



Mary Jo Nye Oral History Interviews, March 27, 2015

Title

“Reaching Across Disciplines as the Horning Chair”

Date

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Location

Valley Library, Oregon State University.

Summary

The primary focus of interview 2 is Nye's activities at and memories of Oregon State University.

The session begins with Nye providing background information on her husband's life and work, and also answering a question about interactions with History faculty at the University of Wisconsin. She then describes the decision to move to OSU after twenty-five years at the University of Oklahoma. She discusses the circumstances by which the Horning Endowment was made to accommodate two endowed chairs and outlines the agenda that she and her husband developed for the outreach component of the chair. In this, she reflects on the interdisciplinary mandate of the Horning chair and the exhilaration that she felt in working with colleagues from many different OSU departments. Nye also describes the ways in which close access to the papers of Linus Pauling steered her toward certain research topics

The interview then turns its attention to Nye's memories of the state of the History department upon her arrival at OSU, and various colleagues who made an impact on her during her OSU years. One individual discussed in particular is a former graduate student of Nye's, Terry Christensen, who researched and wrote on the physicist John Wheeler, in the process earning his Ph.D., despite being legally blind. Nye also recalls her association with the University Honors College and the genesis of her relationship with the University of Cambridge.

From there, Nye discusses her work researching and writing two books, *Before Big Science: The Pursuit of Modern Chemistry and Physics* and *From Chemical Philosophy to Theoretical Chemistry: Dynamics of Matter and Dynamics of Disciplines, 1800-1950*, the latter described as a particularly challenging book to write. She likewise describes her process in developing her books on Michael Polanyi and P.M.S. Blackett, before reflecting on her interest in politically active scientists and her friendship with Roald Hoffmann.

Near the conclusion of the interview, Nye shares her experience of receiving the prestigious Sarton Medal from the History of Science Society in 2006, and describes the impact that OSU has made on her. The session closes with Nye's thoughts on the current direction of OSU - including her concerns over the costs of higher education and the movement toward online instruction - and shares a few ideas on future projects that she might pursue.

Interviewee

Mary Jo Nye

Interviewer

Chris Petersen

Website

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

Transcript

Chris Petersen: Okay, today is March 27, 2015, this is our second interview with Mary Jo Nye, we are in the Valley Library, where adjacent to us there's a little bit of drilling going on, but we've been told it won't last for too long, so we will soldier on and do our best and hopefully have good sound here in a second.

So, we are going to talk mostly about your Oregon State experience in this interview today, but one thing I wanted to ask you about that we kind of got lost in the shuffle a bit in our last session was just a little bit of background on Bob, how he made his way to Wisconsin and how you guys met.

Mary Jo Nye: Well, we met in Madison. I had come from Nashville, from Tennessee, and Bob came from California. He grew up in the East Bay area, Walnut Creek; Berkeley, Walnut Creek, and had been an undergraduate at San Jose State. And when he was at San Jose State, he became very interested in history. Originally he went—well he was always interested in history. Originally he went there on a basketball scholarship and then he ended up deciding that he didn't want to spend, make that much of a commitment during his college years to basketball. So, his last year or two, I guess, he didn't play, and he had tuition with—I don't know, I guess he had, well he certainly had room and board. Tuition was practically nothing because in those days California tuition at the state colleges was minuscule.

But anyway, he had some very good professors there, one of whom, Charlie Burdick, did German history and military history. And so, Bob applied to graduate schools, came to Madison, and he was more interested in French history than he was in German history at that point. But initially he was interested in military history. And he did a master's thesis that had to do with French military history in the late 18th century. And then he—we met as graduate students, as the time we met I was finishing up and going into graduate school. And he ended up taking history of science as a minor field.

And in the end, Bob's research, although he often—it took him some time to, he still, I don't think would call himself a historian of science, so to speak, and his degree was from the History department and he's an intellectual history cultural historian, but he has tended to work in fields that also could be called the history of the social sciences, and then as he moved on, medical history and the history of biological sciences including, most recently, theories of sexuality from a medical point of view, but also from a social and cultural point of view. And his work has tended to be at that intersection of the sort of cultural and political factors that are influence in defining categories that become medical and scientific categories, or social scientific, anthropological ideas. For example, in some of his early works in how a culture and certain historical conditions and local conditions and states of mind affect what then are called social theories.

CP: Something else I've been wondering about, just really quickly, I know that History of Science and History were separate departments at Wisconsin.

MJN: Yes.

CP: Somebody who was there in History at Wisconsin right at that point in time was William Appleman Williams, who came here not that long after. Did you, either one of you have much contact with him, or have the sense of his presence?

MJN: No, we didn't. It was partly because he was an American historian. Bob was in a group of European historians and the History department—of course, one knew who he was, but the History department was huge. And there, for the graduate students there was really a compartmentalization in terms of the courses they were taking and seminars in particular. And there wasn't too much mixing of the Americanists and the Europeanists, or others. I mean, people in Slavic Studies and other fields. And in History of Science, I think I mentioned yesterday that one of my regrets in my training was that I didn't have more historical education as a younger person. I did sit in on classes in the History department. I sat in the lectures that were given by two famous Europeanists, George Mosse and Harvey Goldberg, and that was great, but it wasn't the same thing as taking the courses, at all. And particularly not the same thing as taking the exams and doing term papers.

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CP: Okay, let's fast-forward to 1994. You and Bob were very well-established at Oklahoma, you'd been there for twenty-five years, more or less, and you made a decision to come to Oregon State, give me the background behind how this all came about.

MJN: Well, I think we saw it as a wonderful opportunity in lots of ways, and Corvallis was not entirely strange to us, because Bob's sister and her husband are soil scientists, and they now specialize in hydrology and they do a lot of work, they work for the US Geological Survey. They're in Davis now. And now a lot of their water forecasting and modeling has to do with climate change, but Alan, Bob's sister's husband, got his Ph.D. here in soil science. And Laurie, Bob's sister, finished her Ph.D. here at Oregon State in 2002, after she already had been working at the USGS for some time, both in Las Vegas area and here. But we had visited them when he was in graduate school here and their daughters, our nieces, we had visited here with them before they moved, in short, to Las Vegas to work on the Yucca Mountain Nuclear Disposal Project, and then moved to Davis.

So, we were acquainted with Corvallis. We knew it was a beautiful place, we knew it was near the ocean, we knew it had wineries nearby, we knew that it had the axis of universities; Oregon, Oregon State and Portland State and the smaller institutions. And we ourselves had, for a long time, had a cabin in the Redwoods just north of San Francisco where we spent parts of summer sometimes, once we got the cabin. And so, this part of the country was a place that we had thought eventually we might retire after we retired from Oklahoma, or that we might spend more time there, if we had more time before we retired.

So, when this position came up, it just seemed like a very natural thing to be interested in. and of course it had, what it offered, among other things, were parallel positions for Bob and me, rather than one of us being offered something and the other one trying to find something at a nearby university. But it was, it could be an instant move of the two of us and an opportunity to work together and also to form a new community. We knew a few people here, but not well. And it, we were very excited about it and very glad to have the chance to come.

CP: The position was a little unusual, was it not? You shared a chair.

MJN: Well, it ended up being two chaired professorships. I mean, initially I think that it was envisioned that it would be one, but the endowment was generous and was generous enough—very generous—and generous enough that in fact, it could fund two full positions. Now, whether in fact one of us would have had a larger salary if we hadn't both been hired, I don't know, but we were quite happy with the arrangement and the opportunity and the salary and everything else. And we, again, saw it as an opportunity for building some programs here as we came in and had discussions with members of the faculty. And the committee that hired us at that time was an intercollegiate community, committee. In fact, the library was involved in that, because Karyle Butcher was on that search committee, along with members of the College of Science and members of the College of Liberal Arts. And actually, Benno Warkentin in the College of Agriculture and Soil Science, with whom our sister and brother-in-law knew, because they were in that field, he was on that search committee.

And so, we had an opportunity to talk with a lot of people in the university and really get a sense of what we could do, as the position was defined in terms of building bridges between the humanities and sciences, was one of the definitions of it, and bringing a stronger humanities presence to the university, which had a profile as a largely technical engineering and scientific institution. So, that was very appealing to both of us, because in our teaching and in our scholarship, in a certain sense that's what we'd been doing for years. I mean, working in fields that are sort of, have to do with science, have to do with humanities, have to do with the social sciences. And as I talked about yesterday, we really have enjoyed bringing parts of those different communities in the past, and this seemed like a really perfect opportunity and challenge to bring it to our job description, if you like.

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CP: So, how did you go about establishing sort of an agenda for these programs that you've referenced? Or what was your plan?

MJN: Well, there were various things that we thought about, and Paul Farber, who was of course chair of the History department at the time, and when we first were hired Bill Wilkins was the dean of Liberal Arts and Kay Schaeffer came

in as dean the same year we did, so there was a transition from Bill to Kay, and Fred Horne was the dean of the College of Science. And we tended to work very closely with Fred and with Kay, initially. And I mean we also, in the way in which we really had a sense when we came in that our function was a kind of university-wide function, because we met John Byrne. John Byrne was president and Roy Arnold was provost and they all met with, you know, they met us. And so, we felt that we were being treated in a special kind of way and that it had something we wanted to do, that we wanted to do well.

And so, one of the things that we wanted to do was to, initially at least, work out programs where we really involved individuals from different departments, particularly in the College of Science and Liberal Arts. And to bring it—we conceived it from the very beginning in terms of bringing in speakers, running themed conferences, running themed lecture series that would appeal to students in both Science and the Liberal Arts, but also Engineering and some of the other professional schools, including Agriculture. And the last collaboration I did was with Steve Strauss in forestry. So, we continue doing that kind of thing. And the Horning Endowment also provided some funds that enabled us to do some things that help support the library and more general on campus.

One of the first big things that we did the very first year we were here in the spring of '95 was to have a conference that we did with the library and special collections. And of course Cliff and Ramesh Krishnamurthy were involved with that. It sponsored—sponsorship not just from the endowment; the College of Science and other resources, the community. And we did that because Linus Pauling had died in 1994 and what we all conceived of was doing something that would sort of celebrate and commemorate his legacy. But we wanted to do something that was not just one testimonial after another. And so, we conceived of it as this idea of not just his biography and his life history but also having sessions in which we would talk about the way in which biographies are written, scientific biographies that great scientists have done, so that we invited to that conference individuals who were writing biographies of Pauling, one of whom included Tom Hager, and then people who had known Pauling well, either as his students or fellow scientists or his family and children. And we had some very prominent people here. And then from within the university, Ken Hedberg and David Shoemaker.

And then we had some historians of science who were quite distinguished and who had written biographies and who had really faced problems of how do you go about it, including how you go about doing oral histories and how do you, you know, whether or not you can trust transcripts and just how that process is involved; archival sources, testimonials, talking to people, publications, all that kind of thing. And it was a great conference in all kinds of ways. And that really, I think, sort of provided a milieu in which the Horning program, so to speak, came into existence with a fairly high profile on campus, and people had a lot of interest in it. And then it dovetailed with the Pauling Peace Lectures and that kind of thing.

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And we didn't just do Pauling, but we did lots of other things. But that really did end up, I think, setting up in terms of the kind of thing we wanted to do. And we worked very closely with Paul Farber and other people, and one of the last things we did, too, was a collaboration of the libraries in 2007, when we did the theme of the scientist as educator and public citizen. And that was a lot of fun too. But there were other occasions when we'd, well some of my greatest—some things that we did too that I really enjoyed were when we had lectures and visits from scholars and that kind of thing. We often did presentations in union, or conference in union, because it's very convenient. But we also tried, really, to do things in other people's space. So, to do something in the physics department rather than to do it in the more sort of neutral public space, or to do something, we had collaborations with theater, do things in the theater. Charlotte Headrick was very much involved in some of that. And art history. And we did things with music and collaborations with the music department.

So, that was very important to us, to have the sense that other people from the community were involved, again, sort of across the arts and sciences so that students in different disciplines would see a theme that we might be organized in a conference theme or a lecture series theme and find it appealing. And to be able to that, this was a series on which you could rely, that it was worth taking your time out for it.

CP: It must have been very exhilarating to have the means and the mandate to reach out to all these different bodies on campus.

MJN: Of course it was. No, of course it was absolutely exhilarating. I mean, it's a wonderful program, the endowment's a great gift to the university and Bob and I, in turn, have been succeeded by two historians who are doing a great job with, again, conferences and lectures. And one of the programs that we also inaugurated just a few years before Bob and I, in turn, retired, was a visiting scholars program. And that is in abeyance right now. It continued for a few years after we retired. But the idea there was to bring someone to campus who would be here for about a week, a distinguished scholar who would give a series of lectures, three, as well as meet with people on campus and often meet with people who were at the humanities center, have a meeting with humanities, and then whose lectures were published by the OSU Press. And we, one of our visiting scholars published a really fine book with OSU Press called *Aetna and the Moon* about early theories of nature in the ancient Greek and Roman period. It's a wonderful little book and she's really a terrific scholar. So, it was a pleasure for her to do that.

One of the things that I had hoped to do a little more of, but it ended up not quite working out because of technical reasons that had to do with this kind of course, the idea was to bring a scholar to campus and that scholar would give a lecture and meet with students, but then actually give a one-credit course or a brief credit course just over a weekend. But that turned out not to be so easy to implement. I mean, the idea was to bring in someone—we did it once when Jeff Ramsey was in the philosophy department and it was off—we brought in a philosopher of science. In fact, I think I mentioned him yesterday, Larry Laudan, philosopher of science, and Larry did do a seminar and the idea was that he gave big lectures on campus and he met with a group of students who were doing it for credit, but then Jeff Ramsey actually did the grading for the course so that the visiting scholar, who's not a member of the faculty, wasn't doing the grading, but a faculty member in place was doing the grading. I thought that was a good idea, but it just turned out to be a little tricky to implement, in terms of people's schedules for coming in and sort of how you do it technically, and having the right sort of group of students just to meet for a weekend.

[0:20:06]

CP: You mentioned Pauling, I'm wondering if having close access to the Pauling Papers helped to steer any of your research topics.

MJN: Oh yeah. No, I—in fact, the most recent paper that I gave had to do with Pauling, the instruments. I mean, it was a paper in which what I did was to deal with Pauling's years at Caltech and to examine in the paper the various kinds of instrumentation that were developed which he then introduced into his laboratory in a really cutting-edge way. So, beginning with x-ray crystallography and then an electron diffraction apparatus. And I talked some about computers as an instrument and computer implementation, the kind of model building he did, models as instruments, but also other instruments that required a kind of expertise, which in fact he didn't always possess himself but that, for which he had to have graduate assistants or professional assistants who in some cases help construct the instruments in the first place, because a lot of these instruments at Caltech were actually made at Caltech. And yes, I mean for this I was using sources in his collection, and I've done that ever since I came here.

I've never written a book on Pauling, but I've written a series of articles that have had to do with various aspects of his work, in which I've been particularly interested. Some of it is political activism, some of it, again, model building, the idea of molecular architecture and ways in which he transformed the whole tradition of building models, ending up with the space-filling models that were constructed, again, in his laboratory.

And one of the things I found not here—well, the papers are here but actually it was more in Caltech too, in their archives, and it was just, you probably know about this, the fact that the laboratory at Caltech in the fifties was actually making model kits and selling them to other chemistry departments around the country. And they did that for almost ten years and then decided they had to stop. And commercial space-filling models and other kinds of models came in, which the Caltech people collaborated with others and actually designed a commercial product. But that was very interesting because, in part, Pauling and his colleagues conceived of these models as not simply instruments of demonstration but instruments of research. And that was very interesting to me.

CP: Did you ever meet him?

MJN: No, I didn't. I didn't. I had one opportunity in which I might have at least seen him lecture and I regret that I didn't take that opportunity at the time because of conflicting schedules.

CP: What was your, what's your memories of sort of the state of the department when you arrived and how it evolved over time?

MJN: You mean the History department?

CP: Well, History or History of Science.

MJN: Well, fair enough. When Bob and I came here, we came from Oklahoma and Bob was in the History department and I was in the History of Science department in the College of Liberal—of Arts and Sciences. And when we came here, for the first time, well actually when we first came to Oklahoma there was no History of Science department. There was only a History department. So, when I first taught as a visitor at Oklahoma, I was teaching a history of science course within the History department, but then a core group of historians of science forked off from the History department and formed a separate department. And one of the reasons that was done there is because of the really fabulous history of science collections that is in the library there, of very rare books and first additions, but also really the journal runs and its general collection, and really a pride within the university.

But, the History department at Oklahoma had graduate programs like everybody else in the College of Arts and Sciences. And the History of Science department did too. And so we offered master's degrees and Ph.D.s, as did History. And Bob and I both had graduate students. We knew when we came here that we would be able to help reestablish a Ph.D. program in the history of science here. There had been one in the general science department that was part of the College of Science, and Paul Farber and three others had been the founding members of that department, and Paul had chaired that general science department. And that department offered master's degrees and Ph.D.s within the College of Science.

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But when the financial crush came that John Byrne had to deal with, one of the departments that was eliminated was the general science department, and the people who were in general science who still were there, were folded into other departments. And Both Paul and Jim Morris came into the History department. But when they came into the History department, they had at least one, maybe two students at that time who were studying for a graduate degree, Ph.D. So, there was a kind of grandfather clause, and I don't remember now exactly if it was going to—when it was going to run out or if it was going to last forever, but that you could get a Ph.D. or a master's degree in history of science. But it was an odd situation because the History department didn't give any graduate degrees. They were part of the MIIS program, the Masters of Interdisciplinary Studies, but there's no—and there still is—no freestanding master's degree or Ph.D. in history.

So, that was a big change for Bob and me. And one of the things that happened shortly after we came here is that we worked with Paul Farber and we worked with others to go through to the state board here and to make sure that we would have a graduate program in history of science within the History department, and that it would last and that there would be funding to support it and funding to fund some graduate students. And so, one of our challenges was to come up with a mechanism that relied on some Horning funds, but not entirely, because we weren't going to dedicate all the Horning money to that particular graduate program, but we had funding as well from the College of Science and Liberal Arts.

And so, we were able to get permission to set up that program. But then that presented a certainly novel situation in which, within the History department, you had History of Science graduate students coming in, and over time we were able to build the numbers up. I don't think we ever had too large a program. We wanted to keep it fairly small, and I think around 2000 or so we had gotten up to probably something like nine to ten graduate students who could come into a seminar as either masters or Ph.D. students, or who were Ph.D. students or masters students from another department taking our courses.

But there was some awkwardness, I guess you would say, about that. And I suppose there still is some awkwardness, because that's still the situation. And there was a brief period some years after we came when there was talk of trying to do some graduate programs that would be freestanding history programs. But the proposals really never got anywhere and I suppose, though, it was for our traditional reasons here at OSU: financial on the one hand, if it would cost money, and two, the traditional argument that you would be duplicating programs at Oregon, but also at Portland State. And I never bought the duplication arguments, I never thought that they were valid. Again, if one looks at universities in other states, there are multiple universities involved in graduate programs in the same fields, and there are reasons students want to be

at a particular university. And in addition, faculties differ and their expertise differs and in any given History department some people are stronger in a certain field at OSU than they are at U of O and Portland State, and some students might want to come study with that person in that program, just as is true of the sciences.

But in any case, the History department when we came had that sort of difficulty, and we worked our way through it. And I think that, I mean one of the things that was very important is that, as historians of science within the History department, that Bob was always trained as a historian and I was trained as a historian of science. And so, there was, yeah I had a real commitment to demonstrate to my colleagues at Oregon State University in the History department that I was truly a historian, not a historian of science who did science. And when people ask me do I do science, I'm always a little astonished. But I've gotten that, what kind of science I do. I'm a historian. But you know, I can understand the confusion but if you say you're a French historian, people don't say "are you French?" So, I mean it's not so very different being a historian of science and identifying yourself as a French historian or German historian or Japanese historian. You just have this particular specialty.

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CP: And history of science continues to stand as one of three areas that OSU offers a Ph.D. program in, in liberal arts here.

MJN: Right, yeah. So, and as I sort of indicated yesterday, perhaps implicitly, and I should just say it, I wish there were more programs, but I don't think that's, from what I can tell, not going to happen anytime soon.

CP: While we're talking about graduate studies and graduate students, I'm interested in hearing you reflect on one particular student whose Ph.D. you supervised, and it's Terry Christensen.

MJN: Oh, of course you know Terry.

CP: Yeah.

MJN: Terry is a remarkable person and he, I mean Terry Christensen came to us as someone who had had an early career at sea, was qualified in marine science navigation, that kind of thing, and had had a long career and then developed an optical nerve problem that rendered him legally blind. And as he explained to me, and as I observed him as well, what this meant was that he could see shapes but he couldn't really decipher faces easily. And in terms of reading, he can read, he can read very—texts that were displayed in a certain way. In fact, he taught me that this is how I should do my PowerPoints, and that is—

CP: He taught me to do that too.

MJN: Yeah, that's how I do mine now, this bright yellow on dark blue. He told me, you know, "don't do black on white, that's not the way you do it. Your students can see it much better if they have a contrast of the dark background with the bright color." Well anyway, bright color font. And so, his texts all have to be reverse-imaged in that way so that they're white on black instead of black on white, and they have to be enlarged. So, he can read that way but of course when it's really enlarged you read incredibly slowly that way. You can do it if you have to, but you can't really—so, I mean he developed a whole technique of listening. I would just see him sitting when he had half an hour waiting for someone or just some time between classes or something with Dutton, his guide dog, and sitting there and have earplugs, earphones in his ears, and he was listening to books, which just dumbfounded me, because he has an incredible memory.

I mean perhaps, I think he'd always had an incredible memory, because he's a great storyteller, but he could listen to books and he could remember what he'd—so he's part of that oral culture, so much of which we've lost. He could commit it to memory and then later he could use a computer in order to write it down. But he also had the computer technology that would enable texts to be read to him by the computer, so to speak. And he, I was just in awe of Terry in terms of how he went about learning, how committed he was to it and how resourceful he was in getting aid that was legally available for him to have in different kinds of institutions, including the library and the university. And I was just so proud to walk with him for his commencement when he got his Ph.D. It was a really, really important moment.

And he, what were we doing? Oh, I guess our daughter was working at Princeton and so we were going to Princeton fairly often for a few years, and Terry had moved with his wife to the Philadelphia area, and one day we met on the Princeton campus. And again, I thought how does he do this? I mean, he's navigated a system where he's taking the bus from his house to a train and now he's taken the connection to get to Princeton Junction, to Princeton from Princeton Junction and now he's walking, he's walked across campus to meet me. And I found it remarkable. With lots of questions on his mind and introducing me to people in the Princeton archives, and he did a very, very fine dissertation, which of course is of John Wheeler, the physicist, an excellent dissertation.

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And it's online through the Valley Library. And Terry has occasionally told me how many hits it's gotten. It's very, very well-read. He published an article out of it in *Physics Today*. And he did have the opportunity to meet John Wheeler before Wheeler died, which meant quite a lot to Terry. And he's doing well. He's got many, many different interests, but that Ph.D. was a real accomplishment, which I'm very glad to have participated in, to have had a role in it.

CP: I'm interested, too, in hearing you reflect on some of your teaching, especially your association with the University Honors College.

MJN: Oh. Well, when we came here, when we arrived, there was some discussion of setting up an honors college. And it, I was on a committee that recommended setting up the Honors College, as I remember. And I think I was on the committee. It was a search committee. But in any case, I think the Honors College is great. Initially, in the early years, I was one of the people who was reviewing admission, because initially people who were associated with the Honors College in some way were reading the admission essays and just sort of marking them. And I remember very well that I was dumbfounded when Joe Hendricks showed me one of the questions on there. I thought this was absolutely bizarre. The question was a question to write your own obituary. I thought "you're asking an eighteen-year-old to write an obituary? I mean what kind of question is that?"

But of course what I found was that it was much more interesting than the old stock questions of "what do you want to be when you grow up?" It was phrased in a different kind of way, because I suppose these young people perhaps had read obituaries, I don't know, I don't think I read too many obituaries when I was young, but maybe they looked at the obituary page to see how they were phrased and how they're often accomplishments but they also often have to do with a sense of civic responsibility or with kindness to others or with hobbies and habits and that kind of thing. And some of these obituaries were really creative and just clever, that's very, very clever.

And it was a pleasure to teach in the Honors College when I did teach in it. And I think as you know, I team-taught a course with Jim Krueger in the chemistry department. We had a great time with him at Linus Pauling. I think we did it three times and one of the things we did was to require the students, in writing papers, to use materials from the special collections. And when we started, things weren't available online, but even now, I mean when, even if something's available online, you can require students to come in and actually look at the physical documents. And that's life-changing for a lot of students.

In fact, I wanted to mention that to Anne, that I saw the article in the Honors College newsletter about her and Tom and the way in which they taught an honors course just recently, in which they required these students who were taking an American history course to come in and use materials that are in the archives, and how thrilling it was for some of the students, and that it is a thrilling experience, and even if you never become a professional historian, I think it's something the students remember and gives them an appreciation of the past and for real people and real times and real dilemmas that people faced.

And holding that piece of paper in your hand is one of those things that certainly hooked Bob and me on history, because I think I mentioned yesterday that we were doing dissertation research. We were—friends, families who had papers were very kind to us and gave us documents that weren't even in public archives and libraries that we had access to. And we've continued to use archival sources over the years, and they're thrilling. We love them.

But anyway, the Pauling, of course it's a lot of fun, and Balz Frei and Steve Lawson came in, talked with the students very kindly. And we had some very good students, and I think we had the students, I think we got their permission to collect

their papers and at least, I think all of them are here in the special collections in bound volumes. And one of the students wrote a paper on Ava Helen Pauling, which was the first time, I believe, that the Ava Helen Pauling papers had ever been looked at, at least in any serious way. That's what I was told at the time. And Linus Jr. Pauling saw that paper, I was told, and really liked it. He was sort of thrilled to see something about his mother.

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And of course Mina Carson has written a book about Ava Helen Pauling since then. But those courses with Jim were a lot of fun. The students were very good and we enjoyed it. But Bob, although he's retired, he's continuing to teach in the Honors College, so we still felt like that we had affiliation with it, although I haven't taught in the college since I retired.

CP: Well, we mentioned yesterday you were, you served a number of fellowships at a lot of different places and 1995, '96 was the beginning of a relationship with Cambridge? University of Cambridge?

MJN: Yeah. When we came here from Oklahoma, the year we came here was supposed to have been a full year of sabbatical for us from Oklahoma, and we had received fellowships at Churchill College in Cambridge for the '94, '95 academic year. But we talked about it very carefully, Bob and I. We talked with Paul Farber and we decided that we would come here immediately rather than taking the year off and not showing up for another year, or fifteen months. And I think that was the right decision. I think that we did the right thing, but as a consequence we had to give up the fifteen months we had planned to spend at Churchill College in Cambridge.

But they were kind enough to allow us to then take what they call a by-fellowship which is a shortened fellowship period. And so we did that in the spring of '95, and OSU was kind enough to give us leave. So, we were here full time the first term from, well we arrived here in June and we stayed until...how did that work? I can't remember now if we were here for a bit of the winter quarter, but we ended up going—because I'm trying to think about how the Cambridge system works. I mean, it's different because the system there, on the last bit of the academic year, doesn't end until the end of June or early July. But in any case, we went there for the spring and early summer. And then once we had been in Churchill at that time, we had the opportunity, if we wanted to go back again as sort of visiting fellows, ex by-fellows, if you like, and we did that a couple of times and enjoyed it a lot.

And I was beginning work then on the books that became my two books on Michael Polanyi and Patrick Blackett. And Blackett's part of—Blackett's career had been at Cambridge, so I was able to use materials there as well as at the Royal Society in London for Blackett. Polanyi had been part of his career at Manchester, so that put me in England so that I could go to Manchester occasionally, if I needed to do so.

CP: That's interesting. You published a book in 1996, I don't know where you were at the time, but *Before Big Science: The Pursuit of Modern Chemistry and Physics*.

MJN: Oh that one, yeah.

CP: I assume that was already in the can by the time you were starting these other two.

MJN: Yeah, yeah. It was—well maybe I was finishing it up when I came here. I mean, that has had—that book was supposed to be part of a series of books that was going to be published by Twain. And this, which I guess was a subsidiary of Macmillan, but it was an odd kind of thing, how it happened, because I met my deadline for getting the book in, but apparently a couple of other authors didn't and the publisher, and I'm not sure exactly, but anyway, a decision was made not to continue the series. So, it was supposed to be a series but my book was the only one that came out. And it was a hardback and I had really wanted it to be in the hands of students and I was very, very fortunate that later Harvard University Press agreed to bring it out in paperback, which made it available to students. And I've been told that it has been used in places as a textbook. So, I was very happy about that.

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But that was a book that really came out of my teaching. It came out of my research interest, but my teaching, particularly my graduate teaching, had really focused on the history of modern physical sciences. And that's a book about that, from the early 19th and the 18th century up until 1940. So, I didn't do any book with—I decided not to deal with the Manhattan

Project and with physics of the war, but to stop just right before that. And I had a lot of, I had fun with that book, and I'm glad people seem to read it.

CP: So, this was not the hardest book that you ever wrote. You mentioned yesterday that the two hardest—

MJN: Oh, no I think the two hardest that I wrote, conceptually, were the theoretical chemistry book *From Chemical Philosophy to Theoretical Chemistry*, that was hard conceptually in a lot of ways, and the book on Polanyi was hard conceptually in a similar way in terms, in part some of the philosophical, trying to understand some of the philosophical arguments to get straight some of the developments in science and technology studies and sociology of knowledge. And then in that book I decided to do, because Polanyi's interests were so wide-ranging, so there—it had to do with his chemistry, it had to do with his politics, it had to do with the philosophy of science he developed, but it also had to do with his economic views, because he published a book and wrote a number of papers that had to do with economics. And anything that—economics that was an argument against socialism and planned economies, and in particular, I guess, what he called the planning science, ultimately which became a political point of view that scientists should control science and technicians, or technocrats, shouldn't control science and there shouldn't be any central agendas that are planned and funded for what scientists should do, but they should be left to do things that they are led to do from within their own scientific communities.

But there were some things about that book that were hard. And it also was a book that was bridging Hungarian culture, of which I had to learn a good deal about, and then economics, which I had to learn a good deal about, and then years in Germany. And so, I had to learn a bit more about the structure of what was then Fritz-Haber Institute. And then the years at Manchester, which brought me back to more familiar terrain, because at the time I'd finished the Polanyi book, I had done the work on Blackett, so I was more familiar with the British sort of work. And Polanyi and Blackett were colleagues at the University of Manchester for a brief period, and although they were politically very different from each other, they were quite good friends.

And originally I had thought that I would write a kind of dual biography, a double biography contrasting and comparing the two of them, sort of using the theme of the friendship, despite their fundamentally different views about how society should be organized and how the economy should be organized, although they pretty much agreed on how science should be organized, in the end. But it didn't fall—it fell apart as a double biography. And in fact, when I, I had NSF money for some of that research and one of the referees for that project said "this will fall apart, but it's interesting enough, give her the money, let's see what she turns up." So, it became two books. I did write one article, which had to do with the two of them together.

CP: So the two books, the idea for the two books was originally one book and it became two books.

MJN: Yeah.

CP: Interesting. Was the, you did three stints at Max Planck Institute, was that feeding into the Polanyi work?

MJN: Yeah, yeah. I think when I was there the first time I was finishing up the Blackett book, but I was beginning work on Polanyi, and the Max Planck was just a great place to be. And in fact, one of my thrills, one thing that I was just very pleased about is that the Max Planck Institute for the History of Science is part of the whole Max Planck Gesellschaft conglomeration, which I think has seventy or so institutes. And Michael Polanyi, in the 1920s and early 1930s, had been at the Max Planck Institute for electrochemistry and physical chemistry, which is in a suburb of Berlin called Dahlem, and at that time it was run by Fritz Haber, a great chemist, and controversial because of his role in the development of gas warfare during the first World War, and his actually turning his institute into a kind of factory for the production of gas, as well as research on gas.

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But after I had finished the Polanyi book and it had come out, I was invited to give a talk at what is now called the Fritz Haber Institute, which is one of the Max Planck Gesellschaft institutes. It's the original place where Polanyi worked and I was able to give a lecture in the building where he originally had his laboratory and where he was employed. So, that was a real pleasure, and it was a real pleasure to be invited by the people there.

CP: The Polanyi book received a lot of acclaim. Do you regard it as your most prestigious work, or do you have any ranking of the books that you've written, the ones that are your favorites?

MJN: Oh I don't know. I think, I mean I had a lot of fun with the Blackett book and I'm very pleased with the way that book turned out. The Blackett book, one of the criticisms of the Blackett book was that it wasn't long enough. In fact, Francis Everitt made that claim; he's a physicist at Stanford and he knew Blackett and worked with him, and I was so pleased that Francis liked the book, and he gave it a blurb, but he said, and other people said to me, "it should have been three times as big." It was a pretty small biography. There's a lot of room for somebody else to do a major study of Blackett. I mean there are some, there's a very big book, for example, on Heisenberg, and of course there are lots of big books on Einstein. And you could, because of Blackett's, again, multifaceted career; he deserves four or five hundred pages, not just the sort of not quite three hundred pages that I gave him.

But I was really happy to do that book. I learned a lot about operations research, because he worked in, some of his work had to do with movements of the earth's crust, and this kind of in the early days before plate tectonics really was developed as a theory. His work fed into that and that was something new for me. So, I've enjoyed moving around in those ways. And I guess I have tended to the interests—I have tended, when I have focused on individual figures rather than to develop an argument about the development of theory or something like that, I've tended to focus on people who often had strong political views and who were politically active and who had complicated lives. And one of the reasons I didn't do a full-scale biography of Polanyi is not—it's a biographical study, again, I wouldn't say it's a biography—that was partly because I really had no hopes, in a limited time, of learning Hungarian, which I would have had to do to do a real biography, a really serious biography.

And a very good biography of Polanyi had appeared just a few years before mine came out, co-authored by a physical chemist who had died before the book was actually published, Martin Moleski, who was int—so that Bill Scott was interested in Polanyi's scientific work, but also actually his religious views. And Martin Moleski was interested in his religious views but also in his philosophy of science. And they did a very good job. Bill Scott I think, I'm pretty sure doesn't read Hungarian, but he had assistants who did, who could work with him for some of the Hungarian texts. And most of Polanyi's scientific work is done in German or English, not Hungarian. Just his really early work is in Hungarian.

But the correspondence often, I mean this is one of the things you run into. For example, Polanyi's brother, Karl Polanyi, was a very famous economic historian, a very important one. They both were refugees from Hungary. They emigrated from Hungary and then they eventually left Austria and Germany, so they had two periods of refuge from places where things had turned politically bad for people who were Jewish. And their correspondence is available, but they often did write in Hungarian. But then sometimes they didn't; they'd write in English or they'd write in German. It's very interesting how people do those things. I mean, you have people who are truly multilingual, sort of write in one language depending on their situation or switch within a single letter between languages. I mean, that's common for anybody who's truly multilingual. But, if the language is Hungarian, it poses a real problem for some of us.

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CP: Why do you think it is that you've been particularly interested in scientists with this political angle as part of their persona?

MJN: Well, I guess I've followed politics since I was a young person. I've just been interested in trying to learn more about political cultures and how they affect people's attitudes; how they affect what people do as well as affecting people's attitudes towards each other and how people can live. And certainly, in the history of science, since I ended up working, from the very beginning, mostly in the late 19th and in the 20th century, that period of forced immigration of so many Jewish scientists is one that really requires you to look at politics, but then you find you have to look at it earlier. You have to look at antisemitism, which then brings up all kinds of questions about ethnicity and migrations and that kind of thing for people who are not Jewish, who chose to come to the United States or just more who chose to go other places.

France, I was doing my early work in France and Jean Perrin was a Nobel Prize winner for his work on atomism, and atoms and molecules, actually calculating the size of molecules. But he was—I didn't touch it in my dissertation, but one of my first articles that I published in French historical studies was on his politics, because he was a member of the Popular Front in the 1930s; he was very politically active. As a young person, he signed petitions that had to do with

the arrest and incarceration of Alfred Dreyfus during the so-called Dreyfus Affair in France, and he was a socialist, very proud socialist. And initially, in my dissertation, I dealt with his scientific work and a little bit that had to do with philosophy of science and philosophical arguments about atoms and Molecules and whether they were real or whether they were simply instruments from developing better theories and that kind of thing. If you can't measure them, then they don't exist, so he measures them.

And I think I said yesterday that my dissertation was contained, and I wrote it as a study in experiment, but with a philosophical twist too, with a few, a little brief biography at the beginning, but not much. But I, you know, you couldn't look at Perrin without thinking about how he's spending his time, and he was spending a lot of his time in demonstrations and rallies and working on government committees. He was in government cabinets, and then when France—he was out of the country, he was in New York when the Germans came to Paris in 1940. And so, he was part of the French resistance, but from abroad, and he died before he could get back to France. But he was a man for whom politics was part of what life was all about, and one of his best friends was Paul Langevin. He was part of—Perrin was part of the Curie, Joliot, Langevin circle. And of course they were all political left, and of course that intrigued, I should say of course it did intrigue me. And in the end, I've done some work where I tried to combine in one big study these full, faceted lives.

CP: I know that you're friends or at least acquaintances with Roald Hoffmann, are there other, more contemporary scientists who intrigue you?

MJN: Well, Roald is of course something; he's very, very special. In fact, when I was, well to answer your question, not quite in the way that Roald does, I suppose. I mean Roald's work as a major scientist, but also as a playwright and poet, it was just simply remarkable. And he, of course, has a complicated history as well, and he's a very kind and gentle man. And I was just in touch with him just actually within the last week or so, because when I was in, I was in Japan recently and I met a woman who is also remarkable in her own right, Japanese woman whose interest has been in France. In fact, she presented a paper at this conference on—it was fascinating—on women scientists in the Radium Institute from 1911 to about 1934 when Marie Curie died. And there were over forty members, like forty-four women, many of whom had rather distinguished careers. I knew nothing about this.

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So, this Japanese woman speaks, I was told by someone else who's French, speaks fluent French, really, really good French. And she was speaking very good English, because she was speaking to me. Although I guess we could have spoken French, but her English was just really, really good. And then it turned out, I can't remember how it came up, that she has translated into Japanese Roald's last play. And she is hoping to be able to get some funding to have a performance of it in Japan, there in Tokyo. Isn't that amazing?

CP: Hmm.

MJN: So, I'd written Roald just to tell him that I had met her and that I understood that this was going to be the case. I mean, someone else who I got to know a little bit and very much admire is Dudley Herschbach. I mean, he's also very, very special to me as a scientist. And he's interested in the history of science, the history of chemistry, and he's written some very interesting pieces that are historical in nature and reflections on work that developed historically. He's great and I admire his career. And of course he was, he knew John, he knew well John Polanyi, Michael Polanyi's son. And so, among other things, Dudley sent me a photograph that I could use in my book that was taken from—in fact, Dudley has in his office this photograph of Michael Polanyi with his son John Polanyi and Eugene Wigner, who was a very good friend of theirs as well. Or maybe it's just John and Michael in that particular photograph. There's another one too. But anyway, Dudley, among other things, has a personal connection to the Polanyi family, and that was one of things that sort of, at a certain point, got us in touch again when I was working on the Polanyi book.

CP: Well, certainly a high-water mark of your career was in 2006 when you received the Sarton Medal from the History of Science Society, the most prestigious medal that the society awards. Can you take me through your memories of your experience at that time?

MJN: Well, it was a thrill. It was quite a thrill. I don't know what else to say about it. I mean, it was very, very special. I had, I was taken by surprise, and it was a nice surprise, and it was an absolutely lovely evening in which I was given

the award, and my daughter flew out for that and of course Bob was there with me and some of our students from the OSU program were there and some of my former students were there, including Terry Christensen was there. It was in Vancouver, British Columbia, where the award was given. It was a History of Science meeting, was meeting in Vancouver at its annual meeting.

But Terry was there and one of my students from Oklahoma, who is one of, who's now a professor in Japan at Nihon University in Tokyo was there. In fact, he had planned to come to the History of Science Society meeting that year. He doesn't often come, but he had planned to come, and knowing that he was coming through, we invited him to come to Corvallis, and he gave a very nice colloquium to our students here in 2006 on the history of science in Japan, how it's practiced, who is doing it, what kind of issues Japanese historians of science are interested in, something entirely new to them, and in some respects to me as well. But he was there, and I have a photograph that I just very much treasure of Yasu and Terry together. So, Terry was my last Ph.D. student here and Yasu was my first Ph.D. student at Oklahoma. So, that's very nice, isn't it?

CP: Mhmm. Well, you retired from Oregon State in 2008, you and Bob continue to live in Corvallis, and just this past January OSU held a festschrift in your honor, probably a good time to reflect, I suppose, on your association with OSU. Can you share some of your thoughts about just the time that you spent here and what OSU has meant to you?

[1:05:11]

MJN: Well I, Bob and I both were, as I've said earlier, we were very pleased to come here initially. We were ready for change, we had been in Oklahoma for twenty-five years, and it was difficult leaving Oklahoma, in a way. The place had been very good to us, the university had been very, very good to us, and we have many close friends and colleagues there. In fact, we were just back there recently for our friend's ninetieth birthday. But it was thrilling to sort of start all over again in a new place. And Oregon State, when we arrived, everybody was very welcoming to us, and we were extremely fortunate in coming here as senior scholars in the privileged position of having endowed professorships, which gave us budgets and which gave us prestige, and we were able to work with Paul to get a graduate program going that had a core to it that—we've had some excellent students in that program, of whom we're very proud and with whom, most of them with whom we're friends and keep in touch.

And the sort of, the sense of building something here, or rebuilding, I mean there had been a History of Science program, but then also just helping with Paul to build up the History department in a certain way, because we worked together to bring in some very good young people, and many of whom, not all of whom, but many of whom are still here in what's now the School of History, Philosophy and Religion. And as I've said, we had the opportunity to work within our college, as once within our department and across colleges, and in a way, either being expected to do so or feeling that we would like to do so, but having means to do so gave us a sense of being university members, in a very wide sense.

I mean we always, we still have maintained our larger professional ties, national and international and that kind of thing, but we felt very much a part of a community, which is why we stay in Corvallis, we're not leaving. And we still go to the chamber music, we still come to lectures on campus and Bob goes to Dixon regularly, and I go somewhere else, I don't go to Dixon. And we're very happy to be here. And of course the library is still here for us, special collections is still here, for me in particular. And all of you as friends are here. So, it's a good place.

I will say one more thing, and that is I was really quite sort of astonished initially, and I'm so pleased and so happy, when after John Byrne retired as president, he worked it out with Peter Copek that his office would be at the Humanities Center. I thought to myself "how absolutely wonderful." I mean, here you have someone who is a major marine scientist, who has been president of this institution and who's been internationally, nationally recognized, and who I also see at chamber music and theater, and he's chosen to have his office in the Humanities Center. What a statement. So again, we've had a sense that there are people that we've known here since we arrived, and we'll go to the Humanities Center and we see John there, which is nice.

CP: Yeah. Well, this project is being produced, I guess, in commemoration of an upcoming major anniversary for OSU, and this is a time of great change here at the university, and one of the things that we've been asking just about everybody that is part of this project is to give a prospective on how things are changing, where they see the university going at this point.

MJN: Well Chris, I can only begin by saying that I guess I'm a traditionalist. And so, like a lot of my colleagues at public institutions all across the country, not just Oregon State, I have some misgivings about the consolidations that have taken place of departments, creating schools which sometimes don't fit together very well in terms of individual disciplines and expertise, and my own department, the History department and the School of History, Philosophy and Religion, that has some comparison to, there's some other schools that I don't, which I don't see the commons. And I think that individual—I preferred the individual department structure that existed when I came here. We had that at Oklahoma; that's what I experienced at Vanderbilt, Wisconsin, although there were interdisciplinary departments, certainly, but there also were the individual departments. And we were able to collaborate across departmental lines.

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So, I realize there are financial, perhaps intellectual reasons, but also sort of centralization management techniques that have become very prominent in lots of universities that go along these lines. Like lots of my colleagues, particularly my age, I have some misgivings about the e-courses. I think a few e-courses are fine, but I think that, and I understand this has always been the case, there are students who have full-time jobs, and we used to call them older students but unfortunately now they include some younger students, because tuition has gone so high, which is another matter, related matter for public universities, because of the way which legislatures have defunded them and insisted that they go on their own.

And so as a consequence, we're building a two-class society. And the private universities, for the most part, those that exist right now will probably do just fine as the very elite education. Then there are the public universities, and within public universities, they're going to become more elitist than they used to be. Not like the privates, but students, we all know about student debt and how difficult it is for students to pay without taking out large loans.

And e-classes, I meant to look before I came over here, and I think I got distracted by something and didn't do it, at just what the fee structure is for students for the e-classes and then to actually get an online degree. I know that they are lucrative for the university and for—that they've become a way of helping to fund departments, because departments lost the funds from the university because the universities lost the funds from the legislature. So, there's a financial incentive to departments, and to some individuals to give them. I don't guess the MOOCs are here yet on our campus. We have a couple of colleagues who are doing MOOCs now, and part of those colleague's rationales is that they can reach a larger audience of students than they ever could on campus.

Well, maybe so, but isn't this sort of all working at odds with what we've been told about how education works? We're told over and over again that education works best in small classes that, certainly laboratory science, but I would also argue work in the humanities and social sciences is hands-on. And you can't do that kind of hands-on thing through a computer screen. Although with e-courses I gather there can be communication between the teachers and students, I have had people who were teaching e-courses on this campus tell me, comparing the two classroom format and e-course format, how unsatisfying they find the e-course, that they don't get to know the students.

I mean, you can't follow up in the way that you can in the classroom. Students learn from each other, too. And we learn from students and there's a give and take in the classroom, not to mention the lab and fieldwork and that kind of thing, but there's a give and take in the classroom that I think is absolutely essential to education. I think I would feel very alienated if my education had all been, well my courses, if I hadn't had the experience of knowing people and knowing faculty and my professors and interacting with them and feeling part of a group, part of a community. So, I have my misgivings about that.

Of sports, where—I probably should stop, I mean I think, you know again, I don't, I mean I think that the ways in which Oregon State is changing, I mean obviously we're, there are some things that are very good about the way the university's changed over the last more than twenty years that I've been here in Corvallis, and that's good. The university, when I came here, there were about fourteen thousand students, both undergraduates and graduates on campus. Now it's thirty thousand. That's affected the town, as most universities, you see it everywhere. One reads over and over again about sports facilities around the country, as well as the competition for students by building new recreation centers, as well as by building football teams. And again, it's partly money-driven, because universities everywhere, not just here, are after donors, because their legislative—public universities—legislative funding has dropped off.

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And I think it's a real shame, because we used to have a system of public education in this country, supplemented by private education, that was the pride of this nation. And I have just witnessed its slow demise over the last decade. And it's, I mentioned that Bob at San Jose State—I went to private school initially, Vanderbilt, and then I went to a public university, and I've taught in public universities, although I have taught as, not regular faculty, but other places.

But, you know Bob went to San Jose State and California was just an absolute model in the fifties to sixties, even the seventies, the system, the university system and Cal State system, and then the community colleges later. They really did make education available to everyone. Bob's parents were not college-educated, but all three of their children were college-educated in the California system. One is a professional veterinarian, one is a scientist with USGS, and then there's Bob. They all have higher degrees. And the California public education system helped make that possible. I wish Oregon had more of a tradition and more of a future, I guess you could say, in funding public education and making it possible for people to stay here who want to stay here and possible for people to come here who want to study with some of the stellar disciplinary experts in the sciences and humanities and social sciences who are here.

CP: Well, you retired in 2008 but you mentioned you just gave a paper in Japan?

MJN: Yeah.

CP: Last month, this month? The last question I have for you is just what's on the horizon for you?

MJN: Well, I'm not sure. Right now I'm writing a series of papers. I don't have a big book project in mind at the moment. It might happen, although it probably wouldn't be big, it'd probably be small, but I've been working. One of the themes with which I've been working recently has to do with collaboration with scientists in the sciences and co-authorship patterns among scientists and how that's changed over time, and the factors that have driven those changes. And Pauling has been one of the scientists that I've looked at in terms of his patterns of co-authorship, and first authorship and shared authorship, the way in which his laboratory was organized and why it was, that kind of thing.

And I've looked at some other scientists too, and so I have been, as I've mentioned, sort of looking at instrumentation a little bit more than I used to do, although when I've—my first project, Jean Perrin, was I was concerned with the kind of instruments he was using for measuring molecular radii and that kind of thing. But I've got several things I'm working on right now, and I'm scheduled to give a couple more papers, and we'll see what happens.

CP: Terrific. Well, I really appreciate this Mary Jo, the last couple of days have been very enlightening for me, and I appreciate you spending the time to share your story with us.

MJN: Well thank you Chris, very much. It's a pleasure to talk with you.

[1:19:26]