's...what's the word I want? It's just revivifying. I mean, something that just brings you alive again, to do that kind of thing.

CP: You mentioned a lot of trips to France for work early on. Your second book in 1986 was a French topic: *Sciences in the Provinces: Scientific Communities and Provincial Leadership in France*. You want to tell me kind of the story of this book, how you arrived at that topic and how it developed?

MJN: I think, let me think. I think I had been asked to do a piece. Well, you may have my vita there, I don't remember. I think I'd been asked to do a piece on Paul Sabatier. But in any case, I had, something had led me to Paul Sabatier, who had been a chemist at Toulouse. And I thought he was a really interesting figure because he'd won a Nobel Prize, he shared it with Victor Grignard in catalysis in 1912. And he wasn't in Paris, he was in Toulouse and I thought that was interesting. And then I read some other things about him and realized that unlike most scientists in France at the end of the 19th century, he was a practicing Catholic. Very strongly devout practicing Catholic, like Pierre Duhem was in Bordeaux.

[1:05:36]

But okay, so Duhem's a physical chemist, he's in Bordeaux, he's also a practicing catholic, wasn't in Paris, important as a physical chemist and then became a philosopher and historian in science. And it occurred to me to sort of look elsewhere and see what was going on among scientists who were notable but were not in Paris. There was a lot of discussion around that time, what was called the center periphery model in sociology and social history, institutional history, with the theory that the most important and exciting things happen at intellectual centers, and then what is innovated in those centers then spreads out from the center to the periphery. And the periphery's sort of derivative from the center.

So, I began looking at some of these figures and trying to consider why it was that these important scientists had remained in the provinces rather than going to Paris. Had they chosen to stay in the provinces, or was there some reason they couldn't get to Paris? Because the French structure is one in which you get—you do have more prestige if you're in Paris and you also get a higher salary. And in addition, at that time you couldn't be a member, a full member of the Academy of Sciences unless you were a resident of Paris. You had to live within a certain number of kilometers of the Academy and the Senate, Île de Saint-Louis and Île de la Cité in order to be a member of the Academy. And so, the cards were kind of stacked against you if you were in the provinces, in certain ways.

So, I conceived of this project in which I chose these various cities in the faculties of science, and what I did then was to look at a scientist in each of these faculties who was very well-known and very important and then to look at the scientist's education, where it had taken in place in Paris, and then the circumstances that—where he wound up where he was at the provinces and then the circumstances that made him decide to stay there or prevented him from getting back to Paris. And I also looked at the way in which the faculties of science were organized in these provincial cities. And in part, I chose them because they were all cities where the science faculties did establish important reputations. And one of them,

Grenoble, became a real center for foreign students study. And so that project also became a study in changes in higher education in France at the end of the 19th century.

And the ways in which my argument became, as I looked at the documents, the ways in which in French provincial cities that the faculties of science were much more open to accepting funding from local entrepreneurs, business people, local interests like agriculture, mining, forestry and whatnot to fund chairs or departments that in some ways would enhance the economy of the local region. And that the Sorbonne, the University of Paris was resistant to that for some time. And then in addition, I documented in this how it is that those French provincial cities were much—they developed degree programs for foreign students earlier than did the University of Paris. And so, my argument became that not only did some of the work that was done by physicists and chemists in the provinces constitute original work that owned nothing to anyone in Paris, but that in addition, the sort of structures of science and scientific education and technical scientific education were much more innovative in the provinces, and then worked their way back to Paris instead of the other way around.

So, I mean the book was sort of a consequence of, again, sort of noticing things and thinking about them, but also some of what was going on in the literature at the time. As I say, noticing the science, the center periphery argument, and at this time there were several scholars in a, there was a center for research that was split between Paris and Lancaster, I guess it was, that was interested in these kind of problems. So, I also knew that there was a kind of, in the scholarly literature and amongst the people I respected, there was interest in this kind of thing. So, we had a lot of fun with that project. I mean, it took us to Nancy and Toulouse and Bordeaux and Grenoble and Lyon. Wonderful, beautiful city, Lyon.

[1:10:56]

CP: Well that was my next question. I'm gathering you kind of travelled around a lot in France and probably were at various different archives and various different environments.

MJN: Yeah. And one of the things that I did, which is incredibly boring, is to—you're an archive, you're in the archives here for OSU—I slogged my way through a lot of university documents and a lot of faculty meetings. And in fact, some of the stuff which you have here in Special Collections on microfilm are my microfilms of, among other things, they're really valuable, the microfilms of meetings of the faculty of sciences at the University of Paris. And I think you've got Lyon. I don't remember, I'll have to look at my records of which other ones I actually did microfilm so that we would have copies of them. But it's really boring.

And you turn up some interesting things, of course, and in terms of understanding structure, what's going in, and at the same time you know, as you're looking at these meetings of faculty, these minutes—they purport to be minutes of faculty meetings, the *procès-verbaux* you know that they can't be accurate all the time. And so, if you get some insight into personal relationships or struggles or debates or whatnot, it's a real cardinal, because usually those things are papered over. I mean usually those things don't go into a formal *procès-verbaux*. You keep them out. But those were little gems for me at the time I was working on this, in terms of arguments about applied science and fundamental science and sort of who has the key for the laboratory and what were the hours that there was these—you know, as there are at a lot of universities, petty struggles over—but they're important, over who has the key and what that means about whether, you know, who has to leave at five o'clock and who can stay and work until ten o'clock, that kind of thing. Careers were at stake. And sometimes personal matters would come out in the *procès-verbaux*. Again, this religion matter, because Sabatier was religious and there were people in his faculty who resented his stance. And sometimes that would come out in a comment that somebody would make and you would realize that he was under some strain for being a practicing Catholic.

CP: Well, 1988 looks like it was a big year for you. You were visiting professor at Harvard and also president of the History of Science Society.

MJN: Yeah, I was busy.

CP: Yeah.

MJN: I was very, very busy. Yes, it was, that was a good year too. And of course we have one daughter and she was following, she was with us on most of these excursions, so she had a very peripatetic life as a young person. But always in those years coming back to normal. That was her home, she always knew we would be coming back and the house was there, things would be stable when we got back. But, so it wasn't peripatetic in the sense she felt existentially uprooted, I guess you would say.

But yes, the History of Science Society, I was elected vice president in 1987 and then Bill Coleman, who was then teaching in Madison developed a horrible leukemia and he died. Well, he got sick. I mean, he was really, really sick and he died. And so, as a consequence, usually what happens in the society, as Paul Farber may have discussed, you're vice president, you're elect—there's an election, and tested elections, so somebody gets elected. And so, you're vice president for two years and then you're president for two years and then very often, it sort of depends, for two years after that you're sort of past president. You're still very much involved in society business and decisions and that kind of thing.

[1:15:19]

So, I had been on various committees and then was elected in '87 as vice president. But because of Bill's illness, I was asked to step in as president. And actually at this moment, I can't remember formally if I became president at his death or when he was incapacitated, but that doesn't really matter. So, my term was up and I was vice president for one year and then president for two years. And it was, there was a lot of things going on then in terms of some of your organizations within the society. The worst thing that happened was that—I don't know if it was the worst thing that happened—but what was that the year? Wasn't it '88 when the stock market crashed?

CP: '88, '89, one of those two, yeah.

MJN: And the History of Science Society had been given what appeared to be a great deal of money. They were all in stocks. But we couldn't actually get them until a certain date, and by that time it'd crashed. And it was, I don't know if it was horrible, but it instigated a lot of things that had to do with financial matters and strategizing and whatnot, setting up committees and one thing or another and not being able to do exactly what we thought we wanted to do. But it was very lively. I met a lot of wonderful people and worked with them, like Sokal, who was the secretary when I was president, later on became president himself. But when I became—when I was elected vice president, what was said at the time was that I was the first woman in the so-called modern history of the society to be president. Dorothy Stimson had been president earlier, in an earlier period, but there hadn't been any woman who was president recently until I was elected.

So, that was seen as a breakthrough in the society, for women who were becoming increasingly members in the profession, in history of sciences and other fields. And then after me there have been a good many women presidents. There is one now, there was the last time. So, it's no longer, of course, uncommon at all for women to have the presidency. And since I was president too, and within the society, some changes have occurred, the most notable of which there is now, and there has been for some years now, someone who's called the executive secretary of the society, who's not a volunteer, as we all had been, but who is a paid, salaried employee of the History of Science Society. And that's made a huge difference because everything was volunteer, and having someone who is a full-time paid professional with some academic training and who—it really makes a difference. And Keith Benson was the first. No—is that right? But anyway, Jay Malone is now. And the sort of seat of the History of Science Society has moved around. It's now at Notre Dame University. But the place where the sort of headquarters were located has changed over time. So, the Society doesn't have its own building in Washington D.C., it's nothing like that.

But anyway, Harvard invited Bob and me both to teach during '87, '88 in the department. But we ended up only doing it in the spring of '88 because of our daughter. It just didn't make sense to do it for the whole year. And that was great. I mean, it was really fun. And as we, again, we made friends there among students, graduate students, who, many of whom we've kept up with over the years. And we each taught two courses that spring on the face of a seminar and then a course, but the course was fifteen students, something like that. But one was a graduate seminar, the other was an undergraduate class. And it was fun.

[1:20:04]

And after that, I guess it was after that, I became a member for one term or two of the overseers, the board of overseers for the history of science department at Harvard. So, I was going sort of regularly for a while to Cambridge for those meetings. And I enjoyed that. It was good. So, there was, we ended up, after having had spent a lot of time in the west coast, a period in which we were spending more—some time as well on the east coast, and our access in the middle, you know, Midwest sort of, Wisconsin, Oklahoma. So, I feel, again, very lucky to have had those kinds of experiences, to be able to have a sense of many different places and environments and programs across the country, but yet at the same time having the security in the place of home of being either Oklahoma or at Oregon State.

CP: At the time did it feel like you were breaking new ground as a woman in the profession?

MJN: Well, sometimes it did. I mean not always, but sometimes I would become aware. I mean at Oklahoma I kind of sort of looked at the figures of how many women that were on the faculty and then certainly how many full professors there were. I mean, it was very striking that it was a little unusual in the early days of 1978 I was promoted to full professor. No, that's not right, '75 was assistant, '78 was associate, so full wasn't until the early eighties, maybe '82, something like that. But even so, that was, there weren't a whole lot of women professors, full professors. And then in Norman, when I got the research professorship too, there weren't too many women who had the George Lynn Cross research professorships. So, that was good. And I, as I say, I felt that I got some good breaks and I was grateful to both men and women who did things that helped me along. And I've tried to do that for some people in turn.

CP: In '92 you were promoted to chair of the department in Oklahoma and you had been acting chair for a period of time in 1981, from what I gather. How did you find administration?

MJN: Well...

CP: I've talked to a lot of people who have gone from research to administration and it doesn't always necessarily suit them.

MJN: I did it because it was my turn. I didn't do it really because I wanted to do it. I did get an increase in salary, that was nice, and we were a small department and I had a huge advantage in being a small department, in that at that time the department didn't offer undergraduate courses during the summer. And I was able to work out a deal with the dean that I didn't have to be there till the summer. I had to be available by phone or by fax. Email wasn't probably as big then. And I was very grateful for that, I mean that it was understood that I could go away most of the summer and not have to be on campus, so I could continue to do research during the summer and travel, because that had always been very important to me. I sort of dealt with the budget, learned how to do that kind of thing, and was supervising everybody.

It made me just—it was one of the things that decided me I didn't want to become an administrator. There were some things I learned as chair, though, that I very much valued, and in a certain sense I would recommend for many people, not all people, to consider, if it's their turn, to take it and try, if you don't have to do it for too long. And I—it was interesting at that stage, being a member of the inner group, in the sense of going to the meetings of just the department chairs. And at Oklahoma, unlike at Oregon State, we had a college of arts and sciences. So, it was the scientists and the human and social scientists all together in these chairs meetings. And it was really good in the sense that we'd talk around a big table and we had interdisciplinary, cross disciplinary kinds of discussions.

[1:25:00]

And I think I really became aware then of a key projection that's often made to colleges of arts and sciences, that the scientists bring in a lot more money through research grants than the arts, and that as a consequence they'll always have more power if you have a combined college. I never really bought that argument. I still don't buy it. I think that there are a lot of advantages at Oklahoma in having the combined college, and I think it gave us in the humanities and the social sciences of Oklahoma a lot more respect, to be part of a college of arts and sciences than if we had just been on our own. But I liked those meetings on the whole. And of course I like talking to scientists, so it didn't bother me too much, as much as it might have been some people who have a critique of science. Or I have perhaps more of an understanding of the scientist and what they're doing and their research needs and why they need the kind of money they have, that kind of thing.

And as someone in the history of science, as I've already indicated, I had had access ever since I was a graduate student to the National Science Foundation. So, I knew from my experiences there how grants work and how overhead works and how you can structure funding into it if you're so lucky as to have some kind of research assistance. And in my case, I didn't need equipment, I needed travel funds. Although in those days I did sometimes try to put in some money for photocopies or microfilm. So, that was part of my research expenses. But maybe, as I said, my attitude towards colleges, combined colleges of arts and sciences, conditioned by being a historian of science and liking scientists and enjoying their company, that worked fine for me.

CP: Well, another book came out a year later, *From Chemical Philosophy to Theoretical Chemistry*, with a broader focus than your previous two books. You want to tell me about how, again, how that project came about and developed?

MJN: Well, I suppose, I mean it partly had to do with the fact that I had long, in my research, been focusing on scientists and on scientific work that was, if you like, at the interface of chemistry and physics. So, physical chemistry, physics, organic chemistry, and some of the papers I have written have been on physicists. I suppose more have dealt with chemists. But I was myself interested in the interconnections and how—why is it I was calling somebody a physicist? Why wasn't I calling that person a chemist? Or is this person a chemical physicist or a physical chemist, or a theoretical chemist?

And then, again, one of the things that around that time was of interest in some of the literature, and what I was reading, had to do with disciplinary identity and how disciplines are formed, how they are maintained to conceptual elements in a discipline, but also the social elements to disciplines; how people fit together, how networks fit together, that kind of thing. And I'd long been interested in the way in which languages work and the way in which different disciplines have different languages, different ways of speaking and defining terms, so that people in biology, people who were studying genetics—well genetics may not be such a good example, but let's say people who were studying genetics have a very different vocabulary than people who are doing the core physics. And the two of them very often, unless they really are generalists, can't read each other's scientific papers, partly because of the language.

So, this project was one that was meant to be synthetic, to be sort of bigger conceptually than some other things that I had done. But then to examine the ways in which chemistry had developed from the late 18th, early 19th century mainly, up through the 1960s, fifties, sixties, something like that, with building upon different journals, different kinds of institutions, different departmental structures, different titles for people who did it, and conceptually how physics and chemistry fit together within the larger discipline of chemistry.

[1:30:03]

So, I had a lot of fun with that project because it, again, had this leeway to do social history, to look at personalities, to look at instruments and how different instruments characterize different problems you'd have to solve and then—but how you learn how to use instruments, either because you're in a certain discipline or you learn to use an instrument because you're trying to solve a certain problem. And then the language structure. And one of the things I did in that book was to develop an explanation, which is not original with me, of the ways in which chemical classification and the language of chemistry is similar to natural history. So, if there are analogies and common roots, common ways of speaking within the two. So, that book was, it was in some respects a hard book to write. It may have been, well it was one of the two hardest books, I guess, I've had to get on with. But I learned a lot from doing it.

[1:31:08]