SCIENCE HISTORY INSTITUTE

JEAN KANE

Transcript of an Interview
Conducted by

Benjamin Gross

at

Culpeper County Library
Culpeper, Virginia

on

27 and 28 February 2012

(With Subsequent Corrections and Additions)

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> Jean Kane, interview by Benjamin Gross at Culpeper County Library, Culpeper, Virginia, 27 and 28 February 2012 (Philadelphia: Science History Institute, Oral History Transcript # 0881).

Science History Institute

Chemistry · Engineering · Life Sciences

JEAN S. KANE

1940	Born in Poughkeepsie, New York on September 29

Education

1962	BA, Keuka College, Chemistry
1964	MS, Mount Holyoke College, Chemistry with Thomas O. Zajicek at the University of Massachusetts

Professional Experience

	Douglas Freeman High School, Henrico County, Virginia
1964	Math Teacher
	RCA Laboratories, Princeton, New Jersey
1964-1967	Research Chemist
	Freehold, New Jersey, and Fairfax County, Virginia
1973-1976	Degree Substitute Teacher
	United States Geological Survey, Reston, Virginia
1976-1988	Research Chemist, Geochemical Analysis and Method Development
1988-1990	Research Chemist, Coordinator USGS Geochemical Reference Sample Program
	National Institute of Standards and Technology
1990-1995	Research Chemist, Project Manager in the Standard Reference Materials Program
1988-present	Member of the Editorial Board of *Geostandards Newsletter*, later renamed *Geostandards and Geoanalytical Research*
1997-present	Member of the International Association of Geoanalysts, and at various times a member of Governing Council, the Proficiency Testing Program's organizing committee, and chair of the Certification Committee

Honors

1960, 1961	Research Experience for Undergraduates, National Science Foundation
1992	Invited speaker at Open University meeting titled *Geoanalytical Techniques: Current Capabilities, Future Potential*
	Guest editor for Geoanalysis Conference Proceedings
1997	*The Analyst* (v. 22, # 11) following Geoanalysis 1997
2004	*Geostandards and Geoanalytical Research* (v. 28, #1) following Geoanalysis 2003
2004	Guest lecturer at the National Research Center for Geoanalysis in Beijing, China, for three days; invited as IAG Certification Committee chair; spoke not only at the NRCG but also at the Chinese Bureau of Standards, Metrology, and Inspection
2009	Special Commemorative Session organized at Geoanalysis 2009 in South Africa to honor work in the IAG since its formation

ABSTRACT

Jean S. Kane grew up mostly in Tenafly, New Jersey. Although her father was an accountant, Jean was the first in her family to attend college. She began at Keuka College, intending to get a nursing degree, but she discovered chemistry and changed her major. By her senior year she had finished all Keuka's science and math courses and, with Margaret Cushman's help, entered Mount Holyoke College and obtained a master's degree in chemistry. Kane wrote her thesis with Thomas Zajicek at the University of Massachusetts; there she also met Robert Kane, a chemical engineering graduate student whom she married.

Moving to New Jersey, Kane got a job at RCA, working on potassium tantalum niobate under John van Raalte, and solid-state crystals under David Kleitman. She left RCA before the birth of her second child and volunteered with the public schools while her children were young. The family moved to Vienna, Virginia, for her husband's next job, and Kane found employment at the United States Geological Survey (USGS) in the Branch of Analytical Chemistry, working mostly on atomic absorption spectrometry and publishing about method development research. Inductively conducted plasma optical emission spectrometry (ICP-OES) replaced atomic absorption spectrometry (AAS), as it greatly increased the efficiency of sample testing. Kane took over the Geochemical Reference Sample Program at USGS, which attempted to categorize and standardize geological samples according to their chemical composition, using analyses from labs all over the world.

Kane was recruited to the Standard Reference Materials Program at National Institute of Standards and Testing (NIST). There she was manager of about ninety reference materials; her customers included laboratories from all over the world, labs seeking a wide range of materials. She managed the certification of forty or so reference materials while at NIST and standardized the certified values, as required by the International Organization for Standardization (ISO). Retiring from NIST, Kane remained on the editorial board of *Geostandards and Geoanalytical Research,* and took an active role in the leadership of the International Association of Geoanalysts (IAG).

Kane discusses her feeling that the concept of materials standards is esoteric and theoretical and error-prone. She explains some of the difficulties controlling ultimate standards and data collection. International Association of Geoanalysts (IAG) requirements strengthened the data's reliability. Kane's contribution of greater precision in analysis and standardization of methods is widely acknowledged. Finally, Kane advises women interested in pursuing chemistry to follow their inclination. She says the subject is fascinating; women have become accepted in upper echelons of the workplace; affordable child care and workplace flexibility are more available than they were during her early career years.

INTERVIEWER

Benjamin Gross studies the history of corporate science and the American consumer electronics industry. During his postdoctoral fellowship at the Chemical Heritage Foundation's Institute for Research, he oversaw a variety of projects related to material innovation. He also served as curator of the Sarnoff Collection at The College of New Jersey and oversaw the development of *Innovations That Changed the World*, an exhibition on RCA's contributions to the history of electronics. Dr. Gross earned a PhD in the history of science from Princeton University and in 2018 published *The TVs of Tomorrow: How RCA's Flat-Screen Dreams Led to the First LCDs*. He is currently Vice President for Research and Scholarship at the Linda Hall Library of Science, Engineering and Technology.

ABOUT THIS TRANSCRIPT

The Center for Oral History, Science History Institute, is committed both to preserving the recording of each oral history interview in our collection and to enhancing research use of the interviews by preparing carefully edited transcripts of those recordings. The preparation of interview transcripts begins with the creation of a verbatim typescript of the recording and proceeds through review and editing by staff of the Center; interviewees also review the typescript and can request additions, deletions, or that sections be sealed for specified periods of time. We have established guidelines to help us maintain fidelity to the language and meaning of each recorded interview while making minor editorial adjustments for clarity and readability. Wherever possible, we supply the full names of people, organizations, or geographical locations mentioned during the interview. We add footnotes to the transcript to provide full citations for any publications that are discussed, to point to extant oral history interviews, and to clear up misstatements or provide context for ambiguous references in the transcript. We use brackets to indicate the addition of material that was not in the audio, and bracketed ellipses to indicate the deletion of recorded material. The transcript also includes time stamps at five-minute intervals. We omit without noting most instances of verbal crutches and all instances of nonlexical utterances. We also make small grammatical corrections where necessary to communicate interview participants' meaning. Finally, staff of the Center create the abstract, chronology, table of contents and index.

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INTERVIEWEE: **Jean Kane**

INTERVIEWER: **Benjamin Gross**

LOCATION: **Culpeper County Library**
Culpeper, Virginia

DATE: **27 February 2012**

GROSS: [. . .] It is 27 February 2012. I am here [at the Culpeper County Public Library] with Jean Kane. We are going to be talking about her life and career as a woman in science.

So normally I like to begin these interviews with, well, the beginning of the story. So if you could please start off just by talking a little bit about where you were born, where you grew up, and a little bit about your family, that would be great.

KANE: All right. I was born in 1940 in Poughkeepsie, New York. We lived there very briefly. I don't really know when we moved to Westchester County, also in New York. My sister [Chloe] was born [one] and a half years—eighteen months—later. There were the two of us until 1946, when [my second sister, Alice] was born. By then, [my family] was living in Tenafly, New Jersey. I remember Tenafly very well. We were in a very middle-class neighborhood, that the street was nicknamed Fertility Road, because the large number of children, roughly elementary school age who lived within six or eight houses of each other, up and down the street. I remember that home very, very well, and enjoying my sisters. The younger one, as I say, was born when I was in first grade.

I probably was four when I learned to read and drove my mother crazy because every time she would pick up a newspaper or a magazine, I would lean over her shoulder and read to her every word I could recognize. But clearly, they weren't all the words in what she was trying to read. This was nonstop and perpetual. In fact, she's got pictures of me in a snowsuit in a rocking chair outdoors in the middle of a snowstorm reading a book because I'd been told I had to get out of the house once in a while. [. . .]

[Mom] had so much difficulty in those early years getting me to <**T: 05 min**> do anything outdoors. [. . .] She was always concerned that I just had to learn to know the outdoors in a way that she couldn't help me do. So we went to summer camp, Eagle's Nest Farm on the Delaware River, run by the church we went to. Oh, how I loved summer camp. [. . .]

GROSS: Now how old were you when you went to summer camp the first time?

KANE: I went to summer camp the first time just before—I had a September birthday. You were supposed to be eight to go to summer camp, but my eighth birthday was going to be at the end of the camp season instead of before it started, so my mother did a lot of pulling strings to get me in that first year, when I was seven and three-quarters, rather than eight years old. But I stayed with the camp through high school. I did their counseling training program one year, and then spent two years there as a camp counselor. [. . .]

GROSS: A quick question or two. [. . .] Could you tell me a little bit more about your parents?

KANE: Mom [. . .] always wanted us to do very, very well in school, and basically we did. I can recall going to mom and [saying], with a C in Art, in eighth grade or something of this sort, and being very upset because she expected to see A's and B's. Her comment was, "Well, you're not as good as Michelangelo. Why would you expect an A?" It was years later, before I thought, "Well, Mom, I wasn't as good as [William] Shakespeare either, but you did expect A's in English. Wasn't as good in [math or] science as [Albert] Einstein, but you did expect A's in [those subjects]." So there were those kinds of disconnects in her ability to think about and relate to some of our feelings and perceptions of things.

My father was a certified public account. He actually became an accountant through, I think, through the mail . . .

GROSS: Like a correspondence course?

KANE: Correspondence course. But he ended up—about the time we moved to Tenafly, which would have been [1945], [Dad] started with Arthur Andersen [then and] moved up through the ranks. By the time he died, he was a partner in the firm—back in its glory days, clearly. But he got in without a college degree only because he was entering during World War II, when so many other college-educated men they would have preferred to hire were in the service and totally unavailable. Despite being the right age, because he had two pre-Pearl Harbor children, he was not drafted, so that gave him an opportunity for a career that otherwise could never have taken place . . .

GROSS: Interesting.

KANE: Quite honestly. But as I say, by the time he died, which was 1964 . . . yeah, he was fifty, he had been a partner for two or three years. Actually that partnership came about because during my senior year in high school, Arthur Andersen asked him to go to Paris [France] for six months. In going to Paris, he left vacant his [position in the] small business division where he was a manager for the time he was away. They suddenly realized how much they counted on him in a way that, <**T: 15 min**> again, without the college degree, without the credentials, without any number of other things, but that six months away gave them a new appreciation of him and his capabilities and what he brought to the firm.

GROSS: So when he came back . . .

KANE: So when he came back he was made a partner. Of course, [those] six months that he was in Paris were the last six months of my high school years. My father had made up his mind that, yes, he was going to take the assignment, but the family was going to come with him.

GROSS: So you were also going to go to Paris.

KANE: I was also going to go to Paris. It was a very traumatic family situation for a while. I still do not know whether my parents contacted the school principal or whether being a very small high school—there were thirty-nine or forty of us in the graduating class—whether the principal at the high school just picked up the vibes from what was going on, you know, because I was talking to teachers, friends, whatever. But just before my father was going to leave, and we were all going to have to go with him, and [my parents were thinking], "What difference does it make what year you graduated from high school? In the long run of your life, nobody will care. It won't really make any difference." Of course, we were saying "It's going to make all the difference in the world. How can you possibly say this?" But the principal fundamentally said, "If your father's leaving in early December, if you can find a way to stay here until [midterm] exams in January, and then go meet your parents over there, I will let you drop out of school for six months, come back, and reregister the day before final exams. If you pass the finals, you graduate with your class."

GROSS: Oh, wow.

KANE: I mean, this was just totally incredible. But in so many ways, it's the whole story of my . . . you know, go back to getting into summer camp when I desperately needed that experience and breaking all the rules to get in early. Then we go on to high school, and it's graduate in spite of the fact that you took the second half of your senior year off. I took the books, took course outlines from all the teachers, took everything with us, and mailed things back once every two weeks or so. But basically finished my senior year as an independent study project.

[The whole faculty generally, once the principal said the girls are going to do this—drop out, go to Paris, re-register to take finals—the faculty as a whole was enormously supportive.

They were wonderful about it. They were, quite frankly, wonderful about finding a way for me to do geometry, when I would otherwise have missed it and needed that math course. So in general, I was very, very happy there. I mean, my sister was a cheerleader. I was manager of the drum majorettes. I was editor of the year book. I was on the newspaper staff. You know, just into everything.]

GROSS: That's astounding.

KANE: Well, it keeps going from there, because I basically did the same thing leaving Keuka [College] and getting to Mount Holyoke [College].

GROSS: Well, not let's skip too far ahead, yet.

KANE: Not skip too far ahead, but that's what happened there. [. . .]

GROSS: Did you and your family talk about current events or politics at home much?

KANE: Not really. [. . .] [Rather little] of that kind of conversation.

GROSS: Okay. So let's talk a little bit more about, I guess high school, because that's where you seem to have the clearest recollections. When you got to high school for example, were you already interested in science?

KANE: No. [I began high school at Holy Angels; I was there from eighth grade through the middle of tenth grade.] I don't even think they had a science course. They might have, but I don't have any recollection of a science course at Holy Angels. [. . .] My [family] moved out of Tenafly into Park Ridge [New Jersey] midway through my tenth-grade year. The commute to Holy Angels was no longer even remotely <**T: 25 min**> possible. I mean, couldn't have gotten there. So I went to Park Ridge High School which, as I've already mentioned, they were a very small high school. They had just recently opened a regional high school that took care of three or four other surrounding communities that had previously gone to Park Ridge, so we were what was left. It was tiny, tiny. But also because of the timing of the move, there was a lot of difficulty making course schedules fit. Geometry had been a sophomore subject at Park Ridge, but it was a junior-year subject at Holy Angels.

GROSS: So you hadn't taken it at the time?

KANE: So I hadn't taken it, but I couldn't fit it into my junior year, except that, again, they let me do an independent study. I went through the geometry in six months of the nine months of the school year, gave me the final exam. Not quite knowing what to expect from the way I'd gone through the course, [they] ended up telling me it was the highest final exam score they'd had in five or six years of school.

GROSS: So you had a bit of a talent for mathematics.

KANE: So [I've] got a bit of talent for mathematics. It seems to me that biology and chemistry in high school were the only . . . there may have been a physics course in high school, but I don't remember that that well. [. . .]

GROSS: Were there any teachers that really stood out as role models or kind of inspirations?

KANE: I don't remember the gentleman's name [John Burke] who did the geometry. He also did second-year algebra, and Mr. [Clinton] Byer was the one who did matrix [algebra], or he did solid analytical geometry, or something like that <**T: 30 min**>. Basically, I really liked both of the math teachers very, very much. There were a couple of English teachers who were topnotch [teachers and] first-rate people, one of whom [Evelyn Long] I stayed in touch with until after I left [for] college. So yeah, there were a number of teachers that, if you were going to go into teaching, would have been wonderful role models. Most of them, frankly. [. . .]

GROSS: So [. . .] at that point, did you have any idea in mind kind of where you wanted to go career-wise?

KANE: Not really. I knew that [. . .] I was going to be the first-born in the family, cousins, nieces, you know, anyone, to go on to college. There was no doubt that I was going to go to college. But [from my parents' perspective] there was also the sense that teaching and nursing are appropriate careers for women. I enrolled at Keuka College with the intention of getting a bachelor's in nursing.

GROSS: Why Keuka, anyway?

KANE: Why?

GROSS: I never heard of it before.

KANE: Yeah. Well, why Keuka? Again, a very small high school, very limited in the way of the guidance department. I don't know whether the guidance counselor was actually a Keuka graduate herself, or had simply sent two or three other students before me there. It was a college she was very familiar with. It did have a nursing program. The other factor was that, once we had been in Paris for the last six months of my senior year, my parents didn't know if my father would be sent back to Paris after that, while I was going off to college. The aunts and uncles were in New York State. [My parents] wanted [it] to be easy for them, if they have to take care of [me]. So [Keuka] was more a convenience in that regard, and definitely a very good school in terms of a nursing major. But there wasn't the seeking out the ideal college for the student that there is today, back then, especially not for women.

GROSS: Certainly. Did you receive any sort of scholarship?

KANE: There was a small [. . .] Strong Scholarship through Park Ridge [High School]; again, it was for Keuka College, specifically. That had to have been . . . it was probably five hundred dollars. I don't recall . . . again, the details are long since gone. [. . .]

GROSS: Right.

KANE: I had both the scholarship and was in the house [Strong Hall] that gave you [an on-campus job].

GROSS: So what sort of work did you do?

KANE: I was in the kitchen, [preparing meals].

GROSS: And you did that while taking the full range of courses . . .

KANE: While taking . . . well, I not only took the full load of courses, I think fifteen credit hours, if you do it every semester will get you to graduation. <**T: 35 min**> Well, I took twenty-one as often as I could.

GROSS: By choice?

KANE: By choice, because I wanted to get everything I could out of [my college years]. Because after the first semester in the nursing program, I'd experienced chemistry, and fundamentally said, "Let's drop this nursing major as fast as I can get it dropped, and go on to something that's going to be a lot more exciting."

GROSS: So this was not your first time ever taking a chemistry course, but it was the one that really changed your mind . . .

KANE: But it was the one that changed my mind. The woman—and it's interesting because [of] the woman who taught that freshman chemistry course, which was required of every nursing major. You could not get a degree, as opposed to an RN, if you didn't take college chemistry, so most of the people in the class didn't want to be there. They had to be there [to receive a BA in nursing], but they really didn't want to be there. This poor soul, Margaret Cushman, I will never forget this woman. She had been a chemist in a hospital laboratory for twenty, thirty years, and wanted to change in the way she was using her chemistry background. How she ended up in Keuka, I do not know, and it's long since gone in terms of any opportunity to ask her. But she just made chemistry come alive and seem exciting to me. As I say, most people in the class were there because they had no choice. They didn't want to be there. I was eating up everything that she had to give us. Other people were struggling and I was flying through it and loving it.

In some ways she may not have been a good teacher, if by being a good teacher you have to be able to reach those people who don't want to be there, but are anyway. But for somebody who had the slightest interest in what she was doing, because she just exuded enthusiasm for the subject, it was heaven on earth.

GROSS: And this was your first year you were taking this?

KANE: This was my first year. I went home at Christmastime and I said, "I'm dropping the nursing major and switching to chemistry, and my mother said, "You're what?" Again, back to the comments about a C in art. Chemistry. Madame Curie. "Do you really think you can do that kind of chemistry?" Why should I have to do *that* kind of chemistry? Why is that the only basis for saying you want to be a chemist?" So I made the switch. I really don't think they were terribly happy about it. There was also no holding me back. By the end of my junior year at Keuka, I had taken every chemistry, math, and physics course that the school offered. Okay, senior year has to be spent in residence, but they had nothing left to offer me. They were not ACS [American Chemical Society] [approved]. You know, they did not have all the courses that [ACS suggests a chemistry major] ought to have. So the interesting thing there, again, how does serendipity happen? Because none of this was planned. But Anna Jane Harrison, who was chairman of the [chemistry] department at Mount Holyoke at the time, came and did . . . came as a visiting lecturer for one week during [Mount Holyoke's] spring break, which didn't coincide with [Keuka's], or maybe it wasn't their spring break, she just took a week off. I don't know how that worked exactly. But Margaret Cushman, and Colleen [M.] Gorman was the one who taught organic at Keuka, and Irene [P.] Monahan was the one who taught math. The three of them and Anna Jane, just <**T: 40 min**> kind of confabbed the whole [week] . . . what are we going to do with Jean?

GROSS: Were you the only one in this position, by the way?

KANE: Oh, yeah, because again . . . I mean, the chemistry department . . . they were known for their nursing department. They were not known for their chemistry department. [And chemistry was not a major that many women were choosing then, especially at so small a school lacking the full range of ACS-recommended chemistry courses. Mount Holyoke was quite another story.]

GROSS: Was there a chemistry major?

KANE: There was a chemistry major, but I mean, I [already had all the chemistry Keuka offered]. I had a double major with chemistry and math, and a physics minor. But again, there couldn't even have been a minor because I don't think there was but one physics course. But you know I was the only one who really followed through a lot of chemistry. As I say, I took everything they had to offer, but I was going to be spending my senior year just biding my time, taking English and history and religion to fill up the day. I had credits enough—probably had credits enough—to graduate. I'm not positive, but that senior year in residence was kind of befuddling everybody. But Anna Jane said, "Well you know, if she's interested in graduate school, I can offer her a teaching assistantship at Mount Holyoke. Let her take the things we offer, that you don't, for her senior year chemistry credits, as well as taking a graduate school class that might overcome the senior year in residency objections . . .

GROSS: Oh, wow.

KANE: . . . from the Keuka faculty. So again, like I say, I mean, there's this serendipity that is all the way through. There was this huge faculty meeting over "are we going to allow her to do this, or are we going to insist on senior year in residence?" Those three women [Margaret Cushman, Colleen Gorman, and Irene Monahan] fought like tigers for me, and got the permission. So I spent my senior year at Mount Holyoke instead of at Keuka and also got one graduate school course under my belt at the same time. In those years, Smith [College], Amherst

[College], UMass [University of Massachusetts], Mount Holyoke, and there should be a fifth school, but it escapes me . . .

GROSS: Is it Hampshire [College]?

KANE: It probably was Hampshire . . .

GROSS: You're talking about the Five Colleges.

KANE: The Five Colleges. The Five Colleges fundamentally said, "You can take classes at any of us and get credits at the other." So here I've got this gift from the gods from Mount Holyoke. But I wanted inorganic rather than organic or p-chem, and that was only available at UMass. So I'm running back and forth across the Notch Road taking classes over there. I ended up deciding that I wanted to do my thesis with Tom [Thomas O.] Zajicek, who was at UMass, not at Mount Holyoke.

GROSS: Now this is your graduate school.

KANE: This is my graduate school. I had one year . . . senior year and graduate school overlapped. Then I had one more year of straight graduate school.

GROSS: Did you have to do a separate admission application to get . . .

KANE: No. Everything . . . this was part of the arrangement. This was part of what they set up for me.

GROSS: So they basically set up a direct path into graduate school in chemistry?

KANE: In chemistry. [. . .]

I was very, very happy there [at Keuka], but I was also . . . I was at a performance level above the average for Keuka. Definitely below the average for Mount Holyoke. I mean, I sacrificed grades in spades when I made that change. But I learned twice as much from my [C's] at Mount Holyoke as I'd ever learned from my [A's] at Keuka. And to me, it was about learning, it wasn't about a grade. I mean, you want the grades, but they weren't what mattered more to me than the learning. [. . .]

GROSS: Sure. So here's another question I have about Keuka. You said you took every course they had in chemistry. That involved that intro chemistry class that you mentioned, right?

KANE: Right.

GROSS: With Margaret Cushman.

KANE: Right.

GROSS: Then there was . . .

KANE: Organic with Colleen Gorman. There was a full year of quantitative analysis, again with Margaret Cushman, and a semester of qual [qualitative], and I think that's it. I could pull a transcript out and look, but I think that's it.

GROSS: That's reasonable. That's plenty, or at least it seems like plenty, to the typical Keuka student at the time, I'm sure . . . Did you have lab work at all?

KANE: Yes. [. . .]

Well, I don't remember organic labs that well, but there must have been. But the quant lab, I remember very, very well . . . the quantitative analysis, the [gravimetric barium sulfate precipitation, both acid-base and redox titrations], and other things of that sort. But the other thing that Keuka had, it was really incredibly helpful, because there weren't that many chemistry classes, they were on a quarter system rather than the semester system. There was a five-week break between the fall and the winter quarter, in which everyone was required to do fieldwork.

GROSS: I was going to ask about this. You mentioned it in your bio.

KANE: Yeah, and the very first field assignment was a term paper—independent research. I did it in literature, I think because we had been in Paris and I was infatuated with the French.

What I remember doing is Victor Hugo's poetry, and in the original French and translating it into English and then doing a paper around his literary style and whatever.

GROSS: Did you know French before you went to Paris for this?

KANE: I had high school French for three years—four years.

GROSS: But I assume it got a little better when you were over there?

KANE: It got a little better, the French are merciless in terms of, if there's a trace of an accent, they pretend they can't understand a word you're saying.

GROSS: Oh, dear.

KANE: So there was some of that frustration. [. . .] *Geostandards Newsletter,* which comes up later, is published by the French equivalent of USGS [United States Geological Survey]. All of their abstracts are in French as well as in English, even though the papers are all in English. For a long time, I remained fairly able to read a French scientific paper, not without occasionally needing a dictionary, but still reasonably capable . . .

GROSS: A solid reading knowledge . . .

KANE: A solid <**T: 50 min**> reading knowledge, not a writing or speaking knowledge, but a solid reading knowledge of French.

GROSS: This is all a diversion from the story of your fieldwork. So this was the first year . . .

KANE: That first year was the French poetry. The second year was, it was at Genesee Hospital, [in Rochester, NY]. [. . .]

Because that's where Margaret Cushman had worked in hospital laboratories all of her career before she came to Keuka, she had enormous contacts. She set all of that up. I believe the apartment was in the divinity school dormitory. It was in some college dormitory. But she got all of that set up. She came to visit me while I was there.

GROSS: This was your sophomore year?

KANE: This was my sophomore year. Then the junior and senior year . . . well, no. It wouldn't have been . . . it would have been the summer between sophomore and junior year. Again, the summer between junior year and going to Mount Holyoke, I went to Syracuse University on the NSF [National Science Foundation] summer research grant program.

GROSS: Okay.

KANE: Each of those required simply a written paper. You didn't get your final summer research grant paycheck until the paper had been turned in. So there was some experience there [not only in the lab, but also of writing] a research paper.

GROSS: Not only that, but apparently it was through a grant program from the NSF.

KANE: Yes, it was through an NSF grant program.

GROSS: So was this your first experience with government-funded science?

KANE: Yes. Again, I mean, I think so much of it was almost done for me by the fieldwork program. I don't recall having had to do much of anything in the way of applying for it. Now, maybe just like fifty years later, I don't remember what I had to do, but . . .

GROSS: You mean applying for the original . . .

KANE: Applying for the grant. I think somehow the fieldwork program at the college put me in touch with it, and kind of laid the groundwork, and made it almost a guarantee. Certainly if faculty recommendations had been needed Margaret [Cushman], and Colleen Gorman, and Irene Monahan, all of them would have done something for me. As I say, I don't remember the business of having to apply for grants anywhere in my career, quite honestly.

GROSS: Really?

KANE: Which, yeah, is quite amazing. It's an area of getting the funding for what's being done, whether at USGS, at NIST [National Institute of Standards and Technology], at whatever else, I . . .

GROSS: You just didn't involve yourself with it [. . .]

KANE: I honestly never touched [. . .] I was never pointed in the direction; I was never told I needed to. If anyone had told me I needed to, I . . .

GROSS: You would have gone out and done it, I'm sure.

KANE: I'd have been the first one to do it. But it never was presented as something I ought to be doing, not even in terms of the graduate research with Tom Zajicek at UMass. He had the grant. It was taken . . . well, I had the teaching assistantship at Mount Holyoke, so I didn't even have any other funding. It's one of those pieces that is a total blank in my career that's kind of amazing to think about right now.

GROSS: Well, let's move to something that's hopefully a little less blank, the two different types of research you were doing during these work/study programs.

KANE: Right.

GROSS: One was at the Genesee Hospital. What were you doing?

KANE: [At Genesee Hospital, it really could not be described as research. That describes the NSF summer programs well enough, but at Genesee] I was doing blood chemistry in the hospital laboratory.

GROSS: And this was your first exposure to this type of work, I assume.

KANE: Oh, yes.

GROSS: So what was that like?

KANE: [. . .] I really loved it, because I was using what little I knew. I was working with people who were incredibly supportive of . . . this is supposed to be a learning experience. I mean, the only thing I really remember about that was having totally blown a bilirubin test <**T: 55 min**> on a highly jaundiced newborn.

GROSS: Oh, my.

KANE: And yet, when the doctor came down and talked to me, and the staff people who had been training me in the procedure talked to me. Finally, the doctor said, "We'll submit a new sample tomorrow. The baby is so jaundiced, I didn't really need the lab result to know that I had a problem." Nobody made a big deal of it, and yet, you stop and think about it, it probably could have been a life-threatening error, but it was handled with enormous grace and compassion. [. . .] I'll never forget it, but it also was a matter of nobody made a big deal of it [my gross error, which in hindsight is tremendously important to the learning process].

GROSS: This is interesting because it kind of takes you back towards the nursing side of things.

KANE: It takes me back towards the nursing side of things [but very indirectly. Mostly I was learning lab techniques]. I mean, in those days I'm pipetting, [. . .] not with syringes, but by mouth; none of those issues come up yet.

GROSS: Wait. Pipetting by mouth?

KANE: I'm serious. We're talking 1960.

GROSS: So you were basically sticking a tube in . . .

KANE: [. . .] and pipetting by mouth.

GROSS: Okay.

KANE: You know the auto pipettes, the Oxfords and the Eppendorfs and whatever, we had them by the time I was at USGS, but they certainly weren't . . . I don't recall anything but glass pipettes [earlier] and by the time I was at USGS, it was you *have* to use the [newer automatic pipettes or a suction bulb if you're using the older glass pipettes].

GROSS: Right, that's what I thought. [. . .]

So I'm going to betray my ignorance. I didn't even know about pipetting by mouth as a technique. I mean, I'm still a little befuddled by it, I confess.

KANE: Well, like I say, I mean, it's the only way I ever [began] . . .

GROSS: I'm imagining basically, sucking up liquid with a straw.

KANE: Yes. Exactly.

GROSS: [. . .] It's a glass tube.

KANE: It's a glass tube that is . . . I mean, there were [markings at] half mil, one mil, two mil, five mil, graduated, just like a graduated cylinder is. I mean, there were different size pipettes, and some of them were straight tubes like a straw that had [gradations at] one, two, three, four, five, and you could get any amount you wanted between . . . half a milliliter and ten milliliters. Others were very specifically with a bit of a bulge tube for different—very specific—one milliliter, two milliliter, five milliliter [volumes].

GROSS: Wow. I am learning a great deal about pipetting that I did not expect.

KANE: Well again, I don't know when the transition really happened, but I really remember pipetting by mouth in the qual [and quant classes], and at the hospital.

GROSS: And these were with blood samples mostly?

KANE: Well, I mean, obviously you're [pipetting] reagents as well. I think with blood samples . . . maybe not. Maybe [with] the blood samples they had something else, but certainly with any of the reagents you're [adding to the blood samples].

GROSS: Sure. For any of the tests you're doing.

KANE: For any of the tests that you were doing. As I say, you know, it's been fifty years. I could have long since forgotten some of the details of that sort. But I . . .

GROSS: So did this make you want to look again at nursing as a potential career?

KANE: No. It really didn't. [And the two summer NSF research projects further cemented my love of chemistry. The first summer I worked on a buoyancy project in Frank Kanda's lab. Then the second year I was making coordination compounds and doing, I want to say, some nucleus type of magnetic property, but I may not have that right. But in any case, it was my first contact with coordination chemistry, and I was fascinated with it.]

GROSS: [. . .] What made it interesting to you? What was appealing about it?

KANE: Some of the appeal was in the fact that that was the point in time when they were first trying to figure out the orbital connections, in terms of how the coordination compounds formed and what the bonding—ligand field theory. Who was the gentleman in [England], who did so much of that work? I want to say [. . .] Orgel—Leslie Orgel—and ligand field theory. Somehow you know, again, there were so many times where the more theoretical end of it is what captured my imagination, but the physical end of doing it had to be there [as well].

GROSS: Right. So you liked both halves.

KANE: I liked both halves. I liked both halves. When I look back at the whole of my career, it was how you put the two halves together, how you do some of both. That was what made the different choices over time fit together.

GROSS: Now were these work/study programs—were you gaining a little bit more of the theoretical background there, or were they more explicitly aimed at getting you kind of hands-on experience with new techniques?

KANE: It was more hands-on experience, but it was also the sense of . . . there might have been ten or fifteen of us on these summer research grants. There were brown bag lunches and seminars, and just that next step up in terms of getting you ready for graduate school and somewhat familiar with the kinds of experiences, and doing a lit study, and writing a paper, and things that just didn't happen in a small school with a limited curriculum.

GROSS: Now the ten or fifteen people, they obviously weren't all from Keuka.

KANE: No. I was the only one from Keuka. They were from probably all over the state. For all I know, some of the students who might have applied and gotten into the program at Syracuse University might have been from other [out-of-state] places, but I would think that it was probably within, you know. We were in the summer-school dorms where mostly graduate students, mostly teachers coming back for that extra—you need a few more credits every so often to maintain your certification—and people like that. So we were in the dorm with a whole bunch of people who weren't undergraduates. [. . .]

GROSS: Okay. That doesn't change the question however, which is, did these techniques come in useful when you went on to Mount Holyoke and started doing graduate school courses?

KANE: The ability to do a lot of lit work, the ability to write the papers, [yes]. The specific techniques, not really. Again, that second year at Syracuse, I did a lot of working inside a glove box. Again, that controlled atmosphere and the gloves that you could get, and I can get into, both. I mean, they don't fit me terribly [well], and yet they had to be a size that literally [fit] anybody.

GROSS: Yeah, one-size-fits-all-type glove.

KANE: One size-fits-all-type glove, so they had to be too big for me. I actually had more connection with that at NIST. I mean, years later, and not doing it myself, but in needing it done by people who were preparing standard reference materials. But here was a lot of . . . I don't know how much of it had direct applicability to coursework, but it certainly gave, again, a breadth of experience. Each new project was just that much more interesting than the last one, because, again, I like variety. I thrive on variety. It was enough connection between all of the pieces that it wasn't quite like . . .

GROSS: Right. There was a variety, but not randomness or chaos.

KANE: That's right. There was enormous variety, but not . . . and something to learn, but again, the theoretical part of it and the ligand field theory. I can remember in a p-chem class the year later, when they were talking about which of the s-p-d orbitals, and which one points this way and which one points that way, in p-chem, and saying, "I don't think you've got it quite the way it's in Orgel's book."[1]

GROSS: Oh, dear. This was at Holyoke?

KANE: This was at Holyoke, you know, and you don't often do that, and you probably shouldn't ever do it, but . . .

GROSS: Well, it was very fortunate that you got kind of shuffled into these early work/study programs to give you that exposure.

KANE: It was great.

GROSS: And this was all thanks to Keuka's faculty then, the various teachers?

KANE: Exactly, yeah. [But also their whole fieldwork program. To the fact that the school simply has the work/study requirement and you've got to have those credits for graduation. They're very flexible about what goes into them and how you fill them, yet they would like—beyond that first freshman year, which is a writing project that everyone does—beyond that they would like it to be related to your major].

GROSS: Wow, that's great. [. . .] Outstanding. All right, so Keuka fades off into the distance as you head off to beautiful, western Massachusetts. Tell me a little bit about what it was like arriving at Mount Holyoke. This was your first time, I assume, in New England, correct?

KANE: Well, yes. I think we may have been there for a three-day family vacation in the Berkshires sometime, sixth grade, eighth grade, somewhere in there. But other than that, I had often said, growing up . . . you know, I don't remember [even being in] New York State, except for the fact that relatives lived in Poughkeepsie, New York. We went back and forth between Tenafly and Poughkeepsie every Thanksgiving. But other than that, I don't think I was outside of New Jersey until I went to college.

GROSS: Well, except for Paris, right?

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[1] Leslie E. Orgel, *An Introduction to Transition-Metal Chemistry: The Ligand-field Theory* (London: Methuen; New York: Wiley, 1960)

KANE: Right, except for Paris. But in the States, I really . . .

GROSS: You never left New Jersey . . .

KANE: Don't think I left New Jersey until I went to Keuka, and then from Keuka to Mount Holyoke. So anyway, that, you know, <**T: 70 min**> very limited exposure to the rest of the world.

GROSS: What was life like as a—I guess you were a senior/graduate student . . .

KANE: I was a senior/graduate student. I was living in one of the two graduate houses on [the Mount Holyoke] campus. So everybody else was full graduate student. As I say, academically, it was exceedingly challenging. There was no being at the top of the class. There was so much I didn't know compared to everybody else there, but at the same time it was exhilarating. It was very, very stimulating. It was also, on occasion, frustrating because as graduate teaching assistants, we were expected to attend all the lectures for the freshmen chemistry class that we were doing lab for, and to take the exams. Again, I was trying to catch up from being miles behind on the courses I was getting credit for. I wasn't spending any time on the pieces of freshmen chem that . . .

GROSS: Right. That you had already taken . . .

KANE: That I had already either taken or that, in terms of the history of chemistry, and some of the things they were doing that were totally new to me, [but] I wasn't spending time on them. So there was again, consternation that my grades on the freshman chem exams weren't quite what people would have liked them to be.

GROSS: These weren't shared with the students, were they?

KANE: No. They were shared with me, but rather pointedly in terms of, "We really need you to be doing this part of it well, too."

GROSS: Now, this wasn't with Zajicek? This was with Anna Jane Harrison.

KANE: That was with Anna Jane. You know, as I look at all of it, I don't . . . here were perhaps . . . there were two gentlemen professors that I remember quite well. There were five or six women professors with whom I had very little [contact] . . . they were in the department, but I had already taken organic chemistry, so I didn't have Jane Maxwell. Lucy [Weston] Pickett was quant and spectroscopy, so I didn't have her. But the first thing you noticed was that all of the women who were professors were PhD's and were single. There was not a . . . yeah, even at Keuka, I mean, none of the three that I've named at Keuka were married women. They were career women, but they were not married women. The thought that you could combine those two pieces of life as a woman in those days was . . . obviously, Mary [L.] Good was of that generation and she did it very well, but for the most part, it was unheard of. I don't recall having encountered. . . .[2] Yes, lots of the women in the blood chemistry lab [. . .] were married, but in terms of [higher-level] professional women, there were not . . . I didn't encounter any who were married.

GROSS: Were you given advice by any of the faculty members explicitly, like, if you want to pursue a career, you're going to have to make sacrifices like that?

KANE: It wasn't said, but when I showed up in the lab with a diamond, it was definitely kind of an "Oh" . . . Discouragement.

GROSS: We should probably talk a little about that. Did that happen during this period, when you were in Mount Holyoke?

KANE: Yes.

GROSS: Okay. So in terms of timing, you were at Holyoke from 1962 to 1964, I think you wrote in your timeline . . .

KANE: It should be [about] right. <**T: 75 min**> [I actually arrived at Mount Holyoke in August or September of 1961. Then] Bob [Robert J. Kane] and I got married in June of 1963. All of my coursework was finished. All of my lab research was finished. But the thesis had not yet been written.

GROSS: Where did you and Bob meet?

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[2] See Mary L. Good, interview by James G. Traynham at Little Rock, Arkansas, 2 June 1998 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0171).

KANE: We met at UMass.

GROSS: Okay. So you were going across the road . . .

KANE: I was going across the road. Computers were so new in those days that computer programming was accepted as a foreign language requirement for scientists.

GROSS: Okay.

KANE: Okay. Bob's graduate teaching assistantship was in the computer lab. He was my instructor for FORTRAN.

GROSS: So why did the chemist cross the road? To take FORTRAN on the other side.

KANE: [Laughter] On the other side, because there was no computer lab or programming instruction at Mount Holyoke, but it was crucial to my thesis project with Tom Zajicek . . .

[. . .] When I was at UMass starting to take this FORTRAN course, and starting to date Bob, we spent the entire summer waterskiing on the Connecticut River. I mean, classes ended, and he had a friend who had a motorboat and skis, and whatever.

GROSS: There was no discussion of conflict of interest, I presume.

KANE: No. No discussion of conflict of interest. [. . .] Well, I mean, you weren't getting a grade. You had to learn the material in order to use it and write the program that I needed for my thesis.

GROSS: This wasn't for a grade *per se*.

KANE: It wasn't for a grade.

GROSS: It was just to help you work on the thesis.

KANE: To learn the programming and then, the fact that I could use it on the thesis was the equivalent of the foreign language exam. [. . .]

[Then] there were a lot of lunchtime [seminars at UMass]. I don't recall [any] women in the graduate [chemistry] program at UMass. There were definitely . . . I mean, there were nothing but women in the program . . . three or four of us, at Mount Holyoke at the time I was there. I don't recall . . . again, there may well . . .

GROSS: This was just in the chemistry program?

KANE: Just in the chemistry program. [. . .]

[In terms of coursework] one of my clearest memories there was of the p-chem course at the graduate level with Howard Stidham, MIT [Massachusetts Institute of Technology], very, very high reputation. An exam in which he asked us to write a proof for some p-chem whatever. I did. He called me into his office afterwards and he said to me, "I spent hours going over your proof. It's not the proof I gave people in class. It's not the proof that anybody else in the class put on paper. I can't find a thing wrong with it. You're getting full credit."

GROSS: So you'd come up with an entirely new proof.

KANE: Who knows. But again, I mean, he really went through it again, and again, and said, "Not what I gave you, but I can't find a thing wrong with it." That just brought me back to all of the geometry stuff that I did totally independently, but did well. What was most gratifying to me in that instance was that p-chem was something that in so many ways I struggled with, and I said it was something I can parrot, but I don't really grasp. Yet that one instance, clearly, I grasped rather than parroted it, and it was just fantastic.

GROSS: So if you went to make a list, maybe a hierarchy, of what kind of chemistry you felt most comfortable with all the way down to, not least comfortable, but less comfortable with, p-chem it sounds like was normally on the lower end of that list.

KANE: P-chem was normally on the lower end. It was something I was very, very interested in, but not what I would call good at. The inorganic, the ligand field theory, and some of that kind of thing, the coordination chemistry, how it works, what it does, how you can use it . . . was a totally different level of comfort. There was that . . .

GROSS: So that would have been the one you were most comfortable with?

KANE: Yeah, at that point in time.

GROSS: And where would, let's say organic or inorganic . . .

KANE: Organic is at the bottom of the list.

GROSS: Poor organic.

KANE: Poor organic. But to me . . . well, the natural products course, that Ken [Kenneth L.] Williamson taught that was part of the graduate experience at Mount Holyoke, was in many ways fascinating, but in so many other ways, it just seemed like there were so many names and structures to memorize, as opposed to understand. I've alluded before to how I want to understand, I don't want it to be memorizing a telephone book. Certainly, the people who enjoy organic chemistry [the way that I enjoy inorganic and analytical] don't look at it that way, but that's the way I saw organic, like trying to memorize a telephone book.

GROSS: Fair enough. So you left that far in the dust when you went off to pursue your master's thesis.

KANE: Yes, indeed.

GROSS: I guess now would be a good time, as good a time as any, to talk about your thesis and how you came to work with Tom Zajicek. Zajicek?

KANE: Zajicek, [. . .] In going across the road, and in talking to different people about what their projects were, his was the one that seemed to fascinate me the most and the one where there was a clear opening in terms of grabbing a hold of it. It was the one that, in some ways, was closest <**T: 85 min**> to that second summer at Syracuse in the way of . . . again, I was working with coordination compounds and had found that rather interesting. It was also the one that probably experimentally had the simplest techniques involved. Basically, I was determining the stability constants for the various complexes that I was looking at by a titration technique. [It harkens back to quantitative analysis with Margaret Cushman, a course I really delighted in.] I didn't need [elaborate] instrumentation. I didn't need to be good at mechanical things. I didn't need to be comfortable with building equipment, with any of that kind of thing. None of that was ever my long suit. So this was something very straightforward and simple [. . .] experimentally, but in terms of . . . stability constant determination was a bit of the theory and a lot of the mathematics.

GROSS: And this is where the computer programming . . .

KANE: And this is where the computer programming and also where my math major came into play. So it was [a] fit in all of those regards.

GROSS: What sort of mathematics was involved in this sort of calculation?

KANE: Simultaneous equations, which would have been virtually impossible to [solve iteratively, by] trial and error solutions without a computer. But it was back in the old IBM days, and card readers, and feed the . . . punch out the cards and feed a stack of cards into the machine. If they jammed, you go back and punch them out again. But you drop [them] in and it [your program] runs overnight. You go back the next day and get your results.

GROSS: Heaven help you, if you made a mistake on one card.

KANE: Yeah. If you ever even noticed that you made a mistake on the one [data] card. I mean, some mistakes wouldn't show up. They'd give you an incorrect answer, but you wouldn't . . .

GROSS: But it wouldn't be like there was an error message or something.

KANE: No. There wouldn't be an error message [if data were wrong. But if the error was in the program code, error messages would appear when you tried to compile the program].

GROSS: Did you get to run past some of these programs with your advisor? Did you two talk out strategy, or did he kind of let you go off on your own?

KANE: As I remember it, he let me go off on my own, although certainly there has to have been some [consultation], and I left the program behind for other people to use, and that kind of thing. By the time I finished it . . . for a while you could find dozens of stability constant research papers in the literature. By the time I finished, Tom said, "You know it's not even worth trying to write it up and publish it. People have lost interest in seeing these." I mean, it's a bit like ninety-seven different compounds that can be formed and just how stable are any of them, and unless you've got an application that needs to know, I mean, it's a bit like K_{sp}. Nobody's still measuring K_{sp}. It was that kind of a measurement, really, on these coordination compounds.

I used the program fifteen years later at USGS. I was wanting to do programming and I just wanted to refresh my memory about the programming code. I made a phone call to UMass and said, "Tom, do you still have a copy of it?" He said, "Yeah, I still have a copy of it. Do you need it?" He mailed it to me. It was a way of quickly relearning how do loops work, and what kinds of things you could do within the program.

GROSS: So you took to programming pretty quickly it sounds like, but maybe you just got a good instructor . . .

KANE: Yeah. I had a good instructor. [. . .]

GROSS: So it was appealing to you because it kind of forced a person to clarify their thought processes.

KANE: Yes. Yes. It really does.

GROSS: I have another question about your project. When you were working on this, were you part of a larger research group or were you doing this pretty much as a solo project?

KANE: I think there were others doing similar work [but we were not working collaboratively on our projects]. Like it felt . . . and just knowing myself, I am much more comfortable to go in my corner and do my thing, and not be part of a group. I mean, one of the things that I have said to numerous people over the years is that I started doing totally different kinds of things when I retired from what I'd done career-wise. Career-wise, [I] was in my lab working with things. No people skills needed. A rock doesn't talk back. The AA instrument doesn't talk back . . .

GROSS: Well, I should hope not.

KANE: [laughter] You know, but try to do some of the things I'd been doing in my retirement and the whole people skill—I'm compromising and finding the middle ground, and developing consensus where none exists. It's a whole new world.

GROSS: Did you find it to be detrimental that you had to wait until retirement, basically, to really start acquiring these skills on a daily basis?

KANE: Not really. I mean, I would have had to learn those skills much earlier, if I had ever been a manager. [. . .] So I could have the best of all worlds and fundamentally that's the way I look at my career. I had the best of all worlds. I didn't do everything at once. I see so many young people today trying to do it all at once. I wonder how they survive. [. . .]

GROSS: So could you give an example of what you've seen young people doing, or trying to do, all at once that you wonder about?

KANE: Well, [our] daughter is . . . she's got a new job now, but she had been a vice president of an environmental trust group. Before that, she was with a similar nonprofit, where, at twenty-two, twenty-three, she was supervising five, or six, or seven, or eight employees, and getting married, and starting a family, [and becoming a supervisor almost before you've done anything else—not quite, but you know . . . And not having a full-time live-in housekeeper].

GROSS: Oh, okay. [. . .] Trying to combine both the research side of a career and, say, a managerial . . . or the work and the—typical work, whatever that work might be—as well, as the managerial responsibilities . . .

KANE: Responsibilities seemed to come all at once, or very nearly all at once.

GROSS: Whereas you feel as though you paced those out.

KANE: And I paced out those out. I spread them out, and in some cases, I just totally avoided them.

GROSS: Okay, interesting.

KANE: But I shake my head in amazement at what she's doing.

GROSS: Well, do you think that this is . . . I mean, I think about this . . .

KANE: Some of it's generational.

GROSS: Well, this is what I was going to ask.

KANE: It's totally generational.

GROSS: Is the way that women now fit into the workplace compared to the way they did when you were there?

KANE: Women definitely fit into the workplace in an altogether different way. The younger men [also] have a different attitude. I mean, one of the things I heard in the short while I was at RCA [Laboratories] was that, quite frankly, women didn't belong anywhere but at home or in the kitchen. Not many people—not the people I worked most closely with, and thoroughly enjoyed, and felt there was a great deal of shared respect and whatever else, but a few people [had that viewpoint] . . .

GROSS: No. I was going to ask about this. You know, women in industry, there aren't that . . . you are, as far as I know, the only woman who worked as a core part of the original liquid crystal research team.

KANE: Right. There was a woman who I worked with very peripherally in the support analytical lab that did the analyses on all of my decay products. But whether that was totally support function and not . . . job as distinct from [professional] career. I really didn't know her well enough or pay enough attention to be able to answer that question. Certainly most of the women I worked with at USGS, where we were in . . . the Branch of Analytical Laboratories [BAL] was a support function for the research geology. Most of the women there never published, never spoke at [any meetings], never did anything that could be called research. I did as much of it as I could find a way to squeeze in.

GROSS: Fair enough. We'll talk more about this as we go through the arc of your career <**T: 100 min**>. So we're still, I think at this point, at Mount Holyoke technically, although we jumped ahead.

KANE: We jumped around.

GROSS: We talked a little bit about your research group. Other than your advisor, did you consult with other members of the faculty at all, or were you basically one-on-one?

KANE: No. With the rest of the faculty, it was more [that] the interaction was in terms of the courses they were teaching.

GROSS: And the courses that you both took as a graduate student and TA'ed for . . .

KANE: Well, at Mount Holyoke all the TA'ing was with freshmen chemistry.

GROSS: Okay. So exclusively with that.

KANE: Exclusively with freshmen chemistry, I think, both years that I was there. None of the TA work was at UMass.

GROSS: At UMass. But you did interact with faculty at . . . wait, you took the courses at UMass.

KANE: I did more coursework at UMass than I did at Mount Holyoke in terms of . . . you know, that first graduate year that was both my senior year and my graduate year, was exclusively at Mount Holyoke. But I think the following year, every course I took [. . .] was at UMass.

GROSS: That may have been slightly awkward. You mentioned there was . . .

KANE: Again, I mean, it was awkward in that regard, and yet, they accepted it, because they were promoting the Five College partnership.

GROSS: The Consortium.

KANE: Consortium [was being very, very heavily promoted], so that it was almost impossible not to accept it, but I don't think I felt the awkwardness from that at the time. In looking back, I'm saying, you know, that was almost like a finger in the eye, after all they did for me getting

[me] there. It really was pretty amazing that it was tolerated. But the only thing I felt was resistance to was getting married. At the time, the coursework and the lab work on the thesis were done, but before I'd actually [written and defended the thesis, and that was definitely an awkward situation].

GROSS: So there were maybe some rumblings from your advisor, for example?

KANE: Not from Tom [or any of the men at UMass]. It was from the women at Mount Holyoke. [. . .]

GROSS: But the women of Mount Holyoke . . .

[. . .] Was the presumption because you now had a marriage that you would not be able to continue pursuing . . .

KANE: That I would not continue pursuing. [. . .] You know, there was almost a ten-year gap. I mean, I was at RCA until 1967. But then USGS was 1976.

GROSS: Yeah. You mentioned there was a hiatus.

KANE: There was a big hiatus in there, because child care in those days wasn't real easy to come by. Again, just in general, society frowned on it, and [. . .] Bob was very, very comfortable with RCA. When I was there between the children, I was working half time. Two days one week, three days the next week, so there was more face time with [our son] than there was away from [him].

GROSS: Sure.

KANE: That was fine. No argument there at all. No difficulty there. But once the second child was born . . . and Dave [David] Kleitman again, hindsight says he and I left pretty much simultaneously. There's also no two ways about the fact, he's the one who hired me. He was kind of a champion in the—again, it's only hindsight that says, how strange it was—before this shared <**T: 105 min**> time on the job, and any number of other accommodations of family and work. But he had no problem with that. He was perfectly happy to have me come back on that basis. But it would have been very difficult after that to find acceptance of it without that one person who was . . .

GROSS: Your advocate.

KANE: . . . an advocate.

GROSS: When you got married, I assume that you moved out. At this point you were living . . . originally you said you were in the graduate dorms. You moved out of the dorms at this . . .

KANE: [Actually, I was in an off-campus apartment with two other graduate students, and not in the dorm the year that was full-time graduate work.] Well, we got married at the end of both coursework and lab research. In fact, we moved to Richmond [Virginia], which is where Bob had . . . Bob finished his things up in March, and I finished up in June. We got married at the end of June. He had already gone to Allied Chemical in the Petersburg plant in [late March or early April], 1963.

GROSS: This is 1963?

KANE: June of 1963 [is when we got married]. So were down there for a year: June of 1963, [and] we came to New Jersey of June of 1964.

GROSS: And this was after you had finished your thesis.

KANE: After I'd finished the thesis. I went back. We got married June of 1963. I did the writing in our Richmond apartment, mailed it back, had the thesis defense was the weekend of John [F.] Kennedy's assassination. I heard about the assassination in the print shop at UMass, picking up the drawings to complete my thesis.

GROSS: Oh, wow. That must have been memorable.

KANE: It was more than memorable. But basically, whether I had defended my thesis the day before and just had to get the drawings for binding, or whether I did the thesis defense right after I [heard] it, again that's totally lost in memory, but it was that weekend that I went back and defended the thesis. Again, the irritation that I described about my having done things the way I did, there were a number of times during the thesis defense when Anna Jane would ask a question, "This belongs in the thesis, and I didn't see it there." Either, Tom Zajicek or Ken

Williamson would say, “It’s right here on Page X. It’s right here on . . . she did put that in the thesis. She really did.”

GROSS: So this is your previous kind of champion/advocate now, who’s turned . . .

KANE: Yeah, she had been . . . but again, because she was my champion when I was heading toward a career that she saw developing in the same way that hers had.

GROSS: Which would end up potentially being a professor at someplace like Mount Holyoke or . . .

KANE: Mount Holyoke or whatever. Instead, I got married and didn’t have a job immediately, and this, that and the other things.

GROSS: Did you end up parting on reasonably good terms, at least?

KANE: Well, yes. I mean, I would not say there was any open hostility. There was just the undercurrent of, “I’m not quite happy with the way this has developed.” [. . .]

GROSS: So a little tension there.

KANE: A little tension.

GROSS: All right. So you defend your thesis. The defense goes well?

KANE: The defense went fine. [. . .]

The degree came in . . . I think it was February. They issued the degree in February, but the thesis defense was finished in November. [. . .] When the thesis [was defended didn’t exactly coincide with] when the degree was awarded. [. . .]

GROSS: Now at that time, your career plans were kind of up in the air, it sounds like.

KANE: Career plans very much up in the air, because it was . . . okay, Bob and I are married. I'm going to go wherever he's going. I'm going . . .

GROSS: And you went to Richmond . . .

KANE: And we went to Richmond. I will look for work, but I will fundamentally take what I can find that lets us stay together rather than [not] . . . I did run into one gentleman at USGS whose wife was at the University of Chicago, and he was in Reston, Virginia. They got together Christmas and Easter—or, you know, who knows quite what—but that was not going to be the way my marriage was going to go. [laughter]

GROSS: You were dealing with what my friends who are now on the job market, sometimes refer to as the "two-body" problem, where you have to find a way to . . .

KANE: Yeah. You have to find a way to bridge it. There was . . . for neither of us were there the kinds of mentors who would be able to help us bridge that kind of a divide. I mean, in looking at Mary Good's biography for an example, she talked about the fact that when she got a placement, there were people connected either to her or to her husband or to both, who made sure that something was found for him.[3] Well, I mean, we didn't . . . we had no hope of anything like that working for us.

GROSS: Well, especially since it sounds like Bob went into industry.

KANE: Bob went into industry. Bob is a chemical engineer rather than a chemist. [. . .]

What ended up working [for me in Richmond] was going in to complete the teaching year for a math teacher who had had an automobile accident and was not going to be able to come back. So I picked up geometry . . . probably just geometry. I don't think I taught anything else, but I may have had three or four sections of high school geometry for the balance of the year. Again, because I had done it independently myself, it was in many ways a wonderful fit, but it was also something that meant I was going back, and relearning something that I really hadn't used in eight or nine years. But I loved doing it. The first few days, not having . . . just because kids will play with people until they get . . .

GROSS: Oh. Oh, they will.

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[3] See Mary L. Good interview, Chemical Heritage Foundation, Oral History Transcript # 0171.

KANE: And, not being the kind of child who would have dreamed of doing that, the first few weeks of getting through that and getting things settled down to the point that you could actually teach, were perhaps a bit traumatic. But I loved what I was doing. I got to the point of really enjoying the kids and having a few kids who had struggled <**T: 115 min**> with the other teacher doing very, very well with me, because I was willing to take the time to work [one-on-one with them]. [. . .]

GROSS: But compared to either of the other . . . well, not either of the other, but two common concerns that people have when they're starting with teaching is classroom management, which you alluded to. Another one is just getting used to lesson planning for a daily lesson as opposed to maybe a TA section once or twice a week . . .

KANE: Right. Well, and I had a lot of fun with that, and I know that most teachers didn't. I basically said, "Where are the kids?" the day I took the class over. "Where does the book end?" Sat down and mapped out how I could get to it all by the end of the year. Most people would do one day's lesson at a time. If the kids aren't getting it, and they needed more time, instead of giving the kids extra help after school and <**T: 120 min**> during study hall, would just slow the whole process down, so they'd never get to the end. Again, my thought process was, I've got to get there. There may be some pieces between here and there that are less important, that are redundant, that can be skipped or can be assigned as homework, if you're having trouble with a concept. But within the first two or three weeks, I'd probably been through the whole book and figured out how I wanted to structure the rest of the year.

GROSS: And this is actually how they often will tell new teachers coming, "Make sure you have a clear idea of what your end objective is going to be, and then work backwards . . . " [. . .]

GROSS: Right. So after a slight learning curve getting back from chemistry-type thinking to math thinking and getting used to a new set of students with different sets of pedagogical and behavioral challenges, shall we say, you spent a rewarding year, rewarding . . .

KANE: It was really . . . it was, I think February to June.

GROSS: So it was only, basically, a semester . . .

KANE: A semester, and yes, it was very rewarding. I really, really liked it. But at that point, Bob was transferred from the Petersburg plant to the Philadelphia [Pennsylvania] plant at Allied Chemical. So we moved to Burlington County, New Jersey.

GROSS: An area with which I'm somewhat familiar. [. . .]

All right. So the year is 1964 [. . .], and you are relocating to Burlington County, right near Philadelphia. Bob has a job at Allied Chemical, [correct]?

KANE: And I'm looking for a job . . .

GROSS: And you're looking for jobs. What are the options that are out there at that point?

KANE: The options that were out there . . . again, I had a very happy and successful—although brief—experience with high school teaching. I applied in Burlington County for a chemistry teaching position. Again, not having teaching credentials, but thinking, you know, [. . .] maybe it'll work. There was an opening, and of course, this is June. School's going to start right after Labor Day. I'm offered a chemistry teaching position. I don't know what high school. It doesn't matter. But I'm also told that Glassboro [State College, now Rowan University] is the nearest state teachers college. "While you have three years to become certified, you may not [set] foot in a classroom in September, unless you have had New Jersey state history and phys ed from a New Jersey state teachers college." [. . .] Phys ed and New Jersey state history.

GROSS: Regardless of subject?

KANE: Regardless of subject [and that I'd had two years of phys ed at Keuka]. [. . .]

GROSS: So you've got phys ed. You've got a semester of teaching, and you've got more time, if needed, to get certified in . . .

KANE: In chemistry.

GROSS: In chemistry in [New Jersey].

KANE: But, so, clearly I said thank you, but no thank you. [. . .]

GROSS: You arrived in July, you said. [. . .]

KANE: Right after the school [year ended in Henrico County, Virginia].

GROSS: Okay. So you get there. Basically, you decide that it would be real hassle <**T: 125 min**> to go to Glassboro on a daily basis, so then what?

KANE: So then we started looking for what else is in the area that will let me use chemistry. That's, you know, how I ended up at RCA. [. . .]

GROSS: Before we get to RCA, and believe me, I'm very excited to talk about it. You also mentioned in one of your emails to me that you were looking at an instructorship position at Rutgers [The State University of New Jersey]. [. . .]

KANE: Chronologically that's two years down the road.

GROSS: Okay. We'll get to it. All right, let's talk about RCA. How did you first hear about a position at RCA Labs?

KANE: I must have seen a job opening in a newspaper. As John van Raalte told you, and you alluded to in your [email] . . . why they were looking for a chemist? Why [had] that looked like a reasonable place to apply?

GROSS: Had you heard of RCA before then?

KANE: I'm sure I had heard of RCA, but not of the research labs specifically. Maybe everyone had heard of RCA: RCA music, Rockefeller Center [laughter]. But in terms of a possible place to work, no. It was a matter of scanning job openings in newspapers and saying, "What's within reason? Is there any point in even looking at this?" We had only one car, and getting back and forth, if I took the job, was going to be a nightmare of a logistics problem, but I went for the interview. And again, I'm thinking chemistry, but the very first interview question was, "Ohm's Law: tell us about Ohm's Law."

GROSS: Do you remember who . . .

KANE: I draw a total blank. I said, “I mean, I know it has something to do with [. . .] current and voltage, but that was five years ago that I took freshman physics. I haven’t thought about it since.” Again, just not even the thought process of, “How do you prepare for a job interview?” that people would go through today, that it just wasn’t there in those days. [. . .]

GROSS: Okay. So the interview with Kleitman started off rocky, but then probably segued slightly into chemistry, I imagine . . .

KANE: I assume. Yet, all the work I did that . . . well, I was going to say that first year, but that’s not right. The work I did initially was on the potassium tantalum niobate . . .

GROSS: KTN?

KANE: KTN electro-optic coefficients that John was working on. Worked very, very closely with him. We shared an office . . . initially. I think there was a third person in the office, another newly hired young woman, when I came back from maternity leave, but it was John and I. Fred Spong, Istvan Gorog were in the next lab, and the four of us were just—we’d have lunch together every day for the whole time I was there. It was a very comfortable working relationship, both personal and working relationship with those guys.

GROSS: So what can you tell me about your first impressions of working in RCA and, more generally, about kind of the difference between industrial chemistry and academic chemistry? This was your first time working in industry, correct?

KANE: It was my first time working in industry. I mean, clearly it was totally different from what you’re doing in the way of teaching high school math. The fact that the lab notebook that . . . it was impressed on me right from the start that notebooks had to be kept, had to be kept up to date, and had to be witnessed—every page had to be witnessed. John and I would pass notebooks back and forth and sign off on each other’s. We were, both of us, very, very faithful to that requirement, as I remember. The fact that, clearly, some day what you did might be a part of a patent, might be a part of a product, took that very seriously, didn’t really know as I was doing things step-by-step where, or how, or when that might ever come to be. I truly did not see the big picture at all. But again, just that’s what you did, and you did not slack off on that.

GROSS: Did you get it witnessed every day or did you normally wait a few days, and then . . .

KANE: We probably would pass them back and forth like once a week. But dating every page, and the witness thing might be a few days off from when the notebook was written. I mean, we'd certainly . . . you know, you're busy enough doing the experimental work and getting it written up that you kind of look for an hour or two of dead time to read somebody else's notebook.

GROSS: And you actually would read this over?

KANE: But we would actually . . . yeah, I really did read it, because, again, go back to: I like to learn. I like to understand, even if very narrowly, exactly what I'm doing, rather than the bigger, broader application of it for future work. That's just me. I like to be knowledgeable. I like to know and to understand, and to be constantly learning, so reading the other notebook was a way to go beyond my small piece and do that kind of <**T: 135 min**> thing.

GROSS: Now I know some people just treated it as a matter of almost busy work or routine, where you would go and just quickly jot your initials down and then move on.

KANE: No. I didn't. I definitely read John's. Again, because I was working for him, it was kind of one way that he could keep better track of what I'd actually done, and what conclusions I'd come to. So his reading it was as much for him, as for anything else.

GROSS: Interesting.

KANE: I think we both—you know, and, again, it was forty years ago. Do I remember correctly? But I think we were both very, very rigorously faithful about that.

GROSS: That's very interesting. Now, another question I had, you mentioned that you were working with John van Raalte as kind of your immediate supervisor. What was the overall structure then of the group? Was he working for Kleitman, was that . . . ?

KANE: He was working for Kleitman. There were . . . you mentioned Warren Moles in your thesis, and there were clearly several people who were working on the "how do we address the crystals?" [question]. Where I was making measurements on how the crystals changed.

GROSS: Although this came later, correct? This was . . . at the beginning of your time, you were doing KTN stuff.

KANE: Well, the KTN is a solid-state crystal. It's an electro-optic crystal, but it's a solid-state one, so that, yes, that was first and then we moved to the other [the liquid crystal]. So the people who were looking at how do we turn it into a television, or some other kind of a display, were using the same materials that I was working with, but they were looking at them from a totally different perspective. I was looking at a much more narrow—apply a current, and what happens to the crystal in this instant in time that . . . you know. [. . .]

Applying different voltages, how the electro-optic properties changed.

GROSS: This seems like a bit of a shift from your theoretical . . .

KANE: Again, it's a huge shift. [. . .] But I was constantly doing something new, something different, something I hadn't specifically prepared for, but I was willing to do the learning that let me do that with a degree of credibility. I don't think most people are comfortable moving from one field to another to something totally new and different and strange, and to finding the way to make it work, and to find satisfaction in it. If I hadn't been able to do that, I couldn't have worked as much of my marriage as I did, because again, Bob would be moving. We would be transferred here. We would be moving there. And I had to be willing to do whatever would fit that individual moment in time.

GROSS: So it was a matter of maintaining a level of intellectual flexibility in order to accommodate the demands of the relationship.

KANE: Yes. Yes.

GROSS: Interesting.

KANE: And that's something that even in [retirement] . . . I mean, most of my joy in retirement is that I can apply that now to history, to philosophy, to theology, to places that there was never any time to go when I was working. That's what libraries are for. [laughter]

GROSS: Fair enough. So I've been asking everyone who worked with Dave Kleitman to tell me a little about him, because he comes across as a kind of larger-than-life personality. Can you describe your impressions of Dave Kleitman <**T: 140 min**>?

KANE: Enthusiastic, flexible . . . I mean, he was willing to give me a job in the sense that, you're really looking back on, how did I fit? I didn't, but he was willing to make it work. He was willing . . . yeah, he was kind of larger than life, and I can't imagine that group without him, quite honestly. Obviously, he left fairly—pretty much the time I left for my second maternity leave. But it's long enough ago that, and he . . . I would have to say Dave kind of left us alone. I mean, once he'd given us the broader lines of what we were going to work on, he let us work. He wasn't over our shoulder. He wasn't micromanaging. Yes, I had interactions with him, but not that many, not that extensively, the way with some supervisors who, since then, never let you alone.

With David I had the sense that he trusted us to do what we were supposed to be doing. He certainly got reports on what we were doing. I mean, I wrote the extensive company private report on the work I'd done, but I don't remember sitting down with him and going through much in anything like the way that John and I did. There should have been the company private report on the life testing of the liquid crystal. Clearly, I mean, there have to have been reviews of things like that, of the sort that I went through later at USGS on any paper that was going to be published, but I don't remember them. [. . .]

GROSS: No, that's fine. [. . .]

It helps add a little bit more to the picture that I'm developing because, unfortunately, he's a figure who didn't leave behind lab notebooks, as far as I can tell.

KANE: Well, you know, I don't know to what degree he was even involved in the lab rather than, in a strictly management position.

GROSS: Well, whatever position he was in, his paper trail right now is [thin]. [. . .] So every bit that I can get, including that recollection, is helpful. Thank you. So some other questions. You mentioned that there was one other woman who worked in the group during the entirety of your time there. By which I mean, . . .

KANE: As I said, I don't know whether she was actually in the group or whether there was a support lab that did chemical analyses. She was in that group, and she was the one who did all the chemical analysis of my decay products, but other than giving her a sample and getting a report back from her, I don't recall a lot of interaction.

GROSS: So this is the question. That was all leading to the actual question, which is how much interaction did you have in general with other female staff members at RCA labs? Were there enough that . . .

KANE: There weren't any [that I knew of]. I mean, other than . . .

GROSS: *This* is my question.

KANE: Yeah. Other than the fact that, as I say, I remember another woman, newly graduated, coming in during my second stint there. I don't even remember specifically where she was assigned. She may well have been working with John, but I don't remember specifically. But no, it was a male province. Sure there have to have been women there. Clearly there were secretaries. Clearly there were women in personnel, that kind of thing. But no, I don't . . . all of my interactions, I remember Joel Goldmacher, Joe Castellano maybe . . .

GROSS: Okay. I've got to check these off, because they're all here. [laughter]

KANE: Okay. <**T: 145**> Definitely Joel because he was there while I was doing the synthetic work. I think Joe came in while I was on maternity leave and was there when I was doing the life testing work rather than the synthetic work. There would have been no reason, once Joe was there, for me to be doing . . . inorganic chemist to be doing organic synthesis work. I mean, he could have brought so much more to how to make changes that might be beneficial than I could ever have done. I remember a George, whether it was George Heilmeier or not, I honestly [can't recall] . . . I remember a George. I remember Sol.

GROSS: Sol Harrison?

KANE: Harrison, not specifically, that I worked with him, but, you know, I was in and out of his office. He was around with the liquid crystal group. [. . .]

GROSS: Because as far as I know—and you were there, you might have more of a first-hand perspective on this—[Istvan Gorog and Fred Spong] were not heavily involved in liquid crystal work.

KANE: No. No, they definitely . . . you know, again, so much of it was my naiveté, frankly, on the first job, in terms of trying to understand where everyone fit into the structuring . . .

GROSS: Well, this is one thing I was really interested about, was . . . you were hired as a member of the technical staff, correct?

KANE: Right.

GROSS: One thing that I found strange, and maybe you have an opinion on this, is the difficulty I've had trying to figure out kind of the organizational structure in the labs, the hierarchies and such, that were in place. Did you have the sense of the org chart kind . . .

KANE: No. Absolutely nothing. Again, at this point in my career, it would be the first thing I'd ask to see, but at that point, I was much too new, much too green, and just doing my piece was more than enough. [. . .]

GROSS: All right, another question. Did they give you any sort of . . . I've heard about this thing called a "research training program." Were you part of that in any way?

KANE: No.

GROSS: Okay. You came in, and you didn't have any choice over what sort of assignment you were going to. [. . .]

KANE: No. I mean, I went where they told me to go and didn't ask questions.

GROSS: Fair enough. <**T: 150 min**> All right. So we talked about most of those. You know, in hindsight, I forgot to ask a silly question. When you were at graduate school, were you earning a stipend? Or were you paying for . . .

KANE: There was a small TA stipend. I don't recall whether I had it that first year, when I was an undergraduate as well as a graduate student, or whether it was only the second year, when I was a full-time graduate student. [. . .]

GROSS: See, this is what I was curious about is where the salaries fit and also this is your first kind of full-time, long-term job with an actual salary, correct?

KANE: Full-time job, right. And I'm pretty sure it was seven . . . it was either, seventy-two hundred [dollars] or it was slightly below that, but I think mine was seventy-two hundred and Bob's starting salary had been eighty-four hundred [dollars].

[. . .] So there's a minor discrepancy between men and women. I don't know.

GROSS: Well, I was going to ask— [. . .] Both working in an industrial lab.

KANE: Both working in industrial situations. Again, if there was a differential, because it was so much more than I had made teaching, it didn't particularly register. [. . .]

GROSS: That's very interesting. I was also, kind of, wondering if the salary you just quoted was fixed, so all incoming MTS's [members of the technical staff] had that. Or if it was . . . there might have been a gender imbalance with RCA. But I don't know how you would . . .

[. . .] But I'm just curious in hindsight . . .

KANE: I have no idea. No idea.

GROSS: Okay. [. . .] We talked a little bit about your work on KTN, the techniques involved; you said that it was basically just applying current or voltage to a . . .

[. . .] Because if I remember—and feel free of course to correct me—when I talked with John about this, he said that the group that Kleitman had formed was explicitly display oriented.

KANE: Display oriented, but exactly what kind or whether they thought that the crystals could be thin enough, that . . . you know, I just don't know.

GROSS: So these were ferroelectric crystals . . .

KANE: Yeah.

GROSS: Okay. <**T: 155 min**> So now we get to the heart of what I am secretly most interested in, but now I told you, so it's no longer a secret. Were you aware of liquid crystals at all before you arrived at RCA?

KANE: No. No . . . had never heard of them.

GROSS: So they were not part of a standard, let's say graduate curriculum in chemistry . . .

KANE: Oh, no. No. They were much too new for that in terms of . . . I think someone looking back at it . . . wasn't it in Germany that some of the original work, and . . . .really had a hard time finding literature about them in the way of knowing what you were working with. There were a few things out there, but I don't remember. [. . .]

GROSS: When you did see them, do you remember dealing with the . . . whether you were dealing with what was later called the "guest/host" effect, or dynamic scattering? [. . .]

KANE: They go from clear to opaque. Probably that is what I saw.

GROSS: So you probably saw the latter, the dynamic scattering. [. . .]

KANE: There was certainly a time when I was reading everything I could find in *C&EN* [*Chemical & Engineering News*] and other places about liquid crystal development and how people were trying to use it, and that kind of thing, but it's been much, much too long to remember much of it.

GROSS: That's understandable. I assume you were getting all of these periodicals through the library at the lab.

KANE: No. I joined ACS as a Mount Holyoke student.

GROSS: Oh, I even . . . I forgot to talk about professional societies. So you remember . . .

KANE: I joined ACS then. I maintained the ACS membership well into my time at USGS. I may have dropped out a few of the years that I was just playing at home, and doing substitute teaching, and things like that, because it got hard to justify the dues payment, and going back to it when I joined the labs at USGS. But I was a member of ACS for a quite a long time.

GROSS: Okay. So you were getting . . . is *C&E News* <**T: 160 min**> through them?

KANE: Yes. *C&EN* was . . . basically every member gets *C&EN*. It was a weekly news magazine for the membership. [. . .]

GROSS: Okay. So you're introduced to liquid crystals. You don't remember who introduced them, but you encountered them and the mission comes down that this is the new project. How did the group change? How did your responsibilities change as a result?

KANE: Well, clearly the group, as I was working with it initially, did not involve George or Sol, or Joe. Also I was not doing organic synthesis until the liquid crystal project was something I was assigned to.

GROSS: So at the outset, what were you doing?

KANE: Well, at the onset of . . . like I say, swimming in benzene. [laughter]

GROSS: Could you elaborate?

KANE: Well, benzene was . . . again, it's much too long ago to have a whole lot of memory of the specifics . . . but, asked to do organic synthesis. Benzene was the normal solvent that everybody used in those days. It simply was the—the whole business of it being carcinogenic, and getting away from using it, and being a hazardous chemical came up long afterwards. Condensers and reflux things, and whatever, but not a lot of detail there. I would have to go back and pull some of those company private reports and reread them, with my notebooks or whatever.

GROSS: So let me try rephrasing the question a little bit then. Early on you said that you were not officially assigned to George Heilmeier's group. You were still, at least organizationally, working for John van Raalte.

KANE: Right. [. . .] Initially, I was strictly doing the measurements on the KTN. [. . .]

Just exactly [when] I moved over to doing organic synthesis more than anything else, I don't know where in that ten-month period of time [before my maternity leave; it could even have been after I returned in 1966]. [. . .]

GROSS: And then you switched over to work for George.

KANE: To do the organic synthesis work.

GROSS: And were you working with Joel Goldmacher at the time on this?

KANE: Yes.

GROSS: Because he was there . . .

KANE: It would have been Joel before Joe came in, and . . .

GROSS: Because at that point, he was kind of their *de facto* organic chemist . . .

KANE: Right.

GROSS: On the team.

KANE: So yeah, it was . . .

GROSS: This is my follow-up question, which is, you mentioned earlier that this kind of organic synthesis work was not really your cup of tea. This wasn't really something with which you had a great deal of experience . . .

KANE: I had no experience, and I basically didn't like organic chemistry.

GROSS: As you mentioned.

KANE: But when you're asked to do a job and you want to keep working you do the job you're asked to do.

GROSS: Agreed.

KANE: [. . .] I can enjoy doing anything once I get into it, if I make up mind that I'm not going to be miserable. That's all it takes.

GROSS: So here's my question. Did Joel Goldmacher provide you with help or training as you went in to learn about how to do this, or were you trying to do it on your own?

KANE: There must have been some, because in terms of all of the equipment, in terms of condensers and reflux <**T: 165 min**> columns and this, that, and the other thing. Even though, I'd obviously seen all of that four years prior in organic lab, there has to have been some support and some help, and some working with me and getting me going.

GROSS: Well, this was one question, and then the follow-up to that follow-up question was whether or not you had an assistant or any sort of [. . .]

KANE: No. I was the technician. Yeah. Again, I was hired as a chemist. But in terms of that particular assignment . . . the real sense was I'm providing technician support to other people rather than, really anything else. Yet, that's not a problem. I was both new enough and inexperienced enough that that's the only definition that really made sense. [. . .]

GROSS: Sure. Now at this point, you were doing purely synthesis, or were you also doing any analysis of these compounds?

KANE: I don't think I was doing any . . . you know, again, there was a support lab that would have been doing all of the analytical work. [. . .]

Well, and I'm sure as well, once we synthesized it, melting point for example, in the way of just the very, very, simple basic characterization of a compound, would have been doing that kind of thing. [. . .]

GROSS: Right, both the solid to liquid and pneumatic or what have you, to isotropic. That makes sense. Okay. That's the day-to-day stuff. Oh, did the group have regular . . . did either of the groups with which you were affiliated have regular kind of group-wide meetings? You mentioned that . . .

KANE: [. . .] I don't remember anything in the way of specifically group, research group, meetings. [. . .]

GROSS: And not only that, but based on what you told me, there wasn't even that much interaction between, let's say, the chemistry side and the . . .

KANE: And the application . . .

GROSS: Application side. At least from your perspective.

KANE: Not that I saw. [Though clearly there had to be some interaction.]

GROSS: Okay, that's interesting. But there were these reports, both . . . and I assume that if you were writing them, other people were writing them too.

KANE: That other people certainly were as well. [. . .]

GROSS: Okay. So what would you say was the biggest challenge with coming back to work?

KANE: I don't know. I mean, I found it fairly comfortable. Again, because of all the guys that I immediately associated with were very supportive. John and Istvan, and Fred and Mike by then, Mike Kaplan . . . that part of it was very, very comfortable and didn't cause any real issues. The biggest thing was that we really wanted our son to know me better than he knew the sitter, so the way the timing worked out, and exploiting it half time meant that was eminently possible. [. . .]

GROSS: Right. So this was one question I had, is whether or not RCA, as a company, had any policies on this, but it sounds like pretty much no.

KANE: Not that I know of.

GROSS: Oh, no. That's very interesting . . .

[. . .] And that even after . . . so even after you came back, and were there for a little bit beyond that, no noticeable . . .

KANE: No. No.

GROSS: Okay. The research changed a little bit you said. You were no longer doing synthesis because that had been passed over to Joe Castellano . . .

KANE: Passed over and . . . again, the life testing, and I'm getting that as much from the reports and from your thesis as from anything I specifically remember. I don't remember at the time having any problem with it. [. . .]

GROSS: So this was the question, was in terms of time frame, would you notice these changes over the course of an hour, a day, or more?

KANE: Or less.

GROSS: Or less. So like even the course of minutes?

KANE: And I'm thinking in the course of minutes to hours. Because otherwise, on the schedule I was working, there would be no way to make it work. [. . .]

If the material was stable over two to three days, well, I could set it up Tuesday morning and see Thursday night it was still fine. But there's somehow a gut instinct that the changes were much more rapid than that, and that it wasn't an issue. [. . .]

GROSS: I was just curious. Sometimes people didn't write everything down in the lab book.

KANE: Well, that's true.

GROSS: So this was a different type of research than the organic synthesis side of things. Did you find it more or less challenging? Did you find it more or less engaging?

KANE: Probably not either more or less [. . .]

GROSS: You tried your best to have a fun time with these life tests then. But around 1967 it looks like, on this professional history you sent me, you decided to leave RCA.

KANE: Well, I left RCA because our daughter was born. Theoretically they had a policy that you could not stay beyond six months. I could tell them, "I have no idea when this baby's due. This baby's due sometime, but I don't know when." I left about the middle of April, and she was born 28 May. [. . .]

I waited a bit longer than I was supposed to wait. Clearly, I was enjoying the job. I expect that [. . .] in the rumblings around the place there was the sense that David Kleitman was going to be gone by the time I would be able to come back again, and that it probably wouldn't work. I've got a fair hunch that that has to have been somewhere in the underground vibrations. [. . .]

GROSS: Okay. You had Deborah Jean. Maternity leave a second time probably wouldn't have been an option then . . .

KANE: Well, whether I called it maternity leave or whether we all conceded that I would not be coming back again, I don't remember. [. . .] I do remember looking for an instructorship at Rutgers, which was just down the road in Camden [New Jersey], essentially being offered a position, except that they made the offer three days before school was going to start and that didn't give me enough of a lead time to make child-care arrangements. They basically said, "You know, we've had somebody who we thought was coming back, who is not coming back at the very last minute. But we've got to fill this position in the next forty-eight hours." I couldn't do it. You know, I couldn't give them an assurance that I'd get it worked out . . .

GROSS: Right, in forty-eight hours.

KANE: In forty-eight hours. So okay, that's that. [. . .]

[It kind of turned into a permanent departure from RCA] and from professional work at all for a period of years. You know Deb <**T: 195 min**> was born in 1967. Rob [Robert Jr.] started school in 1970. [. . .]

There was, you know, there was that five years when I really wasn't working at all, in any context. Then when Rob got started with school, [. . .] time was really hanging heavy on my hands. I started doing a lot of parent advisory council type of work with the public school system. They were in the process of trying to start an alternative high school for kids who were having real difficulty in the regular school format.

GROSS: And this was in Cinnaminson [New Jersey]?

KANE: This was in Cinnaminson. So I worked on that until we left Cinnaminson, which was about two years later. That was—it was 1970, 1971 that I was very heavily involved in that kind of school involvement on a strictly volunteer basis, just trying to help set up an alternative high school and learning as much as I could about . . . Again, just get all the books on education and start reading how kids learn, and how kids think, and what . . .

GROSS: Was this like curriculum-design type work? Or was this . . .

KANE: It was more . . . what are the different approaches that schools . . . it was schools without walls, and it was hands-on learning as opposed to book learning. And how do you work with the kids who really don't fit in a classroom and are totally disruptive, and what kinds of opportunities do you create for them? I think they did eventually get an alternative high school program started. But Bob was laid off from [. . .] Sun [Oil Co.; he was at their office on Walnut Street]. [. . .]

GROSS: What year was that?

KANE: 1972 . . . maybe 1972 or 1973. We spent a fair amount of time without anything, well about six months, I guess [. . .] we were still living in Cinnaminson. He was hired by Commonwealth Oil which was in New York. So the commute was by bus from the Burlington Trailways Station to New York City. [. . .]

He was leaving at five o'clock in the morning [. . .] and would get home at eight o'clock at night. CORCO [Commonwealth Oil Refining Company, Inc.] finally said to him, "This commute is absurd. You have absolutely got to move." [. . .]

So we did and that would have been sometime in 1973.

GROSS: So where did you move to?

KANE: So we moved to Freehold, New Jersey, which was fundamentally halfway in between, in terms of miles, but we cut twenty to thirty minutes only off the commute of traffic and bus schedules. [. . .] So it really didn't buy them what they were looking for and put a different kind of stress into our life in the way of having to move . . .

[END OF AUDIO, FILE 1.1]

KANE: Having to totally start over in terms of my even thinking about working. We were there eighteen months. August of 1974, we moved to Vienna [Virginia], because that was also the oil embargo time, and Bob, with refinery experience, was very much in demand at the new Department of Energy.

[. . .] We moved from Freehold to Vienna in August of 1974. I guess it was 1976 before I pretty much decided there was no way in the world I was going to get into Rutgers or anything like that. We saw the possibility of clearly, in Vienna, [. . .] of EPA [Environmental Protection Agency]. You've got the possibility of geological survey and all of that. Went to USGS to the Branch of Analytical Chemistry . . . Branch of Analytical Laboratories, BAL. [. . .]

GROSS: Before we get too much into this . . . One question, that is according to the career history you sent me, you were also doing substitute teaching.

KANE: Yes, in Freehold I did some substitute teaching. Again, because with both children in school, I didn't need to be in the house during school hours. But I also didn't want children as young as they were coming home to an empty house. So if I substitute taught . . .

[. . .] You know, I walked to school with them. I was in the same school building, literally. It was a five-minute walk from the house, and we walked to school together. We walked home together, and everything worked out like a charm.

GROSS: Did you keep doing that when you were in Virginia?

KANE: I did some of that in Fairfax [Virginia] as well, when we first got there. We moved to Fairfax in August of 1974, and I think it was September of 1976 that I started at USGS. So in between there was a Parent Advisory Council in Fairfax, as well as what I had done in that regard in Cinnaminson. There was nothing like that in Freehold. But I did that in Fairfax as well as PTA and substitute teaching.

GROSS: Okay. So before—this actually seems like a really good place to take a short break. [. . .] Then when we come back, we can get really into your transition into the USGS.

KANE: USGS. Yes. That's fine. [. . .]

GROSS: Is there anything else you want to add before I stop the recorder, anything about your time either at Keuka, Mount Holyoke, RCA, any questions that you think . . . you know, popped into your mind when you thinking about this?

KANE: [. . .] The one thing I would say, Margaret Cushman who had been such a champion in making the move to Mount Holyoke possible, again because we had that winter five-week field period, came and spent that five weeks, my first year at Mount Holyoke at the college, showing an interest in what I was doing, going to classes there, and learning what she could from that experience. That was just a very nice [. . .] personal thing that she did.

GROSS: Well, it sounds like you had some really strong mentors, at Keuka in particular, and, I guess, into Mount Holyoke . . .

KANE: I did. I mean, it's . . . in terms of mentoring, because everything I did was so scattered [. . .], you don't have the continuity that someone would have, say, with a PhD thesis advisor in the way of ongoing mentoring. But there certainly was that <**T: 05 min**> real core champion in what I did [at] Mount Holyoke. [. . .]

GROSS: All right. Well, on that note, we're going to take a short break and then we'll get back to talking about the USGS. I'm going to stop the recorder now.

[END OF AUDIO, FILE 1.2]

[END OF INTERVIEW]

INTERVIEWEE: **Jean Kane**

INTERVIEWER: **Benjamin Gross**

LOCATION: **Culpeper Public Library**
Culpeper, Virginia

DATE: **28 February 2012**

GROSS: All right. It is 28 February. It is a Tuesday morning, and I am here in the Culpeper County Public Library once again with Jean Kane. It is a lovely day, and we're going to do the second half of this oral history interview. [. . .]

I guess we should get right to the USGS. Now take me back, take us back, if you will, to where we were when we left off yesterday. You had just left RCA.

KANE: I had left RCA. I had basically been gone from RCA at that point about six years, and in that hiatus, there was community volunteer work in two school systems, but there was nothing truly professional. A little bit of substitute teaching in the second school system in Freehold [New Jersey]. But then when we moved to Vienna, the children were enough older that substitute teaching, again, was a perfect fit for doing something that would engage me during the day when they were not needing me at home, but that also provided some degree of stimulation for me of the sort that I was really missing. I not only did substitute teaching, and primarily at the elementary school level, but I also was on the superintendent's parent advisory council, which met once a month and looked at all kinds of programmatic things that might be changed within the school system. Both of those avenues were very valuable at the time, but it was also getting **<T**: **5 min>** to be time to get back to chemistry if I was ever going to.

And in the [Washington] DC area, there's really very little industry. It's government or it's academia. And at that point, I was reasonably sure that the instructorships that might have been possible eight or ten years earlier, without a PhD and with the hiatus, were not going to be even thinkable.

So we saw an advertisement, probably in the *Washington Post*, for an opening at USGS, and I applied, and I was hired then. They had a rather strange program called When Actually Employed, where you did not have benefits of any kind. You did not have vacation days. You did not have paid holidays. You got paid the days you were in the lab, period. And it was done at the time, I think, primarily because Congress had put ceilings on the number of full-time positions within the government, and then allocated those out across the different agencies, and the people who were hired WAE did not count towards those ceilings. So it was a way of circumventing and building up your staff. But at the time that I was hired, it was represented to me as, "You haven't worked in six or eight years, and we really don't know how good you're going to be. I mean, your references are all personal references as opposed to career-based references, and so this gives us a period of time in which to see whether you're really going to work out."

GROSS: So it had kind of a two-fold effect. On the one hand, it allowed them to get around government quotas and then expand their staff, and on the other hand, it gave them sort of a provisional status that they could apply to people whom they weren't sure . . .

KANE: Right, weren't sure of. And so, you know, again, perhaps somewhat naïve, but I took it all at everybody's word, and I was quite content. Again, was working part time, because the children were, I think, second grade and fourth grade when I started, so that working five days a week but a six-hour day, they could leave for school as I was leaving for work, they could get home no more [than ten or fifteen minutes before I did]. The after-school child-care programs that exist today were unheard of then, and it solved basically all of the issues of how this was going to work out for the family as well as for me.

GROSS: Now at this point, your husband was working for which firm?

KANE: He was working for the Department of Energy. We had come to Washington. [. . .] He had left Commonwealth Oil when someone then at DOE in the new . . . oh, allocation program that had sprung up as a result of the oil embargo and all of the gas lines and that kind of thing, someone who had known him from his time at Sun Oil and [. . .] knew that he understood the petroleum industry, which they were going to be regulating. And so he was asked if he would be interested and we definitely were. We came to Washington then.

GROSS: Okay. So the next question I have is he had previous experience working kind of in the oil industry and doing chemical engineering work related to energy and petroleum products and so forth. You applied for this job at USGS with how much experience or knowledge about geochemistry?

KANE: Absolutely none.

GROSS: But that did not stop you.

KANE: And it did not stop me. Again, I wanted to find something that fit where we were living, that fit the children's schedule as well as my interest. And flexibility is the name of the game in those situations. And again, as I think I said yesterday, to **<T**: **10 min>** my mind, the most important thing about education is that you learn how to learn, and I loved the challenge of going into something totally new and different, and learning. So I was borrowing geochemistry textbooks from every geologist whose samples I analyzed. I was using the library extensively. I was reading voraciously, not only geochemistry, but also when I was hired at USGS, it was in the Branch of Analytical Laboratories, so we were a service function, but we were also strictly chemical analysts. And the chemical analysis I did at USGS was a world apart from what quantitative analysis in college had prepared me for.

By the 1960s, instrumental analysis rather than gravimetric had pretty much taken over the world. There was still some classical gravimetric analysis being done, but not much. And once you got involved with EPA, you were not really concerned about what the major constituents were. You were concerned with the trace elements that would become pollutants in the environment. I was doing atomic absorption spectrometry initially. As time went on . . . atomic absorption is fundamentally a single-element technique, and of course, as everyone is looking for improved efficiency and greater throughput, what they want is a multielement technique rather than a single-element technique.

GROSS: Could you clarify the distinction between the two?

KANE: The distinction between the two is that with a multielement technique, a single measurement would give me data that can be applied to a dozen or two dozen or three dozen constituents in that sample.

GROSS: Okay. So you might have a sample that has, I don't know, a dozen elements in it, and a multielement approach would allow you to get maybe half a dozen or all of them?

KANE: All of them at once. And again, I say all of them, but in some cases, a suite of them as opposed to all of them. Because for most of the instrumental techniques that I used, the inductively coupled plasma optical emission [ICP-OES] after the atomic absorption, and certainly for the newer [ICP-MS, inductively coupled plasma mass spectrometry]—that comes even much later and that I never specifically used—there are spectral interferences when you're trying to get several elements [simultaneously in a sample without first performing any chemical separations]. There are not only spectral interferences, but physical matrix effects that might suppress the absorption or the emission of the line you're trying to look at, so that at first a great deal of chemical separation [could be involved in isolating the one element, or the two or three elements, that you're particularly interested in].

And again, as the methodology changed so dramatically over the time I was involved, the separations went from being single-element separations to being multielement separations. So there was this tremendous transition. If you go back to the 1930s and 1940s—obviously well before I was involved—geochemistry was primarily a matter of x-ray diffraction to identify specific minerals in a rock, having basically crushed the rock **<T: 15 min>** and then doing heavy liquid separations where you're isolating them [the individual minerals] by their density. You use different solvents. You float some part of the rock away and collect another fraction that's heavier. And then you . . .

GROSS: This is the gravimetric analysis?

KANE: This is actually the mineralogy analysis, which is . . . the gravimetric would have looked at silica, aluminum, iron, the major oxides that make up the rock, and would have been a quantitative measurement of how much of each of those elements was in the rock. The mineralogical says, "Is the iron hematite or is the iron tied up in pyrite?" And there were hundreds of minerals that can make up a rock, so that, I mean, I've only mentioned a few. [. . .] I mean, there's feldspars and there's calcite and there's any number of other things . . . aluminosilicates and micas and what have you. But the mineralogical [analysis] is a totally separate type of analysis from the gravimetric [chemical analysis that determines] how much silica, how much alumina, how much iron, how much calcium, how much magnesium. Those were things like a precipitation or a colorimetric dev . . .

GROSS: Ah, so the separation is with the mineralogical technique you were describing, which is with different liquids and densities?

KANE: And densities, and you're collecting different weight fractions, because each mineral has its own specific density.

GROSS: Right. And then once you've done that, you can start doing these other techniques?

KANE: Well, and in some cases, you don't do both. In some cases, you would only do the . . . [BAL] would only do the chemistry. It was a different group within the geologic division that would have done the point counting and the minerals—heavy mineral separations and—well, you could also do what was called a thin section, where you sliced the rock into millimeter-, centimeter-thick wafers, and polish, and then look at it under a microscope, and in polarized light. And all of those techniques were a group outside of the Branch of Analytical Laboratories. But BAL would have begun with the classic gravimetric analyses, and then they moved to different instrumental techniques. For the major oxides, it was x-ray fluorescence. That was the primary technique. [. . .]

Depending on the type of stimulation, you can either have atomic emission, you can have x-ray emission from a sample, you can have . . . different pieces of the energy spectrum are examined with different spectrometric techniques, and x-ray is one of the ones that I really never used. But the different techniques were ideally suited to different suites of elements. [. . .]

Optical emission probably covered the largest suite of elements, and it was one of the first to be developed, but it was stimulating the sample with the DC arc, and [. . .] you're exciting the atom, and as the atom returns to ground state, it emits the specific wavelengths that had been stimulated. And that was a multielement technique, and it was one of the very, very first that could be applied to trace elements rather than major oxides, although it could **<T: 20 min>** also be applied to the major oxides. But because of the calibration difficulties for that technique, and because of the difficulty of getting . . . well, depending on the matrix of the sample, the excitation from a unified arc might give you 80 percent excitation in one matrix of the element you're interested in, but in another matrix, only 70 percent. So you did not have a truly quantitative technique. You had something infinitely better than had ever existed before, but for a variety of reasons, the method in general was reported as a plus or minus 20 percent method.

GROSS: That's a pretty large margin.

KANE: That's a pretty large margin, but when . . . a gravimetric technique or any of the classical colorimetric, titrimetric, really could not be applied below a tenth of a percent.

GROSS: So for these sorts of trace elements . . . ?

KANE: For any trace elements, they were utterly unusable, and a 20 percent number [. . .] compared to nothing at all is a dramatic improvement, and it led to all kinds of advances in geochemistry. [. . .]

I started doing atomic absorption [spectrometric, AAS] analyses and doing all of the different sample preparations that would get the sample ready, because what you do with both atomic absorption and atomic emission, not DC arc atomic emission, but ICP atomic emission, and you [. . .] are aspirating a solution up into a flame. [. . .] The flame causes the excitation of the atoms.

GROSS: And then when the ground state . . . when it returns to the ground state, that's when it [. . .] emits the spectrum or in the **<T: 25 min>** case of absorption spectrum . . .

KANE: In the case of absorption spectrum, you are measuring absorption by shining a single wavelength light through the flame. And you are measuring the degree to which the sample absorbs part of that light, whereas with emission, you're exciting the sample in the plasma or in the DC arc and then you are measuring the emission from that excited sample as it returns to the ground state. But you have to direct a single wavelength of light through that flame and sample for the atomic [absorption] process.

GROSS: Now for the source of that light, was that typically like a . . .

KANE: It was a hollow cathode lamp, [or an] electrodeless discharge lamp. Different elements behaved differently in terms of whether you could use a hollow cathode lamp or whether you needed a different process to generate that wavelength of light.

GROSS: Okay. So another couple of terms that you threw out before, just . . . I know that you're very excited about the various forms of spectrometry [. . .] but I just want to make sure that a couple of terms get clarified. Titrimetric, is that just titration type analysis?

KANE: That's just the titration type analysis.

GROSS: All right. And for colorimetric analyses, I mean, I assume it's based on color. Were there standardized color charts or were you . . .

KANE: Well, yeah. You've got a UV visible spectrometer. You have your sample in a—you know, a cell, like a test tube. And again, you are then measuring the intensity of the color that has developed at a specific wavelength. Different reagents . . . molybdenum blue, for instance, is one of the easiest one for me to think of, [and it is used in determining silica. There was also a colorimetric determination for molybdenum as a trace metal in the rock]. [. . .] But what you are measuring is the intensity of the color development at a specific wavelength. The reagent you add to the sample reacts with molybdenum or whatever [other constituent in the sample].

[. . .] But you can also do ion exchange separation to remove the bulk of the matrix. You can do any number of other kinds of things, and they all had their place in the lab. There were [. . .] no less than twenty of us [and probably more] at the laboratory in Reston, then we also had other laboratories in Menlo Park, California, and in Denver, Colorado, that were all part of the [Branch of Analytical Laboratories or BAL within the] USGS geologic division.

GROSS: Okay. This gets nicely into a question I had about overall organization as well as your kind of introduction to this new environment. [. . .]

KANE: USGS had several divisions [and several branches within each]. There was a coal . . . well, the coal branch would have been in the Geologic Division. There was a Geologic Division. There was a Water Resources Division. There was a Division of Exploration Research [or] maybe Exploration again was [a branch] in the Geologic Division. But there was a Mapping Division. You know, the old geological survey maps. That was a huge part of USGS.

GROSS: Certainly.

KANE: There was a Technical Reports Unit.

GROSS: Now where were you in this?

KANE: Where was I in this? I was in Reston, in the Branch of Analytical Laboratories, in the atomic [spectrometry group] as opposed to x-ray or neutron activation [or another] group.

GROSS: And the Branch of Analytical Laboratories was in the geological section?

KANE: Was in the Geologic Division.

GROSS: Okay. So you were in the [atomic spectrometry] group? How many people were in that group? [. . .]

KANE: I would say there were ten of us in my group [. . .] at least, and then there was the x-ray group and the neutron activation group as well. [DC arc optical emission and rapid rock were also separate groups within BAL].

GROSS: Okay. So within that group, when you first arrived, were you assigned to work with someone in particular, or were you sent out on your own?

KANE: No, basically the group leader [Fred Simon] gave me a great deal of instruction. [. . .] And then the group was such that there was a lot of interchange, and you might be paired with this person for learning this analysis, with that person for learning the next one. There was a great deal of collegiality and sharing and cross-training and working together that, you know, I never felt abandoned to teach myself in any way at all. [. . .]

GROSS: All right. And how would you compare [. . .] the USGS to RCA Labs as a place to work?

KANE: [. . .] At [USGS] it felt like [I] was part of a much larger group than I had felt a part of at RCA. [. . .]

I had interactions with many more people. And also because it was chemistry rather than electrical engineering, I also had a totally different knowledge level. Even though geochemistry is different from chemistry and instrumental analysis is different from the gravimetric, titrimetric kinds of [analyses] I had experienced in college, there was still much more common ground that made a totally different feeling in terms of how well I fit in and that kind of **<T**: **35 min>** thing. [. . .] So there was much more direct contact with Fred over all the years I was at the labs . . .

GROSS: Compared to with Dave.

KANE: . . . compared to the contact with Dave. And there were more colleagues to associate with on a regular basis, where I felt like I knew what I was doing with them at USGS.

GROSS: Right. The organizational structure, it sounds like from what you were telling me yesterday, at RCA was relatively diffuse or difficult to pin down?

KANE: Difficult to pin down, or again, I just [. . .] wasn't paying attention to it.

GROSS: But it sounds like here, by which I mean, at USGS, it was a little bit more clear-cut?

KANE: It was both more clear-cut and it was on paper and spelled out almost from day one. [. . .]

GROSS: Now you mentioned that this was a whole lot of new material being thrown at you very quickly, and not just material, but also techniques. You also alluded just a moment ago to people who would help you learn some of these different instruments and analytical tools that you would be approaching. Could you comment a little bit or give a specific example or two of someone who helped walk you through the process of learning one of these techniques, and how that sort of relationship evolved over time? How did you, like, for example, first learn how to do, you know, atomic absorption spectroscopy or what have you? Or spectrometry, sorry.

KANE: Well, basically, you know, the very first lab assignment, you had written procedures, in the first place. And in the second place, the very first time you were doing something, you were doing it in someone else's lab, with them kind of watching over your shoulder the whole way. And who was doing it varied considerably. [. . .] And Fred and Paul Greenland were involved as well, so that you were shown . . . and there was always someone to talk to as you were doing it. And yet the techniques, those techniques for the most part were rather straightforward and easy to learn, and I don't recall there having been any real difficulty.

GROSS: Okay. By that point, there were . . . were there standardized instruments or were you basically . . .

KANE: Oh, yeah. The first ICP optical emission was not a commercial instrument, but all of the atomic absorption instruments I used were commercial. And as I think about it, the flame atomic [absorption] was definitely there right from the beginning. I don't remember whether the graphite furnace atomic absorption [. . .] was there when I first started working, or whether it was newly introduced very shortly afterwards, but that let you lower detection limits, but it forced you to do chemical separation. With the flame, you fundamentally had no need to separate the element you were looking for out of the total dissolved rock solution. But with **<T**: **40 min>** graphite furnace [technique, separation was often a necessary first step]. [. . .]

GROSS: Right.

KANE: So [with the furnace you have a higher atomization temperature, so you can atomize] the refractory elements, which can't be done in the flame at all. [You also have greater sensitivity for determining some of the lower concentration elements that you could not detect in the flame]. But the atomization process in the furnace had so many matrix interferences with it that you had to have separated it out first, and cadmium dithizone extraction is the first one that I used routinely in the way of the graphite furnace [analysis]. [. . .] The sensitivity was just altogether different for the two forms of atomization.

GROSS: All right. It sounds like you had a great deal of support as you were going through . . .

KANE: Yes.

GROSS: . . . learning all of these different techniques. And also based on the list of people that you mentioned, it sounded like there was a bit of a closer gender balance. Would that be accurate? Were there more women working?

KANE: There were many more women. One of them was a PhD. How do I say it? All of us were in a service division, so all of us were doing day-to-day analyses of samples to give other people the information they needed for their research, rather than doing a great deal of research ourselves, on the one hand. And on the other hand, because all of these new instrumental techniques were being introduced, and the possibility of expanding the element list beyond the majors or beyond the minors, major being over one percent and minor being between a tenth of a percent and one percent, and then as the new techniques came in, you could get down into . . . by now, you can get down to part per billion, part per trillion. You couldn't initially. So all of that was creating a demand for new methods.

And so the research within the Branch of Analytical Laboratories was about developing the method that would let you measure an element that you couldn't measure last year. Or to go from being able to measure it very accurately of 100 [parts per million, ppm] to being able to measure it equally accurately of 100 parts per billion [ppb]. And so within the branch, there was a fair amount of method development of that sort. Most people stuck with doing the routine analyses and turning out an analytical report for the geologists. Some of us did the method development research.

GROSS: And that was one thing in which you were interested?

KANE: And that was one thing in which, you know, I just took off and flew with. And even the ones . . . even the ones who did the research and co-publish . . . I mean, most of the method development work was two or three people working together, and one perhaps being the leader in it, and the others, again, **<T: 45 min>** being the hands that did all of the analysis as the method was being developed. I did both halves of it. But some of the women who got co-authorships on the method development papers probably did only the . . . not the real thought of how do I . . . you know, what is the chemistry that I'm going to apply, but you got your name on the paper because you did the analysis in the number of replicates that would both demonstrate that the method worked, but also then provide the data on the existing collection of standard rocks that USGS had, so that other people then had a quality control material. Probably half the women did as I did, but others really did . . . you know, were the technical support behind the method development, rather than directly involved in the literature search and the thought process and the let's try this and let . . . you know, but would apply the method to the standard rock data set after the method had been pretty much blocked out. [But they were as critically important as those who designed the method and could generally apply it to hundreds of samples with great precision, which was hugely important].

GROSS: So another question I had about this new environment was whether or not you encountered the same degree of chauvinism.

KANE: A little bit, but not as much. Both at USGS . . . less at USGS than at NIST. But there was some degree to which the PhD's on staff had far more flexibility about doing research, far less responsibility for carrying a part of the analytical support work. [. . .]

Eventually, and I can't place exactly when, we also had a Branch of International Geology. [. . .] Again, I can't exactly place, but the Branch of Analytical Laboratories and the Branch of Exploration Geochemistry at some point during my time there were merged. Branch of Exploration Geochemistry, [GX or] BGC, was only in Denver [Colorado], and A-Labs [or BAL] had been at the three sites. And so now instead of having the branch chief in Reston, which had been headquarters—and so when we were Branch of Analytical Laboratories, the branch chief was located where we were—once we merged with GX, the branch chief was in Denver, because the Exploration Geochemistry Branch was larger than BAL, even though it was all in one place. So the branch chief was located out there, and my supervisor then was out there as well at some point. I think Fred moved to the International Division, and Dan [Daniel W.] Golightly became the supervisor of the [atomic absorption and optical emission spectrometry, the AAS and the OES]. And at that point, [Dan] was developing **<T: 55 min>** a non-commercial inductively coupled plasma instrument with Akbar Montaser at GW [George Washington University] University in DC, and the two of them were collaborating very, very extensively. They also had a gentleman from [JMU] . . . James Madison University, over in . . .

GROSS: G or J?

KANE: I think it [was JMU]. GW . . . was George Washington University in DC. That's Montaser. Then it's James Madison University that's in Harrisonburg, Virginia.

GROSS: Okay.

KANE: I believe. And that's where . . . I think it's Jim [James J.] Leary—he came out summers, when the university was not active, and worked with Akbar and Dan on simplex optimization for the ICP.

GROSS: Now when you say that this instrument was inductively coupled, what does that mean exactly?

KANE: All right. What you've got is a radio frequency coil surrounding the flame of helium gas that's the tip of the torch, and it's the radio induction that triggers the sparking and the flaming of the helium gas. So you've got a glass torch through which the helium is injected, and [surrounding the torch] you've got a radio frequency coil. . . . Well, [. . .] and then above [the tip of] that torch, that coil will basically cause an explosion of the gas. And it's [in] this ball of fire, much hotter than anything in an AA flame, even when you go to the nitrous oxide flame, or you go to the furnace atomization [that the excitation leading to emission takes place]. It's really hot.

GROSS: Okay.

KANE: And again, you're aspirating a solution, and it's in this flame, and then because of the temperature, you get a totally different degree of atomization, ionization, and then excitation of both the atoms and the ions, because you have emission lines from both in this plasma. So you've got a much more complicated spectrum than you had with DC arc, and you're again multielement, because it's emission rather than absorption, and you don't have to have a single line from the hollow cathode lamp. Now of course when I was doing the work on the [simultaneous] multielement atomic absorption [SIMAAC], that's another story. But there you were using a deuterium arc continuum source, rather than a single-line hollow cathode lamp.

GROSS: It sounds like you got very heavily involved in the technical side of this sort of instrument design and testing process.

KANE: Oh yes. Not [. . .] the design, but the testing, and using, and making sure that it would give you accurate results.

GROSS: Right. So you know, figuring out if everything was in the right place, making sure that everything was in the right position, tweaking and adjusting things as necessary. These were all things that if you had told me about it yesterday when you were, you know, describing, for example, your master's research . . .

KANE: A totally different world.

GROSS: Well, this is what I'm kind of interested in.

KANE: Utterly and completely different world, and a world in which truly I was less comfortable. I was involved in all of it, but I was not the lead person. I could never have been the lead person. It was almost like the difference on that instrumental development stuff, where I was more the technician giving them support and less the lead researcher. I was more dependent on other people than independent . . . you know, I could design an extraction process that would separate cadmium from **<T: 60 min>** that rock solution that had everything under the sun that was in the rock in it.

GROSS: Right.

KANE: I could do that totally independently.

GROSS: Okay.

KANE: I could not have done these other things with anything like the same degree [of independence]. I could be part of a team, but I could not be a solo player.

GROSS: Okay. So let's start going through some of these projects and try to pin down not only when they happened, but who else was involved, and where you fit, if that makes any sense. So all of these instrumentation techniques that you've just mentioned, were these from the outset . . . like when you arrived at USGS, you were already involved in the inductively coupled plasma project, the . . .

KANE: No. The inductively coupled plasma project didn't even begin until a few years after I was there. [. . .]

GROSS: What was your first assignment when you arrived?

KANE: The very first assignment was in Fred's group doing . . . and I was doing exclusively atomic absorption work. And then as time went on, within that atomic absorption work, I was doing different types of sample prep, like the cadmium extraction, like the bismuth [extraction], that is probably my very first paper at USGS.[4] Oh, because that was . . . yeah. All right. I got there in 1976. And in 1979, the bismuth method development paper for atomic absorption spectrometry with electrothermal atomization. Again, that little graphite tube as opposed to . . . was 1979. So my guess was we had the flame instrument for sure—and not just the first one that they'd ever had, either. It had probably been around for eight or ten years. [. . .]

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[4] J.S. Kane, "Determination of nanogram amounts of bismuth in rocks by atomic absorption spectrometry with electrothermal atomization," *Analytica Chimica Acta*, 106 (1979): 325-331.

So the flame was either air acetylene or nitrous oxide, depending on the temperature flame you were trying to achieve, and they were there [when I first arrived at USGS in 1976]. And that technique probably . . . first commercial instrument, if I remember correctly, was roughly 1960. So it would have been there and in use for a reasonable length of time before I got there. The graphite furnace was a newer development, and while it may have been there when I first arrived, it may also have come in . . .

GROSS: Around the same time?

KANE: . . . around the same time. But that [generally] did require the chemical separation of elements. And so 1979 is the publication date, which means I probably did the work in 1978.

GROSS: Right. And this was stuff like . . . that was the bismuth piece?

KANE: Bismuth. The bismuth extraction for graphite furnace atomic absorption.

GROSS: Okay. So you started off with atomic absorption work. You moved on to sample prep, the bismuth extraction process. You mentioned another one I think as well.

KANE: Cadmium was done for coal, and that . . . I'm not sure I developed that rather than I *used* it very extensively.

GROSS: Okay. So after doing sample prep, what next? Or I know there was probably some overlap here.

KANE: Yeah. The sample prep was **<T: 65 min>** a part of doing . . . you know, whoever did the atomic absorption analysis [also] prepared the samples for it. [. . .] So, I mean, I was doing the sample prep from day one.

GROSS: All right. But your main analytical technique, once you had done the sample prep, at that point was atomic absorption?

KANE: Was atomic absorption.

GROSS: And this was both flame and then later heated graphite?

KANE: And heated graphite.

GROSS: Okay. My next question is at some point you switched from doing just atomic absorption work to other forms of analysis?

KANE: Other forms of analysis, and again, some of it was simultaneous [with the AAS work] . . . because there were some elements that simply couldn't be done by atomic absorption. And so the colorimetric molybdenum and tungsten and things of that sort were . . . you know, you take a week out of atomic absorption and you do a number of these samples. [. . .]

GROSS: Well, this was one question I had, is exactly how much the machine did the work, right?

KANE: The machine does all of the [measurement] work, in essence. What you are doing is you're preparing the sample. Your job is to get the sample to the point that it can be introduced to the machine, [then to introduce it and] to record the machine reading. [. . .]

And then you could go to a calculator and you would do a least squares fit of the absorption readings for your calibration standards, and then you would back-calculate then every one of the absorbance readings for your samples from that calibration curve. And as time went on, more and more of that became automated, but initially, you were doing . . . you know, there were no [automatic] sample changers initially.

GROSS: You were the sample changer.

KANE: There was no computer printout recording [the absorbance readings which were fairly stable for flame AAS but highly transient and therefore recorded on a strip chart recorder for furnace AAS. You would get the absorbance later as peak height from the tracings]. [. . .]

GROSS: Okay.

KANE: So that changed fairly dramatically over time. [. . .]

GROSS: You were doing sample prep for atomic absorption spectrometry early on. How did you get more involved in developing or refining these newer techniques? The plasma emission one is the one that comes to mind, but it's not the only one, correct?

KANE: Yeah. It was rather interesting, and I don't remember which came first or whether they were happening simultaneously in terms of the multielement atomic absorption work . . .

GROSS: And the plasma?

KANE: . . . and the plasma. But I was involved in both of those as instrumental techniques. Jim [James M.] Harnly was a PhD candidate [working at the University of Maryland] initially, and then, you know, a postdoc [. . .]—working with Tom [Thomas C.] O'Haver, who was at University of Maryland. That's where he [Jim] had gotten his degree. But just, you know, two miles down the road from University of Maryland is the US Department of Agriculture's research facility. So. Jim was at USDA, perhaps while he was working on his degree, definitely as a postdoc, but he was looking for a permanent appointment, and he wasn't getting anywhere. He applied to USGS, and Dan Golightly hired him, and basically said, "Jean, I want you to work with Jim on getting this instrument—finishing the development of this instrument and making it a part of what we're doing." And I was thrilled with the opportunity. Just getting the appointment at USGS meant that USDA, who hadn't been able to move on a permanent appointment [for Jim], could, and did. And so he never came to USGS at all. He had been hired. He'd been offered the job and hired, but he never actually came on board, because just that stimulated USDA . . .

GROSS: Right. That was enough pressure to . . .

KANE: . . . to do what Jim had wanted in the first place. And so I spent a couple of years . . . it's hard to remember how many, going back and forth every Friday from Virginia to Beltsville [Maryland] to the research station and working in Jim's lab with him on this instrument. And **<T: 80 min>** either Nancy [Miller-Ihli] was already there as a PhD candidate under Tom O'Haver when I first arrived, or she came in very soon thereafter. [. . .]

Again, one of the first direct acquaintances with—other than Marian Schnepfe at USGS—with a female PhD [chemist] or PhD candidate. And so the three of us spent a couple of years working quite extensively.

GROSS: This is you and Jim and Nancy?

KANE: Jim and Nancy, working together quite extensively on . . . all right, the fundamental theory is that flame conditions, measurement height in the flame, all of these things in atomic absorption are fairly element specific. And so we were doing an optimization study, you know, [of how high in the flame] . . . into the blue, into the yellow, into the orange, [to make the measurement to see if sensitivity and/or accuracy changed as measurement position did]. And to my mind, if I'm getting 99 percent of true value in one set of flame conditions and 100 percent in another, and 101—but all of those are plus or minus 3 percent, there is no difference.

GROSS: Okay.

KANE: So we did a lot of that kind of work that—that basically refuted a lot of what had been thought to be the case about multielement atomic absorption would never work, because you needed different conditions for every element, and indeed you didn't.[5] We were using, instead of the standard atomic absorption instruments, it was an echelle grating instrument. And we had sixteen photomultiplier tubes behind various slits. I mean, again, it's not as multielement as emission. You can't—you can't do thirty-two—just the geometry was such that a bank of sixteen PMTs was about the best you could do. But the elements they chose at USDA and the elements that we chose at USGS when we started duplicating the instrument were totally different, because I was interested only in the trace elements in a rock in a geologic sample, and they had some of the elements that I would have considered majors or minors that were critically important nutritionally, that they had to have on their cassette. We were looking at slightly different elements. And I would have to go back to both of our original papers to figure out exactly what the differences were. [. . .]

GROSS: And the other difference you mentioned was that in a typical AA setup, you would have a different light source?

KANE: You would have a single . . . you would have to change the hollow cathode lamp. As you went from measuring copper to measuring lead to measuring zinc, you would put a different lamp in position. But with the multielement, you're using a continuum source deuterium arc lamp. So you now have everything, and in terms of the way your cassette is set up in terms of the slits that you've chosen, you're selected out sixteen very specific wavelengths . . .

GROSS: Along this continuum?

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[5] J.M Harnly, J.S. Kane, and N.J. Miller-Ihli, "Effects of air-acetylene flame parameters on simultaneous multielement atomic absorption spectrometry." *Applied Spectroscopy*, 36 (1982): 637-643.

KANE: . . . along this continuum.

GROSS: Okay. Now, it was really you, Jim, and Nancy who were working on this device?

KANE: Right. Tom O'Haver was Jim's and **<T: 85 min>** Nancy's thesis advisor, and obviously he was a major consultant in the process.

GROSS: Certainly. So my question is what roles did each of you play in the course of this development?

KANE: It was Jim's instrument. [. . .] He and Tom had done all of the engineering and had set the instrument up. We were using the instrument to do these optimizations. That is to say, yes, it really does [provide accurate results] . . . to demonstrate the effectiveness of the instrument.

GROSS: Okay. [. . .] Let me ask you another question. You mentioned that there was a transfer of this instrument from USDA to USGS.

KANE: No, we built a new one.

GROSS: You built a whole new one?

KANE: We built a whole new one at USGS. And again, Jim was very much the coach, very much involved in it, but I built it at USGS. [. . .]

GROSS: Okay. This is interesting to me, because as far as I know, even though you were getting better through all of your experience at doing the actual hands-on work involved with this equipment, you have never come across in our previous discussions of your research as someone who built a lot of complicated instruments.

KANE: Well, when I say I built it, we assembled pieces. I mean, I could buy an echelle spectrometer from Spectrometrix [Optoelectronic Systems GmbH]. I could buy the graphite furnace or the flame unit for atomic absorption from PerkinElmer [Inc.]. I could buy the computer that did all the controlling from DEC [Digital Equipment Corporation]. So that it wasn't building it in quite the same sense that the guy who in the very first instance figured out an echelle spectrometer as opposed to a different kind of grating. It was assembling components and getting them put together in a way that would allow us to have a full system.

GROSS: But that still requires a level of kind of hands-on tinkering ability.

KANE: Yes, it does. [. . .] The machine shop at USGS and other people helped me with a great deal of that. [. . .]

GROSS: This is just a very different kind of work that . . .

KANE: Yeah, I didn't even like to have to change the torch in the ICP spectrometer, didn't like to have to assemble the sixteen PMT tubes in the echelle. I did those things. I had to do those things. But they were not—they were not the kinds of things I felt comfortable doing, rather than felt stretched in doing. [. . .]

Jim could uncover the different pieces of his instrument and basically show me what it looked like fully assembled. And from that, I could go and do it. But, I mean, when I say I built the instrument, it really wasn't as complicated as some building jobs might be [and it did not involve any of the initial design creativity].

[. . .] But it was still challenging. It was still . . . you know, it was still the area where this isn't what I do well. This isn't what I like doing. But it's also—it had to be done, and I couldn't be . . . I could ask for help, but I couldn't be totally dependent. [. . .]

GROSS: Well, it also was a chance for you to stretch your comfort zone a little bit.

KANE: To stretch. Exactly. And it's always a good thing to do.

GROSS: [. . .] Let's talk a little bit about the other instrument that you worked on, the inductively coupled plasma. How did you get involved with that project?

KANE: Well, again, it was the same group within the Branch of Analytical Laboratories that did **<T**: **90 min>** optical emission spectrometry and atomic absorption, because they're both atom/ion as opposed to x-ray or [nuclear activation analysis]. And again, because I was one of the only ones who was publishing regularly and who was interested in doing something more than just the routine day-to-day stuff, the opportunity was offered and I grabbed it.

GROSS: Okay. Who offered you the opportunity?

KANE: Well, that was . . . Dan Golightly was the project leader at that point. Fred was no longer . . .

GROSS: And this was internal to USGS?

KANE: This was internal to USGS.

GROSS: Okay. So you described already sort of what the instrument looked like, the inductively coupled . . . when you were telling me about the torch and the coils and what have you. Were you also involved in a similar way in the construction of the instrument, like you were with . . .

KANE: No. By the time we had that instrument, and I was involved, it was a commercial instrument. The original prototype that Dan and Akbar [built and] worked on . . . I had not been involved in at all. But this was after we were introducing a commercial instrument, and were going to be training then most of the optical emissions staff to use it on a regular basis that we were developing the protocol and doing the spectral interference investigations and things of that sort.

GROSS: Okay. So your role in that project was basically similar to a . . . it sounds like it's similar to a piece of your role in the other, which is making sure that the instrument could be running tests to make sure that the instrument could be used in a way that would be productive for the same sort of tasks. Right? For the service . . .

KANE: . . . function of the group, and also you know, one of the things I did was to take what had been several fairly tedious colorimetric or graphite furnace methods, because beryllium was another thing that we did extractions on all of the time, beryllium, tungsten, and molybdenum were all extractions. The moly and tungsten were colorimetric. The beryllium was graphite furnace AA. Tin was graphite furnace AA. That was another extraction. So we had these four extractions, and then lithium was a flame atomic absorption element. And once we had the ICP, you could do the five of them simultaneously, and you could eliminate all of the extractions. And so it was a major efficiency step to find . . . you know, and again, it stood to reason that you could do this, but until you [had] run a number of the reference samples that were already in existence and had demonstrated that you could get correct results for them [. . .], [you couldn't make the switch], so I did a lot of that kind of thing. There was another project on which I did a different suite of trace metals that again had previously been done by atomic absorption, one element at a time, and moved to doing several of them at once on the ICP, and documenting that [. . .] we haven't degraded the accuracy. We can give people what they need, but it's a quicker process. It's much less . . .

GROSS: You've introduced a new level of efficiency?

KANE: Yes.

GROSS: Okay. One thing I didn't ask about during this, I guess I should have, was to try to pin down timing **<T**: **95 min>** a little bit. This is all during the phase that you marked out on your professional history as between 1976 and 1988. The two instrument projects that we've discussed here, the multielement and the plasma, are those both happening simultaneously? Did one come before the other? Are they happening like early 1980s, mid-1980s? [. . .]

KANE: The first meeting paper on the work with Jim Harnly was September 1981 [and the journal publication of that work appeared in 1982].[6] [. . .]

So I would guess late 1980, early 1981. Jim and I started working together, and we continued publishing together through 1984 or 1985. And then the USGS [SIMAAC] instrument showed up . . .

[. . .] All right, 1988, 1989, so the publications would have come out, you know, with a six- to twelve-month time lag. I would say that was like 1985 through 1988 was working on the USGS instrument. And that reminds me of the fact that that was one of the more awkward moments at USGS. Multielement AA was something that was eminently possible, but because it came in pretty much simultaneously with ICP and because instrument companies were not interested in developing both, it was going to be either/or. It very quickly became apparent that multielement AA was going to die. It was not ever going to be . . .

GROSS: It was never going to catch on?

KANE: It was never going to catch on, partly because even with the multielement capacity, you had a sixteen-element limitation, whereas you did not with the ICP, partly because, you know, as I said, instrument manufacturers just were not going to go in two directions at once. So once that became apparent, USGS lost total interest in supporting further development of that

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[6] Harnly, *et* al, 1982.

instrument and that capacity for the laboratory. It would be too much a research novelty and never become a routine, everyday something. [. . .]

So literally overnight I was simply told I was going to have to shut the instrument down and [move to an assignment that I saw as a career dead end]. [. . .] I just drew a few very hard lines in the sand, and I said, “You’re not going to do that to me. You can take the instrument away from me. You can say it’s never going to go. But give me six more months with it to finish the documentation, the publications that are started, and find something else for me to do [that will be equally engaging, not one that represents a downward slope].

[. . .] And I got away with it. I made an awful lot of people very angry. I got the people at USDA and Tom O’Haver at Maryland to write to our chief geologist and to complain about the interruption of the collaborative process and did a few other things that really [upset folks] . . . you know, everybody wanted this to happen without a paper trail.

GROSS: You made sure there was a paper trail?

KANE: And I made sure there was a paper trail. [laughter] And again, as I think about it . . . was it because I was a woman? I don’t . . . you know, I don’t want to go there. I know in industry how many men have been terminated on two weeks’ notice. I wasn’t going to read it that way.

GROSS: Well, you weren’t going to be full-out terminated here, but . . .

KANE: No, but I was going to be put in a position where I saw it as a total dead end. Now, it may not have been. [. . .] But it created the opening to take over the standard rock program when Frank [Francis J.] Flanagan retired, which we’ll get to later. You know, that was [a] time when I rocked the boat a little harder than people liked, but . . .

GROSS: But it worked out. [. . .] So at this point were you still officially part time, or were you doing this . . .

KANE: No, I was I was full time before I even started on the multielement work with Jim. The transition from part time to full time probably happened somewhere in the first two to three years. [. . .] I was there as a full-fledged research chemist [. . .]

GROSS: Right. So that was one question I had. Another question, which we've been alluding to this publication list, and I hardly have asked about it, was about research documentation policies and publication. Now we talked a little yesterday about notebook use at RCA.

KANE: Right.

GROSS: Did they have a similar sort of notebook policy at USGS?

KANE: Yes, they did, [as far as keeping detailed records of the work, but not of having someone else witness every page], and again, how carefully anyone else followed it I don't know. I think when I left, there were about twenty of my lab notebooks. What became more and more difficult was that over time, so many things were on computer printouts that just could not be fitted into the notebook any way, shape, or form, and it's what do you do then?

GROSS: Were you expected to turn these things in to your supervisor, or were you just expected to hold onto them?

KANE: You were . . . I mean, there was a long bookshelf in my office that **<T: 105 min>** just had a line of numbered notebooks, and [when I left, I left the notebooks on the bookshelf].

[. . .] I had a couple that I considered kind of my own private thought process and literature-survey type stuff that I carried home with me, but anything that was laboratory data for the samples I had analyzed, what that turned [into] was a job report on each batch of samples that you analyzed, and that got turned in, and eventually through a quality control process got reviewed, and this, that, and the other, and developing a set of documentation on all the standard rocks that had been run with your batch of samples to confirm the accuracy of your data and things like that.

GROSS: Right.

KANE: So there was quite a bit there.

GROSS: You didn't have to turn in any sort of, I don't know, equivalent to the private company report we were talking about yesterday, right, with . . .

KANE: There were open-file reports that were used very frequently, much more extensively in the Exploration Geochemistry Branch than in A-Labs. But every open-file report counted as a publication. So if all you were doing was service work but you put every single lab report into an open file, as opposed to just a lab report that was turned over to the geologist, you could build quite a publication list. Now, I don't think I did more than a couple of open-file reports myself. I did one, when we had two or three, I don't remember, Egyptian Geological Survey chemists came and brought one hundred or more samples with them. And were trained in our laboratory on how we did our analyses, as a prelude to buying their first atomic absorption instrument and things of that sort. And we definitely did an open-file report on their sample results.

I did an open-file report documenting the construction of our multielement AA system. I did an open-file report years before that, when I wrote a statistical program for the analysis of variance that was part of the bismuth paper and some of the other papers that I did.[7] They always wanted an analysis of variance for between bottle differences in standard rocks to prove the sample homogeneity. And this had been done, you know, on hand calculators and longhand and that kind of thing forever. And I wrote a Fortran program the first year or two that I was there, and I probably didn't even put that one on the list. But I had mentioned yesterday contacting Tom Zajicek when I first went to USGS for [a copy of the program I wrote for my thesis work].

[. . .] That I would have a refresher course, and that was what it was used for. Now by today, there are statistical packages by the hundreds that you can buy that do these calculations for you.

GROSS: Sure.

KANE: But at the time—this was 1976 or 1977—those programs weren't yet commercially available and doing analysis of variance by hand is quite a chore.

GROSS: True.

KANE: So I wrote the program.

GROSS: Did you have access, then, to your own computer in your lab?

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[7] Kane, 1979.

KANE: No.

GROSS: Or did you have to share.

KANE: No. You know, back then, it was an IBM computer up on the seventh floor and, you know, again, punching cards and going up and feeding them in and all of that kind of thing. I did not even have a PC **<T: 110 min>** of my own when I first took over the standard rock program in 1988 or 1987 [when Frank Flanagan retired] . . . I bought one at home and started out that way.

GROSS: More broadly, you had access to AA equipment in your lab and also the multielement that you built. [. . .]

KANE: I also used the plasma one extensively. Now, we all had a chemistry lab for the sample prep, but you don't want any of the acid fumes anywhere near the instruments. [So yes, we all had access to instruments, but not in our labs]. The instruments were in common use . . .

GROSS: Spaces?

KANE: . . . spaces where I might use the ICP on Tuesday and Friday mornings, and somebody else would be using them on Monday and Wednesday afternoons or whatever. I mean, they . . .

GROSS: So it wasn't like everyone had their own set of instruments. This was my question.

KANE: Everyone did not have their own instrument at all.

GROSS: Okay. I figured they were expensive, and, you know, you wanted . . .

KANE: There were common instruments that we basically . . . again, as we were all working on our samples, we kind of keep touch with which day do you need the instrument? Which day can I use it? And plan our work around that kind of thing. I think by the time I moved over to the standard rocks, there were probably three flame units, two or three graphite furnace units. There was never more than one ICP in Reston. There [were] clearly others in Denver and in California. But, you know, and there was one LECO sulfur analyzer, for instance, and there was one specific ion electrode setup for the fluorine analyses [we] were doing, [. . .] because there were some things [that we had to do that couldn't be done on any of these other spectrometric instruments].

GROSS: Okay. So one other thing that I was curious about. Getting back to the publishing question, was about publishing what it looks like were some of your first independent peer-reviewed papers. How did that process surprise you? What did you expect going into it? Because you had written . . . at RCA I think you've listed one paper that was a collaborative paper.[8]

KANE: Was a collaborative paper, and it was the only paper. . . . At USGS, there were several aspects of the publication situation. Because I basically developed the bismuth method and did the hands-on part it was a single-authored paper.[9] It would have been a multi-authored paper if I had developed it and then one of the other chemists had done most of the hand work. It would have been co-authored. The one paper of that—well, there were several of that type at USGS that were collaboratively authored. Tony [Anthony F.] Dorrzapf was the group leader on the ICP at one point. I was doing most of the ICP [and AAS] work on massive sulfides from the Straits of Juan de Fuca. And at GEOEXPO/86 in Vancouver [Canada], I believe, we jointly authored a paper that was both a meeting presentation and then a symposium volume manuscript, describing the details of those two techniques and how they fit together and were used in the laboratory for geochemical analysis.[10]

[. . .] A lot of times the geologists would put the analyst on as a co-author when they were doing a paper about their interpretation of the data, but that wasn't routine. It wasn't normal. But that's where a lot of the NIST or the USGS co-authorships for anyone in the laboratories would come from that kind of a collaboration. I had very few of those, and again, because I did my own chemistry as well as the design of the procedure, a lot of what I did was single-authored, with the exception, of course, of the development work that I did with Jim Harnly and [Nancy Miller-Ihli] on the multielement [AAS], or with Dan Golightly and Akbar Montaser on the ICP, as it was getting started.[11]

GROSS: Now did you run into any trouble during the peer review process typically? Or did you find that to be relatively smooth?

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[8] J. Costellano, J. Goldmacher, L.A. Barton, and J.S. Kane, "Liquid crystals II. Effects of terminal group substitution on the mesomorphic behavior of some benzylidenes," *Journal of Organic Chemistry* 33 (1968): 3501-3504.

[9] Kane, 1979.

[10] J.S. Kane and A.F. Dorrzapf, Jr. (1987) "Current atomic absorption and inductively coupled plasma optical emission methods for geochemical investigations." In *GEOEXPO/86: Exploration of the North American Cordillera, Symposium Proceedings*, ed. I.L. Elliot and B.W. Smee (Rexdale, Canada: Association of Exploration Geochemists, 1987).

[11] Harnly, *et* al, 1982; A. Montaser, G.R. Huse, R.A. Wax, S.K. Chan, D.W. Golightly, J.S. Kane, and A.F. Dorrzapf. "Analytical performance of a low-gas-flow torch optimized for inductively coupled plasma atomic emission spectrometry." *Analytical Chemistry* 56, no. 2 (1984): 283-288

KANE: Typically I found the peer review process no big deal. Occasionally there were truly obnoxious reviews in the way of just tearing something to shreds, but for the most part there were no major issues. There were two or three experiences over time where someone was able to be very critical, but to do so in a way that left you saying, “Wow. You’ve given me the chance to take what was a B, B$^-$ paper and turn it into an A$^+$ paper, because you took the time to read it carefully enough and put the critique thoughtfully enough that it didn’t leave me feeling so put down and so angry that I wanted to just throw the paper away and not bother.” And for the most part, **<T: 120 min>** reviewers are anonymous in terms of the journals that I was submitting to, so you didn’t know who to say thank you to.

The one case where that did—you know, where I knew exactly who had said the perfect thing, Jim Harnly and I in one of the early development stages had gotten to a point where Jim said, “Let’s get it written up,” and I had said, “We really ought to do one more thing to make this really perfect.” And Jim said, “I don’t think so.” And it was his lab, and I was not going to argue with him. But for the internal . . . at USGS there was both an internal review process of your peers within the lab before you could even submit to a journal, and then an external review process. I asked Jim if he’d mind having Tom O’Haver be one of the internal reviewers, and he said, “No, that’s fine.” And at the sentence where we said, “We could have gone one step further but didn’t,” letters this big, underlined, exclamation points, Tom just wrote, “Too bad.” I showed that page to Jim. I didn’t say a word. He said, “Can you come tomorrow?” [laughter]

GROSS: So you added that in?

KANE: And we added that in the review process. Another time that I thought was absolutely outstanding, again, really taking a good paper and making it truly superb, was an invited paper, Phil [Philip J.] Potts at the Open University in 1993. So I was at NIST by this time, and I had done kind of a historical review of the development of a number of techniques in geochemistry. And the reviewer, you know, at first glance it was highly critical. And yet when I started working with the comments, “Oh, I can change this paragraph, and I can change that paragraph, and I don’t really need to do more than add two sentences here and three sentences there.” And when I was done, I felt like, “Wow, you know, this is so much better than what I had done,” and yet it obviously took someone who was both incredibly familiar with what I was trying to do and very, very willing to take the time to be constructive. So I sent the paper back to the editor of the journal with a note, “I know the reviewer’s anonymous, but please, please convey my thanks,” and got back from the editor, “Well, the reviewer had asked to see it again, and he told me to convey to you his thanks for having taken it so seriously.”

GROSS: Wow.

KANE: And so it was just one of those . . . there are not that many instances in a career that it just feels so right and so supported, but that was one of them.

GROSS: Okay. So I have one other question and that's about conferences. You mentioned that you had gone to GEOEXPO and a few other places, I assume, by that point. Those were your first experiences going to professional conferences, or had you been going to ACS conferences or other . . .

KANE: I even got to a few ACS conferences, but the conferences I enjoyed the most are the Geoanalysis Conferences, because there are somewhere between a hundred and two hundred people. They're small conferences. The Canadian Mineral Analyst conference that I was invited to speak at was probably even smaller than that. But again, because at—in the non-conference time, there's so much [more] sociability with people that have very, very common interests, I always found the ACS conferences . . . I went to a few, actually, while I was at RCA, or at least one while I was at RCA, and I went to a couple in **<T: 125 min>** Denver after that. They're just so huge. The one in New York City, where . . . you know, you're looking at ten parallel sessions. And there's a 10 o'clock paper that you'd like to hear, and there's a 10:30am paper you'd like to hear, but they're two miles apart, and there's just no way to get from one conference room to another, whereas at the smaller conferences, there's never any of those issues, so I always enjoyed them far more.

GEOEXPO, I don't know how big that was. It opened with a field trip to some mines, and it was my first field experience as opposed to in the lab experience with geology, so I really enjoyed that. MARM meeting, must have been Middle Atlantic Regional Meeting of ACS? That was one of the very first meeting papers that I ever gave. Went to a lot of the FACSS [Federation of Analytical Chemistry and Spectroscopy Societies] meetings. [...] I was actually an officer for a few years at the national level [in the Society for Applied Spectroscopy, SAS, whose meetings were held at FACSS, since SAS was one of the societies that cosponsored the FACSS meetings]. Those were smaller than ACS, meetings, but still much, much larger than the Geoanalysis [ones]. And then at NIST, it was *absolutely* mandatory that you went to [the Pittsburgh Conference, or Pittcon]. I hated it, but I had to go.

GROSS: Fair enough. Were there a lot of other women at these conferences? Did you make an effort to seek out other female chemists?

KANE: There were certainly more women [chemists] than I had encountered say at RCA or at Syracuse University or UMass.

GROSS: Sure. And did you notice a change over time, would be my next question. Right? Like they're . . .

KANE: Certainly. Dramatic change over time in the number of women who were giving the papers and who were session chairs and who had . . . I mean, even at USGS, once we merged Branch of Analytical Laboratories and Branch of Geochemistry, there was a female branch chief for a short period of time, and I believe there is currently a woman who is branch chief [whose name escapes me at the moment]. The one while I was there was Lori Filipek. [. . .]

GROSS: We talked about previous exposure to geological research. You talked to an extent about the difference between industry and government research. Did you find that you ultimately preferred being in the government setting than in industry? Or that they were just different?

KANE: They were just different. I don't think I could say there was a preference for one over the other. The focus was different, and clearly at RCA, it was . . . the research was not revolving around providing service to other scientists. It had a totally different purpose. Again, **<T: 130 min>** I don't think that interfered in any way with the pleasure I took in the research. I don't think that distinction mattered.

GROSS: Sure. Okay. I think that answers most of the questions I had about that period between 1976 and 1988. We're going to take a lunch break, and then when we get back, we're going to be talking about the Geochemical Reference Sample Program, then I guess your switchover to NIST to work at the Standard Reference Materials Program. And then a little bit about your time post-NIST, which you've told me previously is some of the more exciting stuff that's happened in your career. [. . .]

[END OF AUDIO, FILE 2.1]

GROSS: All right. We are back after a short lunch break, once again here at the Culpeper County Library. Ben Gross here with Jean Kane talking about life at USGS. When we last left off, we had finished talking a great deal about different instrumental projects that you had been working on, the multielement atomic absorption spectrometer, as well as the plasma-based atomic emission spectrometer. But in 1988, it looks like you became involved with something new: the USGS Geochemical Reference Sample Program. Can you talk a little bit about how you got involved?

KANE: Indeed. I took over, actually, a program that I'd been long familiar with at USGS when Frank Flanagan, who was the head of the program, retired. He had fundamentally started the program at USGS. A precursor even before his involvement was the work that was done collaboratively, USGS, Carnegie Institution in Washington, MIT Geosciences Department, back in the early 1950s, when a granite and a diabase were developed as reference samples that could be used primarily for the calibration of DC arc optical emission spectrometry. The difficulty with that technique was that the arc, in exciting the sample, does behave somewhat differently with each different matrix that you're looking at, and so there was no pure standard that could be used [for calibration]. With gravimetric analysis, you were simply looking [at mass, weighing on a balance]. [. . .] But you needed a calibration standard to be able to apply the DC arc optical emission technique to routine analyses, and the standardization was considered to be quite difficult. So at the beginning, something on the order of thirty or forty rock laboratories around the country known or thought to be expert in geochemical analysis were asked to analyze what was the same material. The granite [G-1] was collected in Westerly, Rhode Island. The diabase . . . W-1, I think, was [from] the Centerville Quarry in Centerville, Virginia. And these materials were then ground into the powder that would be used for rock analysis, and were distributed to all of these laboratories, so that everyone would be analyzing the same sample.

GROSS: With the same . . . I guess, I should ask this because you brought it up before. When you're talking about the term matrix in a geological sense, what does that normally refer to?

KANE: What that normally refers to is the major element [and mineralogic] composition. [The two are related but very different. Regarding composition,] a rhyolite, for instance, has 70 percent silica roughly. A granite has 50 or 60 percent silica. A basalt has on the order of 35 to 40 percent, and I may have the numbers wrong. But fundamentally, it's that kind of a change, [since] every rock is a mixture of a number of different elements, and there are fundamentally ten major oxides that make up a rock or that can be used to describe a rock composition, even though the rock itself is not specifically silicon dioxide, magnesium oxide, calcium oxide, [but specific minerals that can be represented as those compounds]. In looking at an analytical report, everything is reported as oxides. And the summation, then, of the complete analysis should be 100 percent. And so that is used as one quality marker. When you total all the oxides, do you get 100 percent? And that works fine until you have a lot of carbonate, for instance, because you're not doing a **<T**: **5 min>** carbon analysis as part of the majors. So if you have calcium carbonate instead of calcium oxide, then . . .

GROSS: That can throw off the numbers?

KANE: . . . that throws things off. And so you also do a loss on ignition, which should drive off any of the carbonate as CO_2, and if you have that loss on ignition number to add back in, then everything should work out fine. Again, sulfur is a complicating factor. Some minerals have a great deal of one of the halogens in them. And so if you've got halogens, if you've got sulfur in anything but very, very trivial quantities, then you still run into problems. But fundamentally, most rock types, if you do your ten majors, silica, aluminum, iron, calcium, sodium, potassium—I've left out one of them—magnesium, and then phosphorous, titanium, and manganese, those are the majors and minors that if you analyze for all of those and you sum them, you should typically, once you've added in loss on ignition, get 100 percent.

GROSS: For them and their oxides?

KANE: For them reported as oxides. And then you have the fact that iron could be FeO or it could be Fe_2O_3 and so then you get into other minor complications, if you're reporting everything as Fe_2O_3, rather than doing speciation. But in general, the classical techniques would have determined just those elements. And the thought was that gravimetry is the definitive method. Properly carried out, everyone in the world who used it would get the same number within that very small measurement [uncertainty] where nothing is quite perfect. And when they did these samples back in roughly 1952 . . .

GROSS: This is the basalt and the diabase?

KANE: This is the [granite G-1] and the diabase [W-1]. [The diabase is at the basalt end of rock matrices, but it has a bit more silica than a basalt does. Granites are more silica-rich than either diabases or basalts.]

GROSS: [. . .] In the matrix?

KANE: In the matrix of the rock. But again, every rock contains trace elements as well, and it . . . most of the time they're totally negligible. It doesn't impact the totaling. But the thought was that all of these numbers [for the major oxides] would come out about the same across the thirty or forty laboratories, and for silica, they did. But for everything else, the disparities were described in the early literature as the bombshell or the shock to the entire analytical community. Not to the chemists, because the chemists understood that they had the potential for errors in their analysis, but for the geologists who were using the data to define this rock based on its chemical analysis has to be this specific rock type, it was mind-blowing.

GROSS: And this was 1952, you said?

KANE: And this was 1952. And USGS picked things up after that. [. . .] There was a series of ["standard rocks"] that were developed under Frank Flanagan's direction, and were then analyzed fundamentally through round robins. [That first series of rocks included the Columbia River basalt, BCR-1; the granite, G-2, which was from the same quarry that G-1 had been from;

GSP-1, which was a granodiorite; the andesite AGV-1; and two ultramafics, PCC-1 and DTS-1.]

In doing G1 and W1, the aim was to find truly expert laboratories to do the analysis. As time **<T**: **10 min>** went on, it was "who out there in the world who does rock analyses is willing to contribute data to let us then try to arrive at a reference value for these materials?" And so there was not quite the degree of control on the quality of the individual laboratory contributions. Reference values were developed, but there was no standardized process for data treatment that gave you anything like the statistical control of the quality of the reference value or the input data to it that exists in today's world. And [. . .] once the database was published, Geological Survey of Canada, the South African Reference Materials Program [SARM], some laboratories in Scandinavia, the French, were all taking these databases that Flanagan had [collected and published] and doing their own statistical manipulation on them. And so you might end up with six reference values for theoretically the same constituent in the same sample. And then as analysts were using the reference material, either as a calibrant for a new instrumental technique that couldn't readily be calibrated any other way or as a method of validation material, "Am I getting the reference value?" Well, yes, they were getting the reference value, but they have five or six of them to choose from, and they always chose the one they agreed with.

GROSS: How convenient.

KANE: And so you didn't have a standard in the sense that we have been moving toward much more recently.

GROSS: You mean a single unified . . .

KANE: A single unified [value] . . . everyone agrees that we will use this value and nothing else. The reference value should be an indication of the true composition. And admittedly, because every measurement has some uncertainty in it, the true value will never be known to absolute perfection, but there are good estimates of it, and there are bad estimates of it, and as a function of how the materials are used, you may not be using the best estimate. The International [Organization for Standardization (ISO)] came into being long after this program got started, and there were parallel programs in several other countries at different . . . the French CRPG [Le Centre de Recherches Pétrographiques et Géochimiques]. [. . .] South African Reference Materials Program, SARM, and the Canadian Geological Survey, and others were all getting involved in this. And then the National Research Lab at Los Alamos got into the act, not so much with preparing materials like this, but with going through the literature, and every time somebody reported the result they got on this material as part of a method validation publication, the group at Los Alamos would pull the number out and develop their own database that was larger than the original one that USGS, Flanagan [the BCR and later series], [Harold

W.] Fairbairn [the original G-1 and W-1] at . . . whether he was at Carnegie Tech or whether he was at MIT on that first batch of materials, I don't even remember.

GROSS: Were these digital databases, like computer databases? Or were they just books of values?

KANE: Well, they weren't truly computer databases, because we're talking about the 1940s and the 1950s and the 1960s.

GROSS: Well, I can imagine there being, you know, spreadsheets of data or tables of data.

KANE: Tables. There were tables of data that would end up published in the literature. But initially, I'm not sure when the first computerization **<T**: **15 min>** of it became possible. Certainly the 1950s, I doubt that it was computerized. [. . .] Kind of everybody sent their [analytical results] in, and then the people who were collecting the data [principally Flanagan] would have created a handwritten . . . that could have been turned into a typescript for the final publication.

GROSS: Well, this was my question, are they being published as books or circulated volumes?

KANE: They were initially published as [books—either a USGS Bulletin or Professional Paper—or as journal manuscripts, for example, *Geochimica et Cosmochimica Acta*].[12] And I think even the Fairbairn study, even though the major collaborators were Carnegie and MIT, [the Fairbairn publication was a USGS bulletin].

GROSS: Now was this all being done, once it got under USGS's kind of purview, was this work being done where you were at BAL?

KANE: When I was at BAL, we were using these rocks as our control materials and to validate methods. We were extending the databases as the methods changed and improved, and we could either go to lower detection limit or add an element that had never been measured before to the database, so this was continuously being extended and then used in laboratories all over the—

[12] See W.H. Fairbairn, W.G. Schlecht, R.E. Stevens, W.H. Dennen, L.H. Ahrens, and F. Chayes, "A cooperative investigation of precision and accuracy in chemical, spectrochemical and modal analysis of silicate rocks," *US Geological Survey Bulletin* 980 (1951): 77; F.J. Flanagan, "Descriptions and analyses of eight new USGS rock standards," *US Geological Survey Professional Paper* 840 (1976): 192; F.J. Flanagan, "US Geological Survey silicate rock standards," *Geochimica et Cosmochimica Acta* 31 (1967): 289-308.

literally all over the world, because anyone who wrote to Frank Flanagan and said, "May I have a bottle of your standard A, B, or C to use in my laboratory," he'd mail it off.

GROSS: Right. I guess my question was more . . . this is a very, as you know, very analytical project by nature, right?

KANE: [Yes, while I was there, before and after as well].

GROSS: Being able to break these things down. Was the main analysis associated with the development of these standards, the initial like . . . not so much extended it to different techniques, but the initial development of samples, was this being done under BAL, or was it being done in another part of USGS?

KANE: It was being [initiated at USGS, but the analytical work was] done literally worldwide. [In BAL for sure but also in many other labs.] I mean, again, the first two, the G1 and W1 that were the collaborative USGS, Carnegie, MIT, and I'm sure there were other labs, because there were more than thirty labs who eventually participated, but they organized the study. You send the sample out and you ask people around the world to analyze it and send the data back to you and then you do the statistical manipulation of the data to arrive at a reference value.

GROSS: At a standard. So where was that central hub? Where was Flanagan working?

KANE: Flanagan was in Reston. Well, I say he was in Reston. They [The USGS National Center staff] were originally at the Navy Yard in DC, and they moved to Reston sometime before I started working for them. [. . .]

[From there the reference samples radiated out for analysis, and the results came back to the National Center to be compiled and evaluated, so that reference values could be derived from them.]

GROSS: So Flanagan was in the same building as you when you were there?

KANE: Yes. By the time I was there, he was a rather cantankerous old gentleman. When he retired, he truly had made up his mind that nobody else on this globe could do the job that he had done, and he was not willingly leaving anything in the way of records of what he'd done behind. So there was a bit of a flap about that, that . . . you know, enough said. I had a very hard time finding any records when I took over the program. At the time I took over, there were the original eight materials, and then there had been another set of eight [the BHVO-1 series].

[. . .] There [were] also two manganese nodules—the Atlantic and Pacific nodules, A1 and P1. And then <**T**: **20 min**> there were three more beyond the eight in the BHVO-1 set. There was the BIR, which was an Icelandic basalt, DNC. What was the third one? [It must have been W-2]. [. . .] In any case, there were by then eighteen or so standards that we were just giving away to anyone who asked for control material, a standard rock, or whatever.

[. . .] Frank Flanagan did the original six, the BCR, the GSP, the G2, that set of materials, and did present what he thought were best values for them, rather than just the tabulation of here's all the data, but he did not do the same thing for the others. And again, that left people with the option of going through the data themselves and deciding what they thought the reference value was. It defeated the whole concept of it being a standard with an agreed-upon value for individual constituents, and I think was the source of a great deal of very, very damaging confusion in geochemistry.

As I said, other institutions were also doing the same kind of thing, and there was enough consensus, even though less than I think we could have had and should have had, that it truly did facilitate all of the development of the new techniques through the sixties, seventies, eighties, even, you know, on into the ICP mass spec, which was more the nineties, because people at least had some reference, even if it wasn't an ideal reference. They had some way of saying, "Well, I'm totally off the mark," or "I'm doing reasonably well." And it then gave people who wanted to collaborate together and were looking at, "Okay, here's four labs who think we're all doing really first-class work, but our values differ by ten percent," to get together and compare the details of their methods and see if they could figure out what was causing the discrepancies within the analytical procedure, and refine their methods so that now you've got a small group of labs who are coming closer to getting the same value. Because again, that true value really is an unknown quantity that you are trying to establish, but when you're using five different techniques, all of which have different theoretical bases, all of which have theoretical potential interferences or problems or sources of error, in many ways, the only way you know you've succeeded is when you all get the same result, this harmony of results from different labs using different techniques, as we were talking about, proving that you've done a good job. . . . In many ways in analytical chemistry, the only proof that you got the right answer is that it can be replicated in other laboratories. And so all of this development of standards is about giving people a quick and easy way of knowing that they're getting the right value or that they're so far off that their method clearly doesn't work, and then giving them some basis for looking at where their errors <**T**: **25 min**> might be.

GROSS: Okay. So I have a few questions. One question is, given Flanagan's assertion that basically no one else could do the job as well as he did, or rather, that he was not going to necessarily be helpful in training someone to succeed him, how did you get chosen to take over the post or the project?

KANE: I basically was chosen because in the course of doing all of my method development and using the materials and talking to people endlessly about, "Well, how do I know which of these literature values I want to agree with," I had shown an interest in the thought process of improving the values that nobody else had shown. I also had a math background, so that detailed statistical data analysis didn't scare the living daylights out of me. [. . .] [Another factor was that, having had the SIMAAC project cancelled, everyone saw it as an appropriate alternative assignment for me.]

GROSS: Well, let's continue the thought process, then. So they approach you because you have the necessary skill set plus an interest in the subject. Flanagan, however, isn't going to train you, necessarily. How did you go about getting acculturated to the full extent of the program? Like how did you come into the . . .

KANE: Basically because . . . through all the time I was there . . .

[. . .] I was looking at the literature for this kind of discussion. *Geostandards Newsletter* had been in existence for a while, and I was reading all of [what was being written by the people] who were coming up with alternative methods [for deriving reference values]. I was looking at the discrepancies that they were arriving at in their take on what the reference value is. And the one example that I remember the best, because I cited it in numerous papers, niobium, is a trace element that is very, very important in some mineralization of rock studies, and niobium in BCR was reported by different people for the best value to be between fourteen and twenty [parts per million]. Well, that's a pretty big spread. [. . .]

Well, twenty [. . .] is one and one-half times fourteen, fundamentally. So that's a big uncertainty. And if other analysts are using BCR to calibrate their x-ray fluorescence determination of niobium in rocks, and some of them are calibrating at twenty and some are calibrating at fourteen, then that error is introduced into all the worldwide geoanalytical surveys of what are the concentrations . . . I mean, every country maps the rocks in their country in terms of what is our baseline in this geographic area, our baseline in that geographic area? Niobium tantalum ratio was one of the most important of the petrogenic ratios that people looked at, and I don't even remember in what context. But if you don't have a good niobium number and if you don't have a good tantalum number, or they can be different by 50 percent, depending on who did the determination, **<T**: **30 min>** then those ratios can vary all over the place, and so you're no longer able to get the petrogenic modeling that the geologist is trying to get out of the data. So you know, and I was interested in all of that, and I was reading about all of those uses, even while I was doing the bench determinations of some of these elements.

GROSS: So once you get appointed, I assume that your responsibilities would involve kind of negotiating this correspondence network, as people were sending in values.

KANE: Well, and there were also a couple of samples that had been prepared, and the one I worked on the most was—to the greatest degree of completion—before I left for NIST was the Devonian Ohio Shale, SDO, [. . .] that had actually been a sample that the IGC, International Geochemical Consortium or International Geochemical Conference [. . .] had worked on in the past, and no values had been finalized for it. But there was going to be an IGC meeting in DC about the time I took over the program, and it was going to be an opportunity to get people working on this again. And oil shale was a big thing in the late 1980s, early 1990s, in terms of how can it be used and what can be done with it. So the thought of the conference was to go back to this material, and [I] worked with a couple of the people in the Denver lab [who] had been instrumental in the earlier studies. And so we then sent that [sample] out to another group of people and collected a new batch of data.

GROSS: These were different labs all over the . . .

KANE: Different labs all over the country, probably all over the world. [. . .] But in any case, [I] sent it out, collected the data, and then based on my literature studies of what was being done other places and what the IAEA [International Atomic Energy Agency] was recommending, and what any number of other people, developed my own protocol for the statistical reduction of the data, and [. . .] derived reference values for publication..[. . .]

GROSS: This is through statistical analysis?

KANE: This is through statistical analysis [of all the submitted data]. And we were still doing a fair amount of data rejection, getting a preliminary average and standard deviation for all of the data, and then iteratively deleting anything that was more than two standard deviations away, and narrowing the database down. Fundamentally, what you know from the start, this widespread divergence of data has to be because some of the data is good and some of the data is an outlier. And when you get to NIST and when you get to the [International Organization for Standardization], the rule of thumb is we do *not* reject data primarily on statistical grounds. We *have* to have analytically justifiable reasons for saying the data is biased.

GROSS: What's an example of an analytically justifiable reason?

KANE: Let's say I'm doing beryllium in a basalt by [ICP-OES]. And the beryllium concentration [in a basalt is] going to be one or two ppm, and there will be a vanadium spectral overlap that will be half to three-quarters of a ppm, and so it's pretty doggoned big compared to the true value. Your bias is 75 percent of the true value. And in the early days of [ICP-OES], when all of the interferences hadn't been totally worked out, the original publication of beryllium in a basalt **<T: 35 min>** included the vanadium overlap as well as the beryllium value, so it was in error by 50 to 75 percent.

GROSS: Which was later ascertained and corrected?

KANE: Which was later ascertained and corrected. So that's one example. And another example would be that in a lot of geochemistry, people are much more interested in the mobile concentration than in the immobile, what's totally locked up and will never dissolve out, no matter how much rain falls, rivers flow over the rock. And so they will do a [determination of the amount that is] leachable. Well, if the leachable content is only 50 percent of the total, and that gets in the database from which you're trying to derive the true total concentration, it's a monumental error. So it's identifying things like that and knowing why they occur, and then either getting the correction or using an alternative method that isn't impacted at all by that error. And all of the ISO guidelines and all of the work at NIST and all of the work I've done with the IAG [International Association of Geoanalysts] after I left USGS [and NIST] was about getting a database that was clean in the first place, that did not need data rejection by statistical techniques.

GROSS: I see.

KANE: But that was not the reality in the early days of [developing] the geological reference materials, either at USGS or at any of the other places. With the possible exception [of the group within] CANMET [the Canadian Centre for Mineral and Energy Technology] [. . .] that was focused on the mining industry and on supporting the analytical data [quality] for the mining industry. [. . .] That was the one place in which there were very carefully defined standard methods and literally certification of analysts in terms of testing their competence ahead of time and qualifying them to even be in the lab. That kind of thing is totally absent from most of the baseline geochemical work.

GROSS: One question I have that's sort of related to this point is about the extent to which individual judgment comes into play when deciding on what results are incorporated into these analyses. It's probably less of an issue in the labs that are doing the actual hands-on analysis, right? Because they have their set of data and that's it.

KANE: [That's part of it, but they also have the literature data sets for any materials they are using for calibration and quality control purposes.]

GROSS: But when they send it all to you, especially as you're now the head of the program, you have to use a little bit of your own judgment, right, to determine . . .

KANE: You have to use judgment. You have to know something about the broad mineralogy of the sample. You have to know something about where spectral overlaps are likely to be a major problem. And again, so much of that is matrix specific. [. . .]

GROSS: So you needed to be aware not only of the distinction between different types of rocks or matrix compositions, but also the analytical errors that could arise?

KANE: That could arise. [. . .]

GROSS: Were you the ultimate arbiter on these questions, as the head of the program? Were you basically the one who was . . .

KANE: Basically, I'm going to each of the analysts and saying, "Okay, here's a problem I see in your data. Help me figure out why," because I really wanted to be able, if you're going to improve the reference sample database, what you've got to be able to do once you see that these discrepancies appear, is to figure out why. But I couldn't do that [without extensive input from the analysts] for all methods in all matrixes.

GROSS: Sure. [. . .] Now to what extent was this role that you played parallel or different from that that Flanagan had kind of established when he set things up?

KANE: Well, I think when Flanagan first did it, he was paying a fair amount of attention to that kind of thing, but I think over the years . . . [. . .]

[He] didn't do the follow-through, [after] getting the rock collected, getting it ground, getting it into the bottle, getting the preliminary sense that yes, the material in bottle one, and in bottle one hundred, and in bottle seven hundred in the run all produced the same result by this technique. That was in many regards his signature contribution, the bottle-to-bottle variability: have we documented that in fact it's absent? And again, he was doing that in the days when DC arc was the primary way of looking at trace elements. So DC arc is a semi-quantitative method, and the ability to see these differences gets better and better and better as the precision of the method improves. He wasn't able to see differences that I could see by the time I was working on the program.

GROSS: As a result of your previous experience working with these different types of instrumentation?

KANE: Right. [But also you know, because with] each of the new instruments, the precision got better, and so then you were able to see differences between methods that hadn't been apparent before, because again, if I want to use a standard analysis of variance [to decide if] these two numbers [are] different, the difference has to be pretty big compared to the within method **<T: 45 min>** standard deviation for fifteen or twenty repeats. [. . .] So [over time] the differences that could be judged statistically significant . . . got smaller, and smaller, and smaller, as methods and instrumentation improved, and analytical capability got better. [. . .]

[And as analytical capabilities changed, the ways in which analysts used existing reference materials also changed.] There were many techniques that are [preferentially] calibrated against a natural matrix material, but you want it to be representative of the matrix that people are going to be analyzing, and you want people to group their samples so that the major matrix errors that might occur in that kind of a process are eliminated. At the same time you're trying to do that, you really do want samples that are representative of the real world. So [if] you contaminate them in the grinding process, [which did occasionally happen,] they're no longer representative of the real-world matrix that you're trying to match. You're also trying to give people a way to validate a new method, and it gets complicated, to find a single material which will serve all of these purposes.

GROSS: Well, this is what I'm curious about, is ultimately the decision of when the sample is fixed comes down, I assume, to an individual's judgment, which is your judgment in this case, right?

KANE: [. . .] [Yes. The goal was] to get past the point that everyone is looking at a literature database of thirty values and saying, "I agree with this one; therefore this one is right, and I'm right," rather than having a more cohesive way of actually evaluating your data and demonstrating, because by the ISO definition, the reference value in non-certified materials or the certified value in a NIST material is by definition the true value for the concentration. And so if everybody's got a different idea of what the true value is, and they're using their **<T: 55 min>** own definition in setting up their calibration curve everything else they do thereafter is . . .

GROSS: Right, there's no way to reconcile these different values. [. . .] Hence the need for a central, ultimate decision.

KANE: A central, ultimate decision. But it took a long time to get there. I mean, there was a central decision on G1 and W1 for the major oxides. Only for the major oxides. Everything that came after that, until SDO, the numbers were in a whole lot more flux. There was a whole lot more opportunity because Flanagan put the compilation together and may have developed an average, but then Abbey in Canada would develop an average that was based [on a different protocol], on rejecting everything three standard deviations away from his preliminary average, and somebody else would develop another average that involved rejection of everything that was two [standard deviations from the initial value]. So there was no one decision.

GROSS: Until?

KANE: And I basically said, "Hey, USGS, if you guys are going to do these materials, we've got to follow the sense that even though legally we cannot call ourselves a certifying body, we've got to tell people, 'We believe this to be the true value. Use this value. Period. Flat. Final,' until we find an error in it and revise it and publish the revision," because, again, ISO defines a certifying body as a technically competent deliverer of reference values. And a certifying body like NIST is unquestionably . . . I mean, they've got some of the most sophisticated analytical capability and the highest emphasis on finding every potential error and developing truly accurate numbers. They do very, very well [almost always]. [. . .] [USGS was also technically competent and met different analytical needs than NIST did.]

[. . .] And the USGS [. . .] because NIST never made any rocks that were certified for the elements of interest to geochemistry, had no alternative, really, but to do these kinds of collaborative things, and it just took years. It's that slow evolution [. . .] of the process to make it the best that it possibly could be, and yet step by step along the way, because techniques got better, the numbers in the database, even if the reference value wasn't as carefully defined as I might like, the numbers got better. And all of geoanalysis and all of what the geologists could do in the way of geochemical modeling of what are the earth processes improved with each step along the way could never have happened without the development of the rock standards, whatever their limitations were.

GROSS: So you were in charge of the program for two years.

KANE: Two years, basically. [Two or two and a half]. [. . .]

GROSS: Which means that . . . you said that you went from the beginning of SDO to nearly the end of it during that time. Was that typical of a two-year . . .

KANE: I finished the SDO. The publication didn't come out until after I . . . you know, again, because of all that is involved.

GROSS: Right. There's got to be . . .

KANE: But I finished SDO before I went to NIST. I came very close to finishing both the coal [CLB] and the disseminated gold [DGPM]. The disseminated gold was actually a totally new material that two people from the Exploration Geochemistry Branch went out to collect at the **<T: 60 min>** Pinson Mine in Nevada. We were near Las Vegas, and I [. . .] spent a week in the field with them, doing preliminary field testing of different areas around the mine to see where we could get the best sample for the concentrations we wanted, and doing preliminary analysis of the rocks in a very, very . . . well, I mean, it was sophisticated for the time in the way of a field lab where you could go out and do a lot of the exploration geochemistry in the field rather than back in the lab.

GROSS: Was this your first time doing extended fieldwork?

KANE: It was the only time I did fieldwork. Everything else was, you know, in the lab, and we were . . . [grinding samples and we were ashing samples and we were, you know, digging, chipping with hammers, pulling rocks out of the ground. It was interesting].

GROSS: How did it compare? Did you like it?

KANE: [. . .] I do not think I would have wanted to do it permanently and full time. As part of, again, the learning process of how you determined the rock is worth collecting [and converting it to a standard in the first place] . . . it was very, very interesting. And I had a repeat experience [later]. I was at NIST still when one of the Geoanalysis Conferences [in 1994, Ambleside, United Kingdom] had a two-day workshop on the preparation of reference materials, where we went into the field and collected the material, and Phil Potts, [Mike Thompson] and I were doing the classroom part of the teaching of all the theory of reference [material collection], and the discussion of preparation techniques. But then we actually went into the field and chipped rocks and collected what became one of the Open University samples [and later the first IAG certified reference material, OU-6].

GROSS: Right. So [two years] of working at USGS on this program.

KANE: Right.

GROSS: During which time you basically took a couple of rocks from in some cases literally the field to . . .

KANE: A finished standard.

GROSS: . . . a finished. And that two-year time frame, that was pretty close to typical for, you know, beginning to end? Was that about right?

KANE: [. . .] It should be. It very often is not, either [at USGS or elsewhere, sometimes] because you want to determine [reference values for] elements that haven't been [done] before, and you can't find enough quality data in that time frame. [. . .]

GROSS: [. . .] I only have one more question **<T: 65 min>** before we get fully into NIST. And the question is your involvement with the newsletter or periodical *Geostandards and Geochemical Analysis*, which you mentioned you got involved with during this period of time.

KANE: I got involved with that, again, when Frank Flanagan retired, because he and Kuppusami Govindaraju over in France, who had—you know, his counterpart at CRPG, had been the original editors of the journal. [It was called *Geostandards Newsletter* through 2002, though the name changed twice since then. It is currently titled *Geostandards and Geoanalytical Research*.] And Govindaraju certainly wanted USGS to be [. . .] on the editorial board, and what have you. So as soon as I took over from Frank, I was invited to join the editorial board. And it was, you know, again, the first publication of [reference values for] our finished SDO material was in the journal.[13] [Later] a much more detailed USGS bulletin that provided far more detail, obviously, than you could . . .

GROSS: Than you provided in the journal.

KANE: . . . possibly provide in the journal. [. . .]

GROSS: Yeah. That report you have here, that is far too long for a journal article. [. . .] So this was your first time actually, I assume, as an editor of a journal.

[13] J.S. Kane, B. Arbogast, and J. Leventhal. "Characterization of Devonian Ohio Shale SDO-1 as a USGS geochemical reference sample." *Geostandards Newsletter* 14, no. 1 (1990): 169-196

KANE: Yes. [. . .]

[But] in many ways, Govindaraju did all of the final editing, but we [editorial board members] were the primary source of reviews for papers that were submitted. And at the time that he was doing it, before Phil Potts took over as editor, it was fundamentally where the databases of any reference material project would be published. It did not do a great deal with methods. It's also where all the literature in the early years on six different ways of evaluating the data would be published.

So the journal has changed greatly over the years in terms of what it does. But the very first [reference values for the BCR-1 series were] published in *Spectrochimica Acta*, [. . .] and very quickly [journals of that sort] started rejecting standards databases as not being *real* research.[14] And so *Geostandards Newsletter* was started as a way of [getting] this data out there, [so that] the standards [would have value] to people.

GROSS: So this was the main method of disseminating . . . ?

KANE: This was the main method of disseminating whatever was being done with regard to [data collected for the standards] . . . you know, the methods papers, to the degree that they were published, were, "I have applied ion selective electrode methodology to fluorine, chlorine, bromine, in ninety-seven reference materials." And then it would be the data. It'd be a very brief description of the method. . . . [The detailed methods papers would instead appear in *Applied Spectroscopy*, *Journal of Analytical Atomic Spectrometry*, or some other analytical chemistry/spectrometry journal]. [. . .]

GROSS: Okay. Once you joined the editorial staff, did the format change in any way? Or did you just kind of keep doing . . .

KANE: Well, the format didn't change until Govindaraju retired, which was four or five or six years after Frank Flanagan did. But then Phil Potts became the editor, and it started taking a very different shape. We at that point devised editorial guidelines that said every reference material article had to talk about the degree to which the work was in compliance with the [ISO] guidelines for reference material development, which had never been the case before.

GROSS: Right.

[14] Flanagan, 1967.

KANE: And again, we weren't trying to force people to adopt the guidelines entirely, but we were trying to nudge people gradually in that direction, and to at least begin to talk about, "Well, the guidelines say I should do X, but X is totally impossible in the practical world, and **<T: 70 min>** the closest I could come is Y," and, you know . . .

GROSS: Just get them thinking about those . . .

KANE: Just get them thinking about those issues, because at some point, and I don't know exactly when, between leaving USGS and now, but at some point, I described [being able to achieve everything] the ISO guidelines for reference material production [. . .] asked you to achieve as being the equivalent of getting a perfect 800 on an SAT test. It [isn't going to] happen very often. Most of the world will never be able to do this.

GROSS: But as long as they're thinking of that and aware of that . . . .

KANE: But as long as they're thinking of that and aware of that, they will do a better job than they would have done before. And it can be that sense of continuous quality improvement that [W.E.] Deming and a few others preached long and hard, and that, you know, the sense that every day can be a little better than the day before. I may never achieve perfection. I not only may never, I will never. But as long as I am striving . . .

GROSS: Right. Moving up towards . . .

KANE: . . . there will be something that gets better and is done a little more carefully and thoughtfully.

GROSS: Okay. So that was probably one of your major contributions along with the rest of the editorial board, to the way that *Geostandards* . . .

KANE: [*Geostandards Newsletter*] developed in the way the IAG [did], when it formed, I think the first meeting was actually 1997 . . .

GROSS: This is the . . . ?

KANE: This is the International Association of Geoanalysts [IAG, which now hosts the Geoanalysis series of conferences. I was still at USGS when I was invited as a speaker at Geoanalysis 1990].

[. . .] This was the first Geoanalysis meeting, which was organized jointly by Geological Survey of Canada and Geological Survey of [. . .] Ontario.

GROSS: Okay.

KANE: And I had met these folks at FACSS meetings. They had seen some of my early papers talking about reference values aren't quite what they could be in *Geostandards Newsletter*. They knew I had taken over from Flanagan. They wanted me at the meeting. [. . .] You know, in the sense of . . . in the geological community, I felt like all of my really serious mentoring and higher levels of support came initially from Canada, and then through that 1990 meeting in Canada, from the people in Great Britain who [later] formed the International Association of Geoanalysts. Those are the people I looked to as the support structure more than anyone . . .

GROSS: More than in the United States? [. . .]

KANE: More than [USGS]. [. . .] There was a lot of collegiality and sharing and learning from virtually everyone in the branch [there]. But in terms of this next step in my career, the support . . .

GROSS: You mean in terms of the . . .

KANE: . . . the encouragement, the **<T: 75 min>** people to talk to about what I was doing, to get ideas from . . .

GROSS: You're talking about the standard materials work?

KANE: I'm talking about the standard materials. That was [largely] the Brits and the Canadians.

GROSS: Okay. So this seems like a good segue point to talk about how you went from being at USGS, where the emphasis from what you just said was more on the chemistry side of things, to

NIST, which is explicitly a standards organization. So how did you get involved with NIST in the first place?

KANE: NIST—the reference materials program—called me and said, "Would you be interested?" And I went for an interview . . .

GROSS: And they were in . . . ?

KANE: And they were in Gaithersburg, Maryland, which was three times as far, but only five minutes further in terms of driving time, just because of seventy mile-an-hour highways instead of back-country roads. They offered me the position in the reference materials program, and it was very interesting. I don't remember exactly the titles. I mean, there was . . . Bill [William P.] Reed was SRMP chief.

GROSS: That's Standard Reference Materials Program?

KANE: [Yes.] And Tom Gills was under him, and he was the one who had more direct [supervision of] the project managers as opposed to the [total] program [which also included the sales staff]. But—and he was the one who invited me to come. And it was initially—well, it was in many ways a very, very wise move, because USGS by 1995 had a massive layoff that I probably would not have survived. And between the time I left and 1995 . . . the title of the *Washington Post* magazine section article shortly after was, "The Day the Axe Fell at USGS."[15] I mean, it was big.

GROSS: Was there a particular reason for the cuts?

KANE: Government funding; [. . .] the budget just got chopped hugely.

GROSS: But by then you were safely out of there. [. . .]

KANE: So you know, everything worked out beautifully, and I loved the job at NIST while Bill Reed was office chief. But once he got moved aside and Tom Gills took over, in spite of the fact that Tom was the one who brought me in and who seemed like he would have done

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[15] Walt Harrington, "Reduction in Force: When the Ax Fell at USGS," *Washington Post Magazine* 19 May 1996, pp. 13-32.

anything and everything to make it really work, **<T**: **80 min>** very quickly turned. I was accustomed to working quite independently. I was accustomed to saying, “I’ve got a problem. Do you have any ideas on how to solve it?” But not in going day to day, “How should I do my job?” Tom wanted that kind of control, and while Bill was still over Tom, I had someone running interference.

GROSS: But as soon as he left . . .

KANE: But as soon as he left, the interference was gone, and in spite of the fact that I loved the work, I hated the job, you know, if that dichotomy is possible. [. . .]

[It was in] 1991 that I went to NIST . . .

GROSS: 1991?

KANE: I believe it was 1991 that I went to NIST. [. . .] Sometime in late 1987 or early 1988 I took over from Frank, and it was March of 1991 that I went to NIST. And I stayed until 1 October 1995. The work was described to me by some people as, “Oh, gee, you’ve moved from chemistry to sales.” And I said, “I didn’t think that’s what [I’ve] done [. . .],” and I did not handle the job as though I had moved to sales. I handled the job as though I were the manager of a reference material project that was totally dependent for the chemical data on the analytical division, that was totally dependent for the data reduction on the statistical engineering division, but that still had me helping to design what the measurements ought to look like, looking at individual pieces of the data as they were coming in, and saying, “Yes, I think we’ve got a problem,” or, “No, I think we’re in really good shape,” and even doing some of my own independent . . . statistical engineering was going to do the final data reduction, but I could play with the numbers.

And I could still do all the kinds of thinking about . . . I had responsibility for all of the geological materials, and I knew that the geological community had nothing that was really a quality reference material, despite the fact that USGS was working on it, for the platinum group elements. I knew that we had nothing that was really first class for tantalum and niobium and for some of the other [so-called] high field strength elements that are, again, very important in petrogenic modeling. I knew we had nothing [ideal] for the rare earth elements, which were not only important in geochemical modeling, but were a way of identifying which power plant was having its ash drop out in the environment, [after being] carried in the air for a while and then [. . .] settling out. [From] the rare earth patterns for coal ash could tell people specifically which coal mine had produced the coal that was causing this fallout environmentally, [but] there were no rare earth certifications whatever by NIST. There were no high field strength, there were no platinum elements [that were optimally certified in rock samples].

GROSS: Neither by NIST or by USGS?

KANE: Well, USGS was certainly [working hard in this area; BCR-1 was probably the best available material in this regard]—**<T**: **85 min>** all of the other geological groups that did reference materials were [also] working on it. But there was not that totally definitive reference value.

GROSS: Which is what NIST was aiming for?

KANE: Which is what NIST was always aiming for and should have been able to produce, but again, because NIST was Department of Commerce, with very few exceptions, if the material did not have a commercial application or need, NIST was not going to do baseline geochemistry.

GROSS: Wouldn't those sorts of issues, for example, qualify?

KANE: Those sorts of pollution issues became very important as EPA became a partner with NIST on reference material development. And the very first coal and coal ash standards that go back . . . I used them in my very first weeks at USGS when I was doing coal ash analyses, so they data back to the early seventies, were done collaboratively by NIST and by EPA. And they did include the pollutant elements that EPA was wanting to monitor, but they did not include a lot of elements that the geological community would have been interested in. [Further, the rare earths are not viewed as toxic elements by EPA, and so certifications for them would not have been a priority].

GROSS: Things like the platinum group elements or what have you?

KANE: Right. So while I was at NIST, we did do two platinum palladium standards, and I was directly involved in those. INCO is a mine up there in Ontario somewhere, nickel primarily, but all nickel ore has precious metals in it. And somehow INCO was involved, perhaps in providing the platinum [and] palladium that went into automobile catalysts. Perhaps they were actually a collector of used catalysts to try to retrieve the platinum and palladium out of them, and probably a little of both. And again, that was a long time ago. But that was one of the first projects I worked on at NIST. I worked on several right off the bat, but . . .

GROSS: I mean, when you got invited, what was the specific responsibility they said you were going to have?

KANE: [. . .] I was the project manager of about ninety reference materials, some of which were already in stock, most of which were already in stock.

GROSS: And a few you had to pursue?

KANE: And a few that we were in process of making or even just at the beginning of going out and finding the material. In terms of what was already in stock, the primary responsibility was to determine how long the material in stock would last and whether we ought to be making the next generation, because we were going to run out in less than the time frame or in just barely over the time frame that it normally took NIST to finish a material, which was two to four years.

GROSS: Right.

KANE: So I had that responsibility, and then I had the responsibility of if a customer called and said, you know, "I'm having trouble matching your result. What do you think [. . .] might be wrong with what I'm doing?" Or, "Are you sure your number's right?" Then I would investigate that query from a customer.

GROSS: So this is where the sales aspect came in?

KANE: This is where the sales aspect—but to me, it wasn't even sales. If I'm telling them what's wrong with their analytical method as to why they can't match our value, or I'm researching whether we made a mistake in the methods we used to develop the [reference value], that to me was more science than it was sales.

GROSS: So the types of customers that you were dealing with, you're using the term customer, which I assume was what NIST referred to them as. Who were typical customers? Government labs? Scientific facilities?

KANE: The whole realm. Corning Glass would be a customer for some of the glass standards in terms of monitoring the control over their manufacturing processes.

GROSS: Wait, there were glass standards, too?

KANE: There **<T: 90 min>** were glass. [. . .] I had all of the rock standards. I had all of the environmental sediments and soils that were being used for pollution monitoring. I had all of the spectrometric solutions that were being used for the calibration of ICP [and] AA-type analyses. The anion solution standards that were being used for the calibration of ion selective electrodes. I had the coals and the coal ashes. I had the physical properties of glasses. And then with INCO, we developed an automotive catalyst standard, where you were analyzing the catalyst—the spent catalyst material for the platinum elements that were still in it. So, I mean, I had an enormous range of materials, and the customers might be in Europe. They might be in the United States. They might be in Canada. They were government labs. They were EPA labs. They were university labs. [. . .] They were anything and everything [in the way of labs].

GROSS: But they could be in industry, they could be in the government?

KANE: They could be anywhere. [. . .] Anywhere all over the globe. [. . .]

GROSS: Interesting. This was a very different sort of interaction. I know, as you said, normally you like to kind of ensconce yourself in your lab and kind of work on problems.

KANE: Right.

GROSS: This must have been a very strange transition, to be . . .

KANE: It was very different. [Yet similar . . . the problems were thought problems rather than lab ones, and yet that's not really so different.] Again, as long as I was working with a fairly small group of chemists on an individual project and brainstorming the chemistry—what should we add to the certification that we're not thinking about—it was perfectly comfortable. It was not a whole lot different [from when] I was at USGS, going to a technical conference and presenting a paper and dealing with questions from the audience. The difficulty came in when I would challenge their science.

GROSS: Their . . . being the customers?

KANE: Their being [the NIST analysts].

GROSS: Oh, their being NIST.

KANE: Okay? As I said, most of the chemical analyses in the projects I worked on were done in house, but occasionally they would invite an outside laboratory, and there were some things where quite frankly in terms of the matrix that was involved . . . we did fluoride and vegetation . . . because the fallout from an Alcoa plant on pasture land surrounding the plant was doing dental damage to the cattle that were grazing there. You know, the excess fluoride browns the teeth and causes all kinds of weird spotting, and this, that, and the other thing. And so suddenly there was an environmental concern about we need fluoride in vegetation, but the fluoride was pretty much wrapped up in the aluminum, rather than in the vegetable matter.

GROSS: Oh, wow.

KANE: And the industrial people who also did these analyses in partnership with NIST got very different results from the [NIST] neutron activation group. And I had the audacity to defend the outside labs.

GROSS: That sort of behavior was not normally tolerated, I assume?

KANE: Not normally tolerated. NIST was God . . .

GROSS: According to NIST?

KANE: According to NIST. And in many, many ways, they were. But they had more time and they had more money to spend on a project than literally anybody else in the world in terms of this kind of analysis, or at least [. . .] of any of the industrial labs or geological labs that might collaborate with them. And then they were going to sell the material, where USGS was giving their materials away, so they were retrieving part of what they spent on **<T**: **95 min>** doing this work. And they just . . . I mean, they had every advantage. They should have been able to do better than anyone else.

GROSS: How did they respond when you raised these questions?

KANE: Oh, they did not like it. But in the end, I believe on that one . . . I mean, again, I spent days in the library looking for information on the methods these other people were using and what kinds of things could have gone wrong, and if you've got three outside methods that agree, and one NIST one that doesn't, it's not proof that NIST is wrong, but it leads to a strong suspicion. And then when I found in the literature a neutron activation report that talked about problems with and interferences in this particular determination, I said, "Hey, guys, you show me that this didn't happen in your analysis, or I'm going with the other numbers." And again, Bill Reed backed me up, and we went with the other numbers.

GROSS: Now did you have at this point . . . were you basically working on your own again? I know that you were a manager.

KANE: Well, the SRM office, each of the project managers, and there were five of us, one with the ASTM [American Society for Testing and Materials] metals project, and, you know, one with the health and clinical type materials, and then I had mine, and I don't remember what the other was, we had our own little cubicle, and for the most part, it was a desk job in the way of tracking what was happening, going to the library and doing the literature studies that would inform the next project, meeting one on one with the [analysts and then with the] statistical engineering people as they were working on data reduction. But it was kind of a . . . yes, I could be by myself in my own little cubbyhole and look . . . you know, the phone would ring, and I'd have to answer a question for a customer, and usually that was . . . again, it was a puzzle to solve. I could take my time in the library and call them back two or three days later, if need be, so that it was a new intellectual challenge. It was something I thoroughly enjoyed doing.

GROSS: What types of questions . . . do you recall the most challenging set of materials, or the ones that kind of provoked the most questions?

KANE: Not really. I mean, having to deal with that kind of thing actually turned out to be a fairly small part of the job.

GROSS: What was the major part of the job?

KANE: The major part of the job was keeping track of all of these materials that I was responsible for writing articles for *American Lab* and *Environmental Lab* magazines, which really, you know, are not scientific journals as such, but to advertise the newest material, or that promoted here it is, come get it, so that you're promoting sales, but you're still writing about the scientific merit of using the material. And then as I said, just fundamentally . . . I mean, I worked on three reference materials during their process of certification at [USGS]. I probably worked on thirty or forty at [NIST] in a space of time that wasn't much longer. So that keeping track of all of it and monitoring what was happening, and are we on track or are we running behind, and drafting the certificate of analysis, and drafting the publication information, and even helping to update the catalogue, the sales catalogue, and things of that sort. [. . .]

GROSS: The materials come with the certificate, then?

KANE: The materials come with the certificate from NIST. And they do now from USGS and from IAG and from most other places. But at the time that I first took over from Flanagan, we [at USGS] were not producing certificates at all . . .

GROSS: At USGS?

KANE: . . . at USGS. And the main reason we were not was strictly political, because NIST at the time of its founding was NBS [National Board of Standards] rather than NIST, but at the time of its founding was chartered as *the only* standards producer for the US government.

GROSS: Hence it was the only one that could issue certificates?

KANE: Right. Eventually, USGS management changed its mind and decided that because NIST did not certify rocks for any of the elements that were of importance to geologists, and because EPA was certifying their materials by then, perhaps we could ignore the fear that we would get raked over the coals [for issuing certificates]. Again, it was an evolutionary process to get that mind change. So USGS now does issue certificates, [. . .] and they're probably on the website, but when I was there, when Frank Flanagan was working on it, that was a major, major impediment to doing the job the way it should have been done, just because we were told, "No, you may not."

GROSS: So along with the choice of samples, then, that was a major distinction between the [NIST] SRMs and USGS samples initially?

KANE: [. . .] Right.

GROSS: The fact that . . .

KANE: They were called the USGS "standard rocks." In quotes.

GROSS: But you weren't allowed to call them *official* standards . . .

KANE: But you weren't allowed to call them official standards.

GROSS: . . . until later?

KANE: And now I think they are calling . . . ISO distinguishes between reference material and certified reference material, and the basic distinction is that a certified reference material must be accompanied by a certificate [. . .] and has an uncertainty been attached to that [reference] value.

GROSS: Certified value.

KANE: Certified or reference value. And USGS initially did not do uncertainties at all, and the way I am now handling them with the IAG is totally different than what I did with SDO. Again, it's been an evolution. The whole concept of traceability has been much more evident over the last ten or fifteen years than it had been before. But my fundamental argument on all of this is that for accreditation purposes, every laboratory [. . .] has to have quality control materials, has to show that they are getting correct values on them. Well, on half the things that people might want to measure, a NIST certified reference material doesn't exist.

They're very, very big on the things that are regulated: food, drug, nutrient standards, EPA pollutants, toxicity, that kind of thing. But for so much of basic, routine, everyday, the ordinary sample as opposed to the one of either environmental contamination interest or very, very critical industrial need, because producing that standard is so time consuming, so difficult, requires such a level of accuracy . . . I mean, the basic guideline is it should be ten times more accurate and precise than the routine analytical data needs to be in terms of the reference value. Well, how in the world do you get there, especially if, as people introduce a new method which lets you measure something that could never be measured before, you're supposed to already have the reference material to prove how well you're doing it? I mean, the whole argument is so circular.

GROSS: So how do you break the cycle?

KANE: You acknowledge that, you know, the cart can't come before the horse, and that if I want an analytical result that is ten times better than the routine data will ever be, it can't be ready the day the first routine data is developed. [. . .]

GROSS: I mean, were they trying to pressure that sort of time frame onto some of these questions at NIST?

KANE: I don't think NIST was [specifically] trying. [But] I think if you take some of the ISO standards totally seriously, and you take some of the laboratory accreditation requirements totally seriously, that's where you end up.

GROSS: You mean, with that sort of mindset?

KANE: With that sort of a totally impossible situation to try and deal with.

GROSS: I guess your conclusion is that the only way to avoid that sort of circumstance would be to reconsider the certification guidelines that are being put into place?

KANE: Reconsider the certification guidelines, reconsider the definition of how an accredited laboratory will have proved [its capabilities] . . . do far more [documentation of processes in the major metrology institutions worldwide]. [ISO, NIST, and IRMM all do] *some* publication—you know, these are the methods we used in certification. But there are very, very few. I alluded to the fact that it—at CANMET, where they're working primarily with ores and, you know, with pricing an ore material, or pricing a concentrate, or, you know, buying and selling commercially important things, they've got a different set of accreditation guidelines and whatever. And I think so much of what happens needs **<T: 110 min>** to recognize that there are different circumstances. One size doesn't fit all in terms of what the regulations and the guidelines and the processes are. And then also realize that if the guideline is truly the impossible goal to be reached—as I think most of them are—that falling short isn't really a problem, that it still gives you something to strive for, and you can still stepwise do better and better.

If you look for perfect compliance, the people writing the guidelines would have to do two things that they've never to my satisfaction done. They would have to provide worked examples of how they've gotten there on some of their own materials. They would also have to do vastly more publication of their refined methods and their evaluation of where analytical error comes from. I mean, they're fundamentally asking anymore that every analytical result be able to show a traceability chain for accuracy back to a real metrological standard at every step of the analysis. [. . .]

GROSS: So this is more of the stuff that was happening after NIST, correct?

KANE: The whole thought process began [at or even before] NIST and [continued in my work with the IAG. I kept probing.] How does anybody, even a NIST or an IRMM [Institute for Reference Materials and Measurements], actually, on a practical level, achieve these standards that they write and describe? I still think they're a touch esoteric, theoretical, and that the average guy who is supposed to be learning something about them and applying some of what they're talking about to every analysis he does and reports and to demonstrate the quality of it, really is in a totally impossible situation. And I wish they would get it a little more back down to earth.

And that's what the IAG protocol tried to do for rock analysis. We now consider the IAG to be a liaison member of the International Organization for [Standardization], ISO. The French turns the letters around in terms of . . . and to have developed a protocol that is the closest that a non-in-house reference material certification can come.[16] Because again, they talk about demonstrating the quality of every laboratory that is contributing to a round-robin database, and ASTM, when they're working with NIST, would have done this by virtue of the fact that they developed standard methods, and in documenting the standard **<T: 115 min>** method, they get twenty or thirty labs to use it and to report data, and so now the spread in the data becomes the inter-lab precision of the method, of the reproducibility, repeatabilities within a lab in a single day or in a short period of time, reproducibilities between laboratories using the same method or between laboratories using different methods for the same analysis. But everybody's supposed to do these things, and when the ASTM labs are contributing to a NIST standard, because they've been [involved in] the documentation of the accuracy of a standard method, you've had a way of qualifying them, of saying they are good enough. But when USGS, for instance, just sent their samples all over the world to anyone who said, "I'm willing to do it," and the data spread might be a factor of two on trace elements, you haven't had any kind of control over the accuracy, and you don't know how many outliers compared to legitimate values you've got in that big wide data set.

GROSS: Right. You need to control not only the ultimate standard, but the way that the data . . .

KANE: The way that the data is collected.

GROSS: . . . for the standard is collected at *every* step of the way.

[16] J.S. Kane, P.J. Potts, M. Wiedenbeck, J. Carignan, and S.A. Wilson, "International Association of Geoanalysts' Protocol for the certification of geological and environmental reference materials," *Geostandards Newsletter: The Journal of Geostandards and Geoanalysis*. 27 (2003): 227-244.

KANE: At every step along the way. And so the IAG, when it was first formed, set up a proficiency testing program for rock labs, and used the results of that proficiency testing program to identify which labs were likely to produce outlier data and which labs were right smack in the center of the database. And so then we used that for the qualification procedure, because geochemistry, there are no standard methods. Again, too many of the people who would be interested in analyzing the sample are university researchers, and if you want to publish something, it better be unique, and so you don't want to be doing anything that involves following a standard method. You want to be developing a new method. You want to be doing something unique and different. And those labs are the major contributors to the database. Forcing them to participate in the proficiency test is a way of qualifying them to then contribute to the certification of a similar or even the same material. In some cases it's the same material that's done in two rounds, once to pick out the qualified labs, and then send the same sample back to them. Other cases, you'll have two very similar materials, and one will become the standard after you've qualified [labs] using the other.

GROSS: We've sort of segued during the course of this discussion from talking about NIST, I think, into your post-NIST . . .

KANE: Into my post-NIST work. And it's difficult not to, because they're so closely related.

GROSS: So before we go full post-NIST, I guess, I should ask a little bit about at NIST whether or not you had the opportunity to publish or present your work at conferences.

KANE: A great deal. It was fundamentally expected. Tom Gills preferred to be the one to tell me I was going to present X at Conference Y. Enough people in the geological community would invite me to other kinds of conferences, and I would always get permission, but it would always be with a little bit of . . . even if you write the paper on your own time at home or on your two weeks of annual leave, I would like you to be working for me any time you're working, and this is not directly for me. And, you know, it was . . .

GROSS: So this was about those control issues, then?

KANE: Some of these control issues that were really, you know, terribly unfortunate, because one of the requirements in **<T**: **120 min>** all of government service for the high-level grade that I had achieved at that time was that you be known as an expert around the [world]—or at least beyond your own agency. If everything had to come through him, and not named specific to me, [. . .] then how do I meet that fundamental qualification for grade level? But he never saw it that way.

GROSS: So did the tension between you two ultimately contribute to your decision to leave?

KANE: Yes. Absolutely. I mean, I left the very day that I was eligible to retire. And again, it was the control issues, and it was the fact that you couldn't talk about them, because if we had been able to talk about them, [we might have been able to resolve them]. [. . .]

GROSS: Why weren't you able to talk about them?

KANE: Well, just, you know, a brick wall. [. . .] Between the two of us. I mean, it even got to the point [. . .] somewhere along the way that a couple of folks in the Analytical Laboratories Division, which did all the analytical work for us, said, "Wouldn't you be happier coming over here and getting out of SRMP?"

GROSS: And you said?

KANE: And I said, "You know, I would feel awkward doing that." I was specifically sought out for SRMP in the first place. In the second place, you know, I didn't say to the person who raised the issue, but I knew in my own mind that this NIST superiority, that most of the truly successful people at NIST had never worked anywhere else, and I had only three years at NIST in sales, [at least from their perspective,] so that I had the [. . .] sense that [. . .] even if I wasn't happy where I was, and by then I knew there were problems, that it was going to be worse doing something else.

And what was . . . with all these control issues, what was incredibly interesting to me, Arati Prabhakar became the director of NIST sometime during my tenure there [in 1993], and she didn't stay very long. But this was, other than Lori Filipek at USGS, when she became BGC branch chief, and Gwendy Hall in Canada, who was the head of the analytical laboratories at Canadian Geological Survey, [. . .] these were the only women in really high positions that I had ever known and met.

GROSS: I was going to ask a little about that.

KANE: And while Arati [Prabhakar] was there [at NIST], I do not remember at this point whether she was the speaker, or whether it was someone she invited in, but the discussion was on mentoring, and [. . .] on the fact that some supervisors are poison mentors as opposed to helpful mentors. And I thought it was very interesting that the very, very senior management at

NIST could be talking about this, but all the way down the line, the poison mentoring was very, very real.

GROSS: I mean, did you ever have the opportunity to mentor anyone during the course of either your USGS career or NIST?

KANE: USGS, when I took over the Flanagan project, and they formally **<T**: **125 min>** appointed a woman in Denver [Belinda Arbogast] as the quality manager for the project, and we had met at—we had met a couple of times. But she specifically asked me if I would function as her mentor, which, you know, as I left a year or so later, was very difficult to do anything with. But we stayed in touch, and she ended up in 1997, which is after I'd left, but she ended up in 1997 as program chairman for the Geoanalysis meeting that—I'm sorry, as general chairman for the Geoanalysis meeting—and I was program chairman, in retirement. At the first meeting in 1990, they [the Canadian organizers] asked me, as Flanagan's replacement, if I would consider serving as chairman for the next conference, and they were thinking in three-year terms, and USGS hosting it, obviously, in that situation. And I just did not feel, having been told I couldn't have travel money for that [2000] meeting [in Canada], [. . .] that I was in a position to say, "Yes, I'll guarantee you that [USGS] will do it." It was, "I'll go back and talk to people about it." And we [USGS] ended up not doing it [organizing the meeting] until 1997. There was an intermediate conference [2004] in England while I was at NIST, where I mentioned that we had [. . .] field opportunity for reference material sample collection, and that I helped teach the two-day training on certification process. But Belinda and I worked very, very closely in putting that 1997 conference together, and . . .

GROSS: [. . .] Okay. I guess, this gets to your decision to leave NIST in a roundabout way.

KANE: In a roundabout way.

GROSS: And this, as you said, kind of came down to the tensions you were having, both with the institutional culture at NIST being . . . well, very superior.

KANE: Very superior to any . . . I mean, they just had a hard time believing that anybody else could do quality work. They could eventually be convinced, but—and you could—we did use USGS for some of the certification work on the materials I worked on. We did use a few outside laboratories on the automotive catalyst platinum group certifications. Well, we did a collaborative project with the Geological Survey of Poland, and they came to NIST for training, as the Egyptians had come to USGS while I was there, and they worked on our soil standards, and I think they were actually developing some soil standards of their own that we helped them set up. And we definitely put leach data on the certification. Again, the fundamental premise is that if you have two totally different methods of analysis that give you the same result, you've got accuracy demonstrated.

Well, obviously, if you have a single [method] like an EPA leach method, you can't have two independent methods of analysis. So there, again, you're back to a collaboration between many laboratories. We did that on the sludge with the New Jersey EPA. We did that on the soils with the Polish group, where USGS and a couple of EPA labs and the Poles—I don't know who else. But all of those, you know, kinds of things were involved. And I suppose there's some degree of mentoring in **<T**: **130 min>** that training and interlaboratory collaboration, but . . .

GROSS: But beyond those few examples, generally, you would characterize as kind of NIST-focused?

KANE: NIST-focused. At least the part . . . you know, as I [said], there were definitely pieces that were much more ASTM focused and [NIST was] able to bring other people in, but not the work I specifically did.

GROSS: So between that and kind of the tensions with your supervisor, you ultimately decide, when you have the opportunity in 1995, to retire. What happens after that? I see here that you've marked down on the professional history you provided that you remained involved with *Geostandards and Geoanalytical Research*.

KANE: Right. I stayed on the editorial board. I still write for them. And as I said, the [IAG] was kind of an outgrowth of the Geoanalysis meetings. The Brits hosted the 1994 conference, and they had a fair amount of money left . . . having set it up as an educational organization rather than a commercial and all of that, they had to find some way to funnel that money into something that would remain educational, and so they formed the International Association of Geoanalysts, and [it hosts] a conference every three years. We're working on the seventh or the eighth? We're working on the eighth. The seventh was two years ago in South Africa, and that's the one where I had basically gotten to the point, I said, "Look, folks, I love this work, but sooner or later we have to find somebody younger and somebody who is still connected with laboratory science, because I no longer am remotely current with laboratory techniques, with where geochemistry is going, and trying to stay current at home by myself just doesn't work." So I had been involved with them right from the beginning, as I said. I mean, immediately after I retired, I was working on putting the program together for the 1997 conference. And after the 1997 conference, they formed the proficiency testing program, and I was on the steering committee of that for [several] years. And then simultaneously, they were trying to form a certification committee and actually have the IAG become recognized by ISO as a certifying body. And I chaired the certification committee from its inception until 2006, 2007. I've lost track of when I handed it over. But in the meantime . . .

GROSS: Did ISO accept it?

KANE: Yes. Well, we became a liaison member 2005, 2004. And have been a [liaison] member ever since. Now we're not big enough to be a full-fledged member rather than a connected, affiliated organization.

GROSS: Right.

KANE: But we are still very much involved, and after we did a couple of our reference materials at the Beijing [China] conference, Geoanalysis conference, in 2006, I had prepared the paper, I had initially planned to go, but after having a heart attack in February of 2006, I said, "You know, if I have another one, and it hasn't been long enough since the last one to know whether this is an ongoing problem or not, there is no way in the world I want to be in Beijing with problems."

So I prepared the talk, but I asked Phil Potts, with whom right from the 1990 meeting I have had the closest personal relationship and professional both—you know, if there were in this whole geostandards **<T: 135 min>** area a mentor, Phil would be it, hands down, no questions asked. He was on the editorial board. He had worked with Govindaraju on some of the earliest materials that the International Working Group, which was an offshoot of CRPG, had prepared, and he and I literally right from day one, leaving the Geoanalysis 1990 meeting, started voluminous correspondence on how we could do better with standards, and how we could move things along.

And so when he became editor-in-chief, you know, again, I was going to be the lead reviewer on everything that had anything to do with standards, and I was going to help get the proficiency testing program off the ground, and I was going to work with him on setting up a [. . .] certification committee within IAG. And then I literally did all of the data reduction personally on our first three materials. Designed the protocol, told people what the experimental plan was going to be. Was first author on the published IAG protocol for how you do a certification project.[17] Step by step along the way [. . .] I drafted it, Phil critiqued it. It was that partnership that got [IAG certification effort] off the ground and got the projects going and set it all in motion.

We have now finished [certifying three materials]. Two of the three were done in conjunction with the Central Geological Laboratory [of Mongolia]. They had done the materials themselves, 2000 or thereabouts. And they wanted to learn our protocol and our process and potentially improve the quality of the certified values in their project and add elements that they

[17] Kane *et al.*, 2003.

hadn’t been able to do before. And they also had some of the most challenging matrices I ever want to work on in terms of all kinds of analytical difficulties, either some elements being totally out of range of the normal geological sample or being in a different mineral form than they appear in most rocks. We had some analytical challenges with those that we hadn’t had with the others. It was not as successful as either they or I would have liked, except as a training exercise for other people. And that training exercise involved Thomas [C.] Meisel at University of Leoben [Montanuniversität Leoben] [. . .] in Austria, who became certification committee chairman probably right before we did that training meeting. But we also involved some people from Germany who had been consulting with the Mongolian laboratories on all of their standards work, and so the training really addressed all of these people, who were hopefully going to take over and follow along behind me and fill in the gaps.

Unfortunately, job pressures for people who were working full time, when this is related but not really the work of their own institution, is incredibly difficult to find. We had thought we would have two new standards finished shortly after a **<T**: **140 min>** Goldschmidt [Geochemistry] Conference in 2009, where I [spent a week at the University of Tennessee] with them going through all of the data they’d collected, all of the data reduction they’d started on. And there were still issues, and they wanted to tweak it and finalize it and make it a little better. Well, I haven’t seen a draft certificate yet, [. . .] [and I just don’t quite know why.]

GROSS: And you’re still right now in the role of overseeing kind of the certification program, or have you stepped back from . . .

KANE: I’ve stepped back. I mean, Thomas as chairman, having taken over from me, 2006, 2007, somewhere in that time frame, is supposed to be the lead person on all of this.

GROSS: Thomas?

KANE: [Thomas C. Meisel at Montanuniversität Leoben].

GROSS: The Austrian university?

KANE: The Austrian university. I mean, the intent was really . . . hey, folks, you know, fifteen years out of the lab and away from geological literature, and all of that kind of thing, I just can’t.

GROSS: Now this is just preparing an institutional transition?

KANE: Transition. And the transition, again, because the [IAG,] International Association of Geoanalysts . . . it's a professional society like ACS [. . .] but [in comparison] it's a very small group with [. . .] members who are scattered all over the world. I mean, we've got people in Mongolia. We've got people in China. We've got people in Brazil. We've got Canada, the U—Denver, Washington. [. . .] It's impossible to maintain a really tight working relationship and collegiality. Email is wonderful, and it makes things a whole lot simpler than it used to be, but it really doesn't fill the gap.

GROSS: So that actually is a wonderful kind of transition towards the last set of questions I have written down here. [. . .]

[recording paused]

GROSS: [. . .] The last set of questions that I have are about kind of changes in geochemistry and geoanalysis. Since you entered the field in the 1970s, and also more broadly, kind of what it's like for women in science since your time, and since your original entry, rather in the 1950s and 1960s. I guess there are a couple of different ways we can approach this. One would be to talk about something you've touched on already, which are kind of major questions or major changes in techniques that have been . . .

KANE: Well, there have been tremendous changes in the analytical techniques available, which means that totally different geological processes can be studied and explored and looked at than could have been before. I think the biggest difference is that we have gone from bulk analyses to microanalyses in terms of being able to sample laser ablation ICP mass spec, to take a solid crystal and look at changes in the chemistry spot by spot across the crystal, so that before . . . you knew bulk chemistry, but you didn't know the variation within a rock in quite the same way. The heterogeneity of a material, looking at zoning in a crystal where you've got some [minerals] forming, you know, almost like a coconut shell rim, and then moving inward and the changing composition. So they can do an enormous amount of that that they couldn't do before. The other biggest change is that with ICP mass spec, again, they are able to do all kinds of **<T**: **145 min>** isotopic studies that could not have been done before.

GROSS: You didn't talk too much about the mass spec side of this story.

KANE: No. I didn't use mass spec, myself. Shortly after the [ICP-OES, optical emission spectrometry] came in you ended up with [ICP-MS], where you were coupling the plasma with the mass spectrometer instead of with an optical emission spectrometer. And that opens up, again, all kinds of opportunities for . . . one, it gives you an independent method, which you didn't have before. But it also gives you the opportunity to look at the isotopic variations within this . . . and the isotopes are not the things I've got anything but a passing acquaintance with. But there is an isotopic fractionation that occurs in the earth with some specific elements. And then there is also the fact that some of them are radioactive elements, and so they will decay. And so the decay time, being able to monitor the decay lets you put an age, a [date on when the rock first formed].

[. . .] So in terms of if you are really trying to see is this a recent event or is this an ancient event and how did the rock change over time, as for instance, a cooling magma will fractionate because each of the individual crystal forms that might come out of that initial melt has a different solidification temperature, and so you can see whether it cooled rapidly. You can see whether it cooled very, very [slowly], gradually. And you can . . .

GROSS: So that has less of an issue to do, for example, with a standard material, because you'd want to . . .

KANE: Well, except that you want an isotopic standard now that you never needed before.

[. . .] You now need to be able to have an isotopic standard, and IAEA is the one who has done most of the stable isotopes in all kinds of materials, rather than the geological surveys. But NIST has been involved as well. And, I mean, the whole isotopic story is a huge one. It's the one I am least familiar with. It's also the one that is newest in terms of the ready accessibility [of measurement techniques that can be used routinely].

I mean, we've had mass spectrometry for a very long time, but thermal ionization mass spec, for instance, which is always used for the isotope-dilution type analytical work, is exceedingly tedious. It is very much single-element [technique]. I mean, you have to get your one isotope totally separated from everything else. And so with ICP mass spec, all of those issues go away. So again, it's been a progression from single element, major oxide concentrations [. . .] down to a tenth of a percent concentrations only, to being able to measure at the part per billion level, and to be able to look at spatial variation within a single crystal or a rock and being able to look at isotopic changes as well as chemical composition.

GROSS: And also looking at multiple elements, as you were saying.

KANE: And looking at . . . [but we were always looking at multiple elements, just not simultaneously]. Again, the gross efficiency of going from single element to [simultaneous] multielement.

GROSS: Right. And then bulk to micro.

KANE: And bulk to micro.

GROSS: And include this new time . . .

KANE: . . . and including the isotopic composition as well as just the strict chemical.

GROSS: Those are the big trends?

KANE: Those are the big trends. And those have all developed really in the time that I was working through this.

GROSS: Now **<T: 150 min>** did you notice a change in the way that the geosciences in general engaged with public debates about science, or in the public sphere, during the time that you were involved?

KANE: Modestly, but not hugely. [. . .]

Again, I mean, I've been away from it fifteen years, so what has happened since I left is a growing awareness of the fact that we had to be paying attention to these problems, and frankly educating people, because one of the talks we did at Geoanalysis 1997 was about the lead pollution in Colorado, and the fact that most of the lead that is the source of difficulty in Colorado is a naturally occurring ore vein of lead. And that, you know, we've got to distinguish between the lead that is there in the ground and that we really can't do anything about [and] the lead that is an environmental problem [caused anthropogenically] as opposed to a natural environmental occurrence.

And you have to also understand that just because it's there and [is toxic metal in some] form doesn't mean it's [actually occurring in a problematic form]. And what that brings to mind is the chromium (VI)/chromium (III) story. Chromium (VI) is highly toxic. Everyone worries about chromium (VI) pollution. But in the soil, the redox potential of natural soil is such that chromium (VI) will disappear overnight. It will become chromium (III), and I don't need to remove it if it's in its nontoxic form. Yeah, over time, there's been a growing awareness of and interest in standards developed in relation to these kinds of issues, but I don't think it bleeds into public policy in a way that affects it **<T: 155 min>** sensibly. [. . .]

I wish we as scientists were better at communicating this stuff. I wish the public were better able to receive the communication. And the general illiteracy on most of this stuff is amazing.

GROSS: All right. Another question, then, sort of, related to that. It's about encouraging greater scientific literacy and [. . .] more broadly, scientific education, especially among women and other underrepresented groups. Have you noticed a change over the course of your career in efforts to reach out to these different groups to get them more involved in geochemistry or geoanalysis?

KANE: I've probably been too far away from where that kind of interaction would take place with young people, but my concern as I look at things like the standards of learning and No Child Left Behind, is this sense that we can demand that everyone be at the same place at the same time, and that the schools are failing if they're not.[18] And it's so much more complicated than that. I don't want anyone to fail, and I don't want to define success such that a thousand children, all at the end of their academic careers, be at exactly the same place in every single subject. And I'm afraid we've done a lot of that, and we've also put pressure on teachers at the early levels to focus so exclusively on math and reading that the playing with magnets, the playing with magnification glasses, and learning nature, and all of that kind of thing which should be, if we want a scientifically literate public, very much a part of young children's lives, gets crowded out of the schools.

GROSS: Do you have any advice for women who are thinking about studying chemistry or entering the sciences more broadly?

KANE: Go for it. It's fascinating. The doors are far more widely open than they once were. Society is much more comfortable with the idea of women working outside of the home, even when they have young children. The child-care opportunities are much broader. But also just plain be flexible. Not everyone is going to want the kind of very, very intense career that a person like Mary Good had, and there's no reason everyone should be striving for that.[19] And I think the opportunity for the kind of flexibility that I've experienced in my career, or my lifetime, because it goes well beyond career, that I'm still experiencing it, is that the world is much more open to people being themselves and to not fitting into a mold. And so much of what made things work for me were the totally accidental, utterly serendipitous things that happened along the way of life that, you know, you can start out with a plan, and you have an idea where you want to go, but **<T: 160 min>** let the plan bend and flex as opportunities come up. Just grab opportunities. Don't be afraid to take a chance.

GROSS: All right. Those are the questions that I had prepared, pretty much all of them. [. . .]

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[18] Pub.L. 107-110, 115 Stat. 1425, enacted January 8, 2002.
[19] See Mary L. Good interview, Chemical Heritage Foundation, Oral History Transcript # 0171.

But at this point, before we wrap up, I normally like to pause and ask if there's anything else that you'd like to add. If there's any question that if you were in my position you would have asked yourself, or if there's just anything else you'd like to have on the record.

KANE: All right. I think the one comment I would add that I kind of glossed over as we were talking about meetings and [. . .] mentors, and then basically the feedback that says, "This is great." I've been fairly critical of how esoteric so many of the ISO standards are, and I alluded to the fact that we tried with the IAG protocol to make things much more practical and truly doable for the ordinary person [. . .] or organization [. . .] The paper that [Phil presented for me] at Geoanalysis 2006 in [China], once it was published, generated the feedback [a member of] ISO REMCO, that they really needed to pay more attention to the practical stuff that I was talking about, and that they were glad somebody had put some of it on paper.[20] And I think that was—you know, one of the most gratifying things, period, in all that I had done through this.

And the other [was], of course, [. . .] Geoanalysis 2009 that I had mentioned was in South Africa, Phil Potts organized a session that was a tribute to what I had contributed to the IAG, and that was also again, enormously gratifying. I gave the opening invited lecture that was the summary of everything we [the IAG Certification Committee] had accomplished to date, but then other people who had worked with me in one way or another over the years in the IAG spoke about some part of the reference material work we'd done jointly, and that was a fantastic capstone [to my career].[21]

GROSS: Well, I think on that happy note that, unless you have anything else to add, I think we're going to wrap up. So thank you so much for taking the time [. . .] to share all of these fascinating stories about your career in chemistry and in geochemistry, and in retirement, too.

[**KANE**: Well, thank you also.]

[END OF AUDIO, FILE 2.2]

[END OF INTERVIEW]

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[20] J.S. Kane, and P.J. Potts, "ISO best practices in reference material certification and use in Geoanalysis," *Geostandards and Geoanalytical Research* 31 (2007): 361-378.
[21] J.S. Kane, "Experience of the International Association of Geoanalysts as a Certifying Body," *Geostandards and Geoanalytical Research* 34 (2010): 215-230.

AFTERWORD

As I read the transcript of my Chemical Heritage Foundation interview a year later in order to edit and correct, I feel compelled to reiterate in summary fashion what I see to be the basic concepts that guided my career.[22] First, I was simultaneously a serious student learning from my teachers and an independent learner. You can and should be able to learn without always requiring training to reach your objective. Second, I was driven to maximize my potential without feeling discouraged that others like Madame Curie had far greater potential. Third, I did not see retirement as the end of professional opportunities rather than as a game-changer in how those opportunities would play out.

Remember that I retired in the fall of 1995. The most recent issue of *Geostandards and Geochemical Research* (v.36, No. 4) arrived as I was completing my review of the transcript. It contained an article that referenced five of my publications, four of them co-authored, and all dated 2003 or later. So for me, retirement was more a new beginning than an ending, and the beginning of extremely rewarding work at that.

[22] On 1 February 2018, as a result of the merger with the Life Sciences Foundation, the Chemical Heritage Foundation changed our name to the Science History Institute.

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